SCIENCE IN ACTION

How to follow scientists and engineers through society

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Not being a native English speaker I had to rely heavily on my friends to revise successive drafts of this manuscript. John Law and Penelope Dulling have been most patient in revising the earlier drafts. Steven Shapin, Harry Collins, Don MacKenzie, Ron Westrum and Leigh Star suffered each on one different chapter. I have been most fortunate in having Geoffrey Bowker edit the whole book, ‘debug’ it and suggest many useful changes.

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Scene 1: On a cold and sunny morning in October 1985, John Whittaker entered his office in the molecular biology building of the Institut Pasteur in Paris and switched on his Eclipse MV/8000 computer. A few seconds after loading the special programs he had written, a three-dimensional picture of the DNA double helix flashed onto the screen. John, a visiting computer scientist, had been invited by the Institute to write programs that could produce three-dimensional images of the coils of DNA and relate them to the thousands of new nucleic acid sequences pouring out every year into the journals and data banks. ‘Nice picture, eh?’ said his boss, Pierre, who was just entering the office. ‘Yes, good machine too,’ answered John.

Scene 2: In 1951 in the Cavendish laboratory at Cambridge, England, the X-ray pictures of crystallised deoxyribonucleic acid were not ‘nice pictures’ on a computer screen. The two young researchers, Jim Watson and Francis Crick, had a hard time obtaining them from Maurice Wilkins and Rosalind Franklin in London. It was impossible yet to decide if the form of the acid was a triple or a double helix, if the phosphate bonds were at the inside or at the outside of the molecule, or indeed if it was an helix at all. It did not matter much to their boss, Sir Francis Bragg, since the two were not supposed to be working on DNA anyway, but it mattered a lot to them, especially since Linus Pauling, the famous chemist, was said to be about to uncover the structure of DNA in a few months.

Scene 3: In 1980 in a Data General building on Route 495 in Westborough, Massachusetts, Tom West and his team were still trying to debug a makeshift prototype of a new machine nicknamed Eagle that the company had not planned to build at first, but that was beginning to rouse the marketing department’s interest. However, the debugging program was a year behind schedule. Besides, the choice West had made of using the new PAL chips kept delaying the machine—renamed Eclipse MV/8000, since no one was sure at the time if the company manufacturing the chips could deliver them on demand. In the meantime, their main competitor, DEC, was selling many copies of its VAX 11/780, increasing the gap between the two companies.
1. Looking for a way in

Where can we start a study of science and technology? The choice of a way in crucially depends on good timing. In 1985, in Paris, John Whittaker obtains 'nice pictures' of DNA on a 'good machine'. In 1951 in Cambridge Watson and Crick are struggling to define a shape for DNA that is compatible with the pictures they glimpsed in Wilkins's office. In 1980, in the basement of a building, another team of researchers is fighting to make a new computer work and to catch up with DEC. What is the meaning of these 'flashbacks', to use the cinema term? They carry us back through space and time.

When we use this travel machine, DNA ceases to have a shape so well established that computer programs can be written to display it on a screen. As to the computers, they don't exist at all. Hundreds of nucleic acid sequences are not pouring in every year. Not a single one is known and even the notion of a sequence is doubtful since it is still unsure, for many people at the time, whether DNA plays any significant role in passing genetic material from one generation to the next. Twice already, Watson and Crick had prudently announced that they had solved the riddle and both times their model had been reduced to ashes. As to the 'good machine' Eagle, the flashback takes us back to a moment when it cannot run any program at all. Instead of a routine piece of equipment John Whittaker can switch on, it is a disorderly array of cables and ship surveyed by two other computers and surrounded by dozens of engineers trying to make it work reliably for more than a few seconds. No one in the team knows yet if this project is not going to turn out to be another complete failure like the EGO computer on which they worked for years and which was killed, they say, by the management.

In Whittaker's research project many things are unsettled. He does not know how long he is going to stay, if his fellowship will be renewed, if any program of his own can handle millions of base pairs and compare them in a way that is biologically significant. But there are at least two elements that raise no problems for him: the double helix shape of DNA and his Data General computer. What was for Watson and Crick the problematic focus of a fierce challenge, that won them a Nobel Prize, is now the basic dogma of his program, embedded in thousand of lines of his listing. As for the machine that made West's team work day and night for years, it is now no more problematic than a piece of furniture as it hums quietly away in his office. To be sure, the maintenance man of Data General stops by every week to fix some minor problems; but neither the man nor John have to overhaul the computer all over again and force the company to develop a new line of products. Whittaker is equally well aware of the many problems pleasing the Basic Dogma of biology - Crick, now an old gentleman, gave a lecture at the Institute on this a few weeks ago - but neither John nor his boss have to rethink entirely the shape of the double helix or to establish a new dogma.

The word black box is used by cyberneticians whenever a piece of machinery or a set of commands is too complex. In its place they draw a little box about which they need to know nothing but its input and output. As far as John Whittaker is concerned the double helix and the machine are two black boxes. That is, no matter how controversial their history, how complex their inner workings, how large the commercial or academic networks that hold them in place, only their input and output count. When you switch on the Eclipse it runs the programs you load; when you compare nucleic acid sequences you start from the double helix shape.

The flashback from October 1985 in Paris to Autumn 1951 in Cambridge or December 1980 in Westborough, Massachusetts, presents two completely different pictures of each of these two objects, a scientific fact - the double-helix and a technical artefact - the Eagle minicomputer. In the first picture John Whittaker uses two black boxes because they are unproblematic and certain; during the flashback the boxes get reopened and a bright coloured light illuminates them. In the first picture, there is no longer any need to decide where to put the phosphate backbone of the double helix, it is just there at the outside; there is no longer any squabble to decide if the Eclipse should be a 32-bit fully compatible machine, as you just hook it up to the other NOVA computers. During the flashbacks, a lot of people are introduced back into the picture, many of them staking their career on the decisions they take: Rosalind Franklin decides to reject the model-building approach Jim and Francis have chosen and to concentrate instead on basic X-ray crystallography in order to obtain better photographs; West decides to make a 32-bit compatible machine even though this means building a tinkered 'kludge', as they contemptuously say, and losing some of his best engineers, who want to design a neat new one.

In the Pasteur Institute John Whittaker is taking no big risk in believing the three-dimensional shape of the double helix or in running his program on the Eclipse. These are now routine choices. The risks he and his boss take lie elsewhere, in this gigantic program of comparing all the base pairs generated by molecular biologists all over the world. But if we go back to Cambridge, thirty years ago, who should we believe? Rosalind Franklin who says it might be a three-strand helix? Bragg who orders Watson and Crick to give up this hopeless work entirely and get back to serious business? Pauling, the best chemist in the world, who unveils a structure that breaks all the known laws of chemistry? The same uncertainty arises in the Westborough of a few years ago. Should West obey his boss, de Castro, when he is explicitly asked not to do a new research project there, since all the company research has now moved to North Carolina? How long should West pretend he is not working on a new computer? Should he believe the marketing experts when they say that all their customers want a fully compatible machine (on which they can reuse their old software) instead of doing as his competitor DEC does a 'culturally compatible' one (on which they cannot reuse their software but only the most basic commands)? What confidence should he have in his old team burned out by the failure of the EGO project? Should he risk using the new PAL chips instead of the older but safer ones?
Uncertainty, people at work, decisions, competition, controversies are what one gets when making a flashback from certain, cold, unproblematic black boxes to their recent past. If you take two pictures, one of the black boxes and the other of the open controversies, they are utterly different. They are as different as the two sides, one lively, the other severe, of a two-faced Janus. ‘Science in the making’ on the right side, ‘all made science’ or ‘ready made science’ on the other; such is Janus bifrons, the first character that greets us at the beginning of our journey.

In John’s office, the two black boxes cannot and should not be reopened. As to the two controversial pieces of work going on in the Cavendish and in Westborough, they are laid open for us by the scientists at work. The impossible task of opening the black box is made feasible (if not easy) by moving in time and space until one finds the controversial topic on which scientists and engineers are busy at work. This is the first decision we have to make: our entry into science and technology will be through the back door of science in the making, not through the more grandiose entrance of ready made science.

Now that the way in has been decided upon, with what sort of prior knowledge should one be equipped before entering science and technology? In John Whittaker’s office the double helix model and the computer are clearly distinct from the rest of his worries. They do not interfere with his psychological mood, the financial problems of the Institute, the big grants for which his boss has applied, or with the political struggle they are all engaged in to create in France a big data bank for molecular biologists. They are just sitting there in the background, their scientific or technical contents neatly distinct from the mess that John is immersed in. If he wishes to know something about the DNA structure or about the Eclipse, John opens Molecular Biology of the Gene or the User’s Manual, books that he can take off the shelf. However, if we go back to Westborough or to Cambridge this clean distinction between a context and a content disappears.

Scene 4: Tom West sneaks into the basement of a building where a friend lets him in at night to look at a VAX computer. West starts pulling out the printed circuits boards and analyses his competitor. Even his first analysis merges technical and quick economic calculations with the strategic decisions already taken. After a few hours, he is reassured.

‘I’d been living in fear of VAX for a year,’ West said afterward. (…) ‘I think I got a high when I looked at it and saw how complex and expensive it was. It made me feel good about some of the decisions we’ve made’.

Then his evaluation becomes still more complex, including social, stylistic and organisational features:

Looking into the VAX, West had imagined he saw a diagram of DEC’s corporate organization. He felt that VAX was too complicated. He did not like, for instance, the system by which various parts of the machine communicated with each other, for his taste, there was too much protocol involved. He decided that VAX embodied flaws in DEC’s corporate organization. The machine expressed that phenomenally successful company’s cautious, bureaucratic style. Was this true? West said it did not matter, it was a useful theory. Then he rephrased his opinions. ‘With VAX, DEC was trying to minimize the risk’, he said, as he swerved around another car. Grinning, he went on: ‘We’re trying to maximize the win, and make Eagle go as fast as a raped ape.’

(Kidder: 1981, p. 36)

This heterogeneous evaluation of his competitor is not a marginal moment in the story; it is the crucial episode when West decides that in spite of a two-year delay, the opposition of the North Carolina group, the failure of the EGO project, they can still make the Eagle work. ‘Organisation’, ‘taste’, ‘protocol’, ‘bureaucracy’, ‘minimisation of risks’, are not common technical words to describe a chip. This is true, however, only once the chip is a black box sold to consumers. When it is submitted to a competitor’s trial, like the one West does, all these bizarre words become part and parcel of the technical evaluation. Context and contents merge.

Scene 5: Jim Watson and Francis Crick get a copy of the paper unveiling the structure of DNA written by Linus Pauling and brought to them by his son:

Peter’s face betrayed something important as he entered the door, and my stomach sank in apprehension at learning that all was lost. Seeing that neither Francis nor I could bear any further suspense, he quickly told us that the model was a three-chain helix with the sugar phosphate backbone in the center. This sounded so suspiciously like our aborted effort of last year that immediately I wondered whether we might already have had the credit and glory of a great discovery if Bragg had not held us back.

(Watson: 1968, p. 102)

Was it Bragg who made them miss a major discovery, or was it Linus who missed a good opportunity for keeping his mouth shut? Francis and Jim hurriedly try out the paper and look to see if the sugar phosphate backbone is solid enough to hold the structure together. To their amazement, the three chains described by Pauling had
no hydrogen atoms to tie the three strands together. Without them, if they knew their chemistry, the structure would immediately fly apart.

Yet somehow Linus, unquestionably the world’s most astute chemist, had come to the opposite conclusion. When Francis was amazed equally by Pauling’s unorthodox chemistry, I began to breathe slower. By then I knew we were still in the game. Neither of us, however, had the slightest clue to the steps that had led Linus to this blunder. If a student had made a similar mistake, he would have thought unwise to benefit from Cal Tech’s chemistry faculty. Thus, we could not but initially worry whether Linus’s model followed from a revolutionary reevaluation of the acid-based properties of very large molecules. The tone of the manuscript, however, argued against any such advance in chemical theory.

(idem: p. 103)

To decide whether they are still in the game Watson and Crick have to evaluate simultaneously Linus Pauling’s reputation, common chemistry, the tone of the paper, the level of Cal Tech’s students; they have to decide if a revolution is under way, in which case they have been beaten off, or if an enormous blunder has been committed, in which case they have to rush still faster because Pauling will not be long in picking it up:

When his mistake became known, Linus would not stop until he had captured the right structure. Now our immediate hope was that his chemical colleagues would be more than ever awed by his intellect and not probe the details of his model. But since the manuscript had already been dispatched to the Proceedings of the National Academy, by mid-March at the latest Linus’s paper would be spread around the world. Then it would be only a matter of days before the error would be discovered. We had anywhere up to six weeks before Linus again was in full-time pursuit of DNA.

(idem: p. 104)

'Suspense', 'game', 'tone', 'delay of publication', 'awe', 'six weeks delay' are not common words for describing a molecule structure. This is the case at least once the structure is known and learned by every student. However, as long as the structure is submitted to a competitor’s probing, these queer words are part and parcel of the very chemical structure under investigation. Here again context and content fuse together.

The equipment necessary to travel through science and technology is at once light and multiple. Multiple because it means mixing hydrogen bonds with deadlines, the probing of one another’s authority with money, debugging and bureaucratic style; but the equipment is also light because it means simply leaving aside all the prejudices about what distinguishes the context in which knowledge is embedded and this knowledge itself. At the entrance of Dante’s Inferno is written:

ABANDON HOPE ALL YE WHO ENTER HERE.

At the onset of this voyage should be written:

ABANDON KNOWLEDGE ABOUT KNOWLEDGE ALL YE WHO ENTER HERE.

Learning to use the double helix and Eagle in 1985 to write programs reveals none of the bizarre mixture they are composed of; studying these in 1952 or in 1980 reveals it all. On the two black boxes sitting in Whittaker’s office it is inscribed, as on Pandora’s box: DANGER: DO NOT OPEN. From the two tasks at hand in the Cavendish and in Data General Headquarters, passions, deadlines, decisions escape in all directions from a box that lies open. Pandora, the mythical android sent by Zeus to Prometheus, is the second character after Janus to greet us at the beginning of our trip. (We might need more than one blessing from more than one of the antique gods if we want to reach our destination safely.)

(2) When enough is never enough

Science has two faces: one that knows, the other that does not know yet. We will choose the more ignorant. Insiders, and outsiders as well, have lots of ideas about the ingredients necessary for science in the making. We will have as few ideas as possible on what constitutes science. But how are we going to account for the closing of the boxes, because they do, after all, close up? The shape of the double helix is settled in John’s office in 1985; so is that of the Eclipse MV/8000 computer. How did they move from the Cavendish in 1952 or from Westborough, Massachusetts, to Paris in 1985? It is all very well to choose controversies as a way in, but we need to follow also the closure of these controversies. Here we have to get used to a strange acoustic phenomenon. The two faces of Janus talk at once and they say entirely different things that we should not confuse.

Janus’ first dictum:

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Just get the facts straight!
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Get rid of all the useless facts

Figure 1.2
Scene 6: Jim copies from various textbooks the forms of the base pairs that make up DNA, and plays with them trying to see if a symmetry can be seen when pairing them. To his amazement adenine coupled with adenine, cytosine with cytosine, guanine with guanine and thymine with thymine make very nice superimposable forms. To be sure this symmetry renders the sugar phosphate backbone strangely misshapen but this is not enough to stop Jim's pulse racing or to stop him writing a triumphant letter to his boss.

I no sooner got to the office and began explaining my scheme than the American crystallographer Jerry Donohue protested that the idea would not work. The tautomeric forms I had copied out of Davidson's book were, in Jerry's opinion, incorrectly assigned. My immediate retort that several other texts also pictured guanine and thymine in the enol form cut no ice with Jerry. Happily he let out that for years organic chemists had been arbitrarily favoring particular tautomeric forms over their alternatives on only the flimsiest of grounds. ( . . . ) Though my immediate reaction was to hope that Jerry was being hot air, I did not dismiss his criticism. Next to Linus himself, Jerry knew more about hydrogen bonds than anyone in the world. Since for many years he had worked at Cal Tech on the crystal structures of small organic molecules, I couldn’t kid myself that he did not grasp our problem. During the six months that he occupied a desk in our office, I had never heard him shooting off his mouth on subjects about which he knew nothing. Thoroughly worried, I went back to my desk hoping that some gimmick might emerge to salvage the like-with-like idea.

(Watson: 1968, pp. 121–2)

Jim had got the facts straight out of textbooks which, unanimously, provided him with a nice black box: the enol form. In this case, however, this is the very fact that should be dismissed or put into question. Or at least this is what Donohue says. But whom should Jim believe? The unanimous opinion of organic chemists or this chemist's opinion? Jim, who tries to salvage his model, switches from one rule of method, 'get the facts straight', to other more strategic ones, 'look for a weak point', 'choose who to believe'. Donohue studied with Pauling, he worked on small molecules, in six months he never said absurd things. Discipline, affiliation, curriculum vitae, psychological appraisal are mixed together by Jim to reach a decision. Better sacrifice them and the nice like-with-like model, than Donohue's criticism. The fact, no matter how 'straight', has to be dismissed.

The unforeseen dividend of having Jerry share an office with Francis, Peter, and me, though obvious to all, was not spoken about. If he had not been with us in Cambridge, I might still have been pummeling out for a like-with-like structure. Maurice, in a lab devoid of structural chemists, did not have anyone to tell him that all the textbook pictures were wrong. But for Jerry, only Pauling would have been likely to make the right choice and stick by its consequences.

(Idem: p. 132)

The advice of Janus' left side is easy to follow when things are settled, but not as long as things remain unsettled. What is on the left side, universal well-known facts of chemistry, becomes, from the right side point of view, scarce pronouncements uttered by two people in the whole world. They have a quality that crucially depends on localisation, on chance, on appraising simultaneously the worth of the people and of what they say.

Janus's second dictum:

Just get the most efficient machine

Decide on what efficiency should be

Figure 1.3

Scene 7: West and his main collaborator, Alsing, are discussing how to tackle the debugging program:

'I want to build a simulator, Tgm.'

'I'll take too long, Alsing. The machine will be debugged before you get your simulator debugged.'

This time, Alsing insisted. They could not build Eagle in anything like a year if they had to debug all the microcode on prototypes. If they went that way, moreover, they'd need to have at least one and probably two extra prototypes right from the start, and that would mean a doubling of the boring, grueling work of updating boards. Alsing wanted a program that would behave like a perfected Eagle, so that they could debug their microcode separately from the hardware.

West said: 'Go ahead. But I betcha it'll all be over by the time you get it done.'

(Kidder: 1981, p. 146)

The right side's advice is strictly followed by the two men since they want to build the best possible computer. This however does not prevent a new controversy starting between the two men on how to mimic in advance an efficient machine. If Alsing cannot convince one of his team members, Peck, to finish in six weeks the simulator that should have taken a year and a half, then West will be right: the simulator is not an efficient way to proceed because it will come too late. But if Alsing and Peck succeed, then it is West's definition of efficiency which will turn out to be wrong. Efficiency will be the consequence of who succeeds; it does not help deciding, on the spot, who is right and wrong. The right side's advice is all very well once Eagle is sent to manufacturing; before that, it is the left side's confusing strategic advice that should be followed.
machine has been conceived by West, through many compromises, to keep all these people happy and busy. He cannot be sure it is going to hold them together. Each of the interest groups has to try their own different sort of tests on the machine and see how it withstands them. The worst, for Tom West, is that the company manufacturing the new PAL chips is going bankrupt, that the team is suffering a post partum depression, and that the machine is not yet debugged. 'Our credibility, I think, is running out,' West tells his assistants. Eagle still does not run more than a few seconds without flashing error messages on the screen. Every time they painstakingly pinpoint the bug, they fix it and then try a new and more difficult debugging program.

Eagle was failing its Multiprogramming Reliability Test mysteriously. It was blowing away, crashing, going out to never-never land, and falling off the end of the world after every four hours or so of smooth running.

'Machines somewhere in the agony of the last few bugs are very vulnerable,' says Alsing. 'The shouting starts about it. It'll never work, and so on. Managers and support groups start saying this. Hangers-on say, 'Gee, I thought you'd get it done a lot sooner.' That's when people start talking about redesigning the whole thing.'

Alsing added, 'Watch out for Tom now.'

West sat in his office. 'I'm thinking of throwing the kids out of the lab and going in there with Rasala and fix it. It's true. I don't understand all the details of that sucker, but I will, and I'll get it to work.'

'Gimme a few more days,' said Rasala.

(idem: p. 231)

A few weeks later, after Eagle has successfully run a computer game called Adventure, the whole team felt they had reached one approximate end: 'It's a computer,' Rasala said (idem: p. 233). On Monday 8 October, a maintenance crew comes to wheel down the hall what was quickly becoming a black box. Why has it become such? Because it is a good machine, says the left side of our Janus friend. But it was not a good machine before it worked. Thus while it is being made it cannot convince anyone because of its good working order. It is only after endless little bugs have been taken out, each bug being revealed by a new trial imposed by a new interested group, that the machine will eventually and progressively be made to work. All the reasons for why it will work once it is finished do not help the engineers while they are making it.

Scene 9: How does the double helix story end? In a series of trials imposed on the new model by each of the successive people Jim Watson and Francis Crick have worked with (or against). Jim is playing with cardboard models of the base pairs, now in the kites form suggested by Jerry Donohue. To his amazement he realises that the shape drawn by pairing adenine with thymine and guanine with cytosine are superimposable. The steps of the double helix have the same shape. Contrary to his earlier model, the structure might be complementary instead of being like-with-like. He hesitates a while, because he sees no reason at first for this complementarity. Then he remembers what was called 'Chargaff laws', one of these many empirical facts they had kept in the background. These 'laws' stated that there

As the summer came on, increasing numbers of intruders were being led into the lab — diagnostic programmers and, particularly, those programmers from Software. Some Hardy Boys had grown fond of the prototypes of Eagle, as you might of a pet or a plant you've raised from a seedling. Now Rasala was telling them that they couldn't work on their machines at certain hours, because Software needed to use them. There was an explanation: the project was at a precarious stage; if Software didn't get to know and like the hardware and did not speak enthusiastically about it, the project might be ruined; the Hardy Boys were lucky that Software wanted to use the prototypes—and they had to keep Software happy.

(idem: p. 201)

Not only the Software people have to be kept happy, but also the manufacturing people, those from marketing, those who write the technical documentation, the designers who have to place the whole machine in a nice looking box (not a black one this time!), not mentioning the stockholders and the customers. Although the
Janus's fourth dictum:

When things are true they hold

When things hold they start becoming true

was always as much adenine as thymine and as much guanine as cytosine, no matter which DNA one chose to analyse. This isolated fact, devoid of any meaning in his earlier like-with-like model, suddenly brings a new strength to his emerging new model. Not only are the pairs superimposable, but Chargaff laws can be made a consequence of his model. Another feature came to strengthen the model: it suggests a way for a gene to split into two parts and then for each strand to create an exact complementary copy of itself. One helix could give birth to two identical helices. Thus biological meaning could support the model.

Still Jim's cardboard model could be destroyed in spite of these three advantages. Maybe Donohue will turn it to ashes as he did the attempt a few days earlier. So Jim called him to check if he had any objection. When he said no, my morale skyrocketed (Watson: 1968, p. 124). Then it is Francis who rushes into the lab and ‘pushes the bases together in a number of ways’. The model, this time, resists Francis's scepticism. There are now many decisive elements tied together with and by the new structure.

Still, all the convinced people are in the same office and although they think they are right, they could still be deluding themselves. What will Bragg and all the other crystallographers say? What objections will Maurice Wilkins and Rosalind Franklin, the only ones with X-rays pictures of the DNA, have? Will they see the model as the only form able to give, by projection, the shape visible on Rosalind's photographs? They'd like to know fast but dread the danger of the final showdown with people who, several times already, have ruined their efforts. Besides, another ally is missing to set up the trial, a humble ally for sure but necessary all the same: That night, however, we could not firmly establish the double helix. Until the metal bases were on hand, any model building would be too sloppy to be convincing (idem: p. 127). Even with Chargaff laws, with biological significance, with Donohue's approval, with their excitement, with the base pairing all on their side, the helix is still sloppy. Metal is necessary to reinforce the structure long enough to withstand the trials that the competitors/collaborators are going to impose on it.

The remainder of the double helix story looks like the final rounds of a presidential nomination. Every one of the other contenders is introduced into the office where the model is now set up, fights with it for a while before being quickly overwhelmed and then pledging complete support to it. Bragg is convinced although still worried that no one more serious than Jim and Francis had checked the helix. Now for the big game, the encounter between the model and those who for years had captured its projected image. Maurice needed but a minute's look at the model to like it. 'He was back in London only two days before he rang up to say that both he and Rosy found that their X-ray data strongly supported the double helix' (p. 131). Soon Pauling rallies himself to the structure, then it is the turn of the referees of Nature.

'Of course,' says the left side of Janus, 'everyone is convinced because Jim and Francis stumbled on the right structure. The DNA shape itself is enough to rally everyone.' No, says the right side, every time someone else is convinced it progressively becomes a more right structure. Enough is never enough: years later in India and New Zealand other researchers were working on a so called 'warped zipper' model that did everything the double helix does—but a bit more; Pauling strongly supported his own structure that had turned out to be entirely wrong; Jim found biological significance in a like-with-like structure that survived only a few hours; Rosalind Franklin had been stubbornly convinced earlier that it was a three-strand helix; Wilkins ignored the keto forms revealed by Jerry Donohue; Chargaff's laws were an insignificant fact they kept in the background for a long time; as to the metal atom toys, they have lent strong support to countless models that turned out to be wrong. All these allies appear strong once the structure is blackboxed. As long as it is not, Jim and Francis are still struggling to recruit them, modifying the DNA structure until everyone is satisfied. When they are through, they will follow the advice of Janus's right side. As long as they are still searching for the right DNA shape, they would be better off following the right side's confusing advices.

We could review all the opinions offered to explain why an open controversy closes, but we will always stumble on a new controversy dealing with how and why it closed. We will have to learn to live with two contradictory voices talking at once, one about science in the making, the other about ready made science. The latter produces sentences like 'just do this... just do that...', the former says 'enough it never enough'. The left side considers that facts and machines are well determined enough. The right side considers that facts and machines in the making are always under-determined. Some little thing is always missing to close the black box once and for all. Until the last minute Eagle can fail if West is not careful enough to keep the Software people interested, to maintain the pressure on the debugging crew, to advertise the machine to the marketing department. (3) The first rule of method

We will enter facts and machines while they are in the making; we will carry with us no preconceptions of what constitutes knowledge; we will watch the closure of
The DNA molecule has the shape of a double helix.

"The DNA molecule has the shape of a double helix."

Why don't you guys do something serious?

May be it is a triple helix.

It is not a helix at all.

If it had the shape of a double helix.

This would explain Chargaff's results, and it would be pretty.

They say that Watson and Crick have shown that DNA is a double helix.

"Watson and Crick have shown that the DNA molecule has the shape of a double helix."

Since the molecule of DNA has the shape of a double helix, the replication of genes is made understandable.

The black boxes and be careful to distinguish between two contradictory explanations of this closure, one uttered when it is finished, the other while it is being attempted. This will constitute our first rule of method and will make our voyage possible.

To sketch the general shape of this book, it is best to picture the following comic strip: we start with a textbook sentence which is devoid of any trace of fabrication, construction or ownership; we then put it in quotation marks, surround it with a bubble, place it in the mouth of someone who speaks; then we add to this speaking character, another character. To whom it is speaking; then we place all of them in a specific situation, somewhere in time and space, surrounded by equipment, machines, colleagues; then when the controversy heats up a bit we look at where the disputing people go and what sort of new elements they fetch, recruit or seduce in order to convince their colleagues; then, we see how the people being convinced stop discussing with one another; situations, localisations, even people start being slowly erased; on the last picture we see a new sentence, without any quotation marks, written in a text book similar to the one we started with in the first picture. This is the general movement of what we will study over and over again in the course of this book, penetrating science from the outside, following controversies and accompanying scientists up to the end, being slowly led out of science in the making.

In spite of the rich, confusing, ambiguous and fascinating picture that is thus revealed, surprisingly few people have penetrated from the outside the inner workings of science and technology, and then got out of it to explain to the outsider how it all works. For sure, many young people have entered science, but they have become scientists and engineers; what they have done is visible in the machines we use, the textbooks we learn, the pills we take, the landscape we look at, the blinking satellites in the night sky above our head. How they did it, we don't know. Some scientists talk about science, its ways and means, but few of them accept the discipline of becoming also an outsider; what they say about their trade is hard to double check in the absence of independent scrutiny. Other people talk about science, its solacity, its foundation, its development or its dangers; unfortunately, almost none of them is interested in science in the making. They shy away from the disorderly mixture revealed by science in action and prefer the orderly pattern of scientific method and rationality. Defending science and reason against pseudo-sciences, against fraud, against irrationality, keeps most of these people too busy to study it. As to the millions, or billions, of outsiders, they know about science and technology through popularisation only. The facts and the artefacts they produce fall on their head like an external fate as foreign, as inhuman, as unpredictable as the olden Fate of the Romans.

Apart from those who make science, who study it, who defend it or who submit to it, there exist, fortunately, a few people either trained as scientists or not, who open the black boxes so that outsiders may have a glimpse at it. They go by many different names (historians of science and technology, economists, sociologists, science teachers, science policy analysts, journalists, philosophers, concerned —}
simply wish to summarise their method and to sketch the ground that, sometimes unwittingly, they all have in common. In doing so I wish to help overcome two of the limitations of 'science, technology and society' studies that appear to me to thwart their impact, that is their organisation by discipline and by object.

Economists of innovation ignore sociologists of technology; cognitive scientists never use social studies of science; ethnoscience is far remote from pedagogy; historians of science pay little attention to literary studies or to rhetoric; sociologists of science often see no relation between their academic work and the in vivo experiences performed by concerned scientists or citizens; journalists rarely quote scholarly work on social studies of science; and so on.

This Babel of disciplines would not matter much if it was not worsened by another division made according to the objects each of them study. There exist historians of eighteenth-century chemistry or of German turn-of-the-century physics; even citizens' associations are specialised, some in fighting atomic energy, others in struggling against drug companies, still others against new maths teaching; some cognitive scientists study young children in experimental settings while others are interested in adult daily reasoning; even among sociologists of science, some focus on micro-studies of science while others tackle large-scale engineering projects; historians of technology are often aligned along the technical specialities of the engineers, some studying aircraft industries while others prefer telecommunications or the development of steam engines; as to the anthropologists studying 'savage' reasoning, very few get to deal with modern knowledge. This scattering of disciplines and objects would not be a problem if it was the hallmark of a necessary and fecund specialisation, growing from a core of common problems and methods. This is however far from the case. The sciences and the technologies to be studied are the main factors in determining this haphazard growth of interests and methods. I have never met two people who could agree on what the domain called 'science, technology and society' meant – in fact, I have rarely seen anyone agree on the name or indeed that the domain exists!

I claim that the domain exists, that there is a core of common problems and methods, that it is important and that all the disciplines and objects of 'science, technology and society' studies can be employed as so much specialised material with which to study it. To define what is at stake in this domain, the only thing we need is a few sets of concepts sturdy enough to stand the trip through all these many disciplines, periods and objects.

I am well aware that there exist many more sophisticated, subtle, fast or powerful notions than the ones I have chosen. Are they not going to break down? Are they going to last the distance? Will they be able to tie together enough scientists and citizens, cognitive anthropologists or cognitive psychologists, and are most often filed under the general label of 'science, technology and society'. It is on their work that this book is built. A summary of their many results and achievements would be worth doing, but is beyond the scope of my knowledge. Are empirical facts? Are they handy enough for doing practical exercises? These are the questions that guided me in selecting from the literature rules of method and principles and to dedicate one chapter to each pair**. The status of these rules and that of the principles is rather distinct and I do not expect them to be evaluated in the same way. By 'rules of methods' I mean what a priori decisions should be made in order to consider all of the empirical facts provided by the specialised disciplines as being part of the domain of 'science, technology and society'. By 'principles' I mean what is my personal summary of the empirical facts at hand after a decade of work in this area. Thus, I expect these principles to be debated, falsified, replaced by other summaries. On the other hand, the rules of method are a package that do not seem to be easily negotiable without losing sight of the common ground I want to sketch. With them it is more a question of all or nothing, and I think they should be judged only on this ground: do they link more elements than others? Do they allow outsiders to follow science and technology further, longer and more independently? This will be the only rule of the game, that is, the only 'meta' rule that we will need to get on with our work.

* The present book was originally planned with exercises at the end of each chapter. For lack of space, these practical tasks will be the object of a second volume.

** Except for the first rule of method defined above. A summary of these rules and principles is given at the end of the book.
CHAPTER 1

Literature

There are many methods for studying the fabrication of scientific facts and technical artefacts. However, the first rule of method we decided upon in the preceding Introduction is the simplest of all. We will not try to analyse the final products, a computer, a nuclear plant, a cosmological theory, the shape of a double helix, a box of contraceptive pills, a model of the economy; instead we will follow scientists and engineers at the times and at the places where they plan a nuclear plant, undo a cosmological theory, modify the structure of a hormone for contraception, or disaggregate figures used in a new model of the economy. We go from final products to production, from ‘cold’ stable objects to ‘warmer’ and unstable ones. Instead of black boxing the technical aspects of science and then looking for social influences and biases, we realised in the Introduction how much simpler it was to be there before the box closes and becomes black. With this simple method we merely have to follow the best of all guides, scientists themselves, in their efforts to close one black box and to open another. This relativist and critical stand is not imposed by us on the scientists we study; it is what the scientists themselves do, at least for the tiny part of technoscience they are working on.

To start our enquiry, we are going to begin from the simplest of all possible situations: when someone utters a statement, what happens when the others believe it or don’t believe it. Starting from this most general situation, we will be gradually led to more particular settings. In this chapter, as in the following, we will follow a character, whom we will for the moment dub ‘the dissenter’. In this first part of the book we will observe to what extremes a naive outsider who wishes to disbelieve a sentence is led.
Part A
Controversies

(1) Positive and negative modalities

What happens when someone disbelieves a sentence? Let me experiment with three simple cases:

1. New Soviet missiles aimed against Minuteman silos are accurate to 100 metres.¹
2. Since [new Soviet missiles are accurate within 100 metres] this means that Minutemans are not safe any more, and this is the main reason why the MX weapon system is necessary.
3. Advocates of the MX in the Pentagon cleverly leak information contending that [new Soviet missiles are accurate within 100 metres].

In statements (2) and (3) we find the same sentence (1) but inserted. We call these sentences modalities because they modify (or qualify) another one. The effects of the modalities in (2) and (3) are completely different. In (2) the sentence (1) is supposed to be solid enough to make the building of the MX necessary, whereas in (3) the very same statement is weakened since its validity is in question. One modality is leading us, so to speak, ‘downstream’ from the existence of accurate Soviet missiles to the necessity of building the MX; the other modality leads us ‘upstream’ from a belief in the same sentence (1) to the uncertainties of our knowledge about the accuracy of Soviet missiles. If we insist we may be led even further upstream, as in the next sentence:

4. The undercover agent 009 in Novosibirsk whispered to the housemaid before dying that he had heard in bars that some officers thought that some of their [missiles] in ideal test conditions might [have an accuracy] somewhere between 100 and 1000 [metres] or this is at least how the report came to Washington.

In this example, statement (1) is not inserted in another phrase any more, it is broken apart and each fragment—which I have put in brackets—is brought back into a complex process of construction from which it appears to have been extracted. The directions towards which the readers of sentences (2) and (4) are invited to go are strikingly different. In the first case, they are led into the Nevada desert of the United States to look for a suitable site for the MX; in the second case they are led towards the Pentagon sitting through the CIA network of spies and disinformation. In both cases they are induced to ask different sets of questions. Following statement (1), they will ask if the MX is well designed, how much it will cost and where to locate it; believing statements (2) or (4), they will ask how the CIA is organised, why the information has been leaked, who killed agent 009, how the test conditions of missiles in Russia are set up, and so on. A reader who does not know which sentence to believe will hesitate between two attitudes; either demonstrating against the Russians for the MX or against the

CIA for a Congressional hearing on the intelligence establishment. It is clear that anyone who wishes the reader of these sentences to demonstrate against the Russians or against the CIA must make one of the statements more credible than the other.

We will call positive modalities those sentences that lead a statement away from its conditions of production, making it solid enough to render some other consequences necessary. We will call negative modalities those sentences that lead a statement in the other direction towards its conditions of production and that explain in detail why it is solid or weak instead of using it to render some other consequences more necessary.

Negative and positive modalities are in no way particular to politics. The second, and more serious, example will make this point clear:

5. The primary structure of Growth Hormone Releasing Hormone² (GHRH) is Val-His-Leu-Ser-Ala-Glu-Glu-Lys-Glu-Ala.

6. Now that Dr Schally has discovered [the primary structure of GHRH], it is possible to start clinical studies in hospital to treat certain cases of dwarfism since GHRH should trigger the Growth Hormone they lack.

7. Dr A. Schally has claimed for several years in his New Orleans laboratory that [the structure of GHRH was Val-His-Leu-Ser-Ala-Glu-Glu-Lys-Glu-Ala]. However, by troubling coincidence this structure is also that of haemoglobin, a common component of blood and a frequent contaminant of purified brain extract if handled by incompetent investigators.

Sentence (5) is devoid of any trace of ownership, construction, time and place. It could have been known for centuries or handed down by God Himself together with the Ten Commandments. It is, as we say, a fact. Full stop. Like sentence (1) on the accuracy of Soviet missiles, it is inserted into other statements without further modification: no more is said about GHRH; inside this new sentence, sentence (5) becomes a closed-file, an indisputable assertion, a black box. It is because no more has to be said about it that it can be used to lead the reader somewhere else downstream, for instance to a hospital ward, helping dwarves to grow. In sentence (7) the original fact undergoes a different transformation similar to what happened to the accuracy of Soviet missiles in statements (3) and (4). The original statement (5) is uttered by someone situated in time and space; more importantly, it is seen as something extracted from a complicated work situation, not as a gift from God but as a man-made product. The hormone is isolated out of a soup made of many ingredients; it might be that Dr Schally has mistaken a contaminant for a genuine new substance. The proof of that is the ‘troubling coincidence’ between the GHRH sequence and that of the beta-chain of haemoglobin. They might be homonyms, but can you imagine anybody that would confuse the order to ‘release growth hormone!’ with the command ‘give me your carbon dioxide!’?

Depending on which sentence we believe, we, the readers, are again induced to go in opposite directions. If we follow statement (6) that takes GHRH as a fact, then we now look into possible cures for dwarfism, we explore ways of
industrially producing masses of GHRH we go into hospitals to blind-test the drug, etc. If we believe (7) we are led back into Dr Schally’s laboratory in New Orleans, learning how to purify brain extracts, asking technicians if some hitch has escaped their attention, and so on. According to which direction we go, the original sentence (5) will change status: it will be either a black box or a fierce controversy; either a solid timeless certainty or one of these short-lived artefacts that appear in laboratory work. Inserted inside statement (6), (5) will provide the firm ground to do something else; but the same sentence broken down inside (7) will be one more empty claim from which nothing can be concluded.

A third example will show that these same two fundamental directions may be recognised in engineers’ work as well:

(8) The only way to quickly produce efficient fuel cells is to focus on the behaviour of electrodes.

(9) Since [the only way for our company to end up with efficient fuel cells is to study the behaviour of electrodes], and since this behaviour is too complicated, I propose to concentrate in our laboratory next year on the one-pore model.

(10) You have to be a metallurgist by training to believe you can tackle [fuel cells] through the [electrode] problem. There are many other ways they cannot even dream of because they don’t know solid state physics. One obvious way for instance to study electrocatalysis. If they get bogged down with their electrode, they won’t move an inch.

Sentence (8) gives as a matter of fact the only research direction that will lead the company to the fuel cells, and thence to the future electric engine that, in the eyes of the company, will eventually replace most—if not all—internal combustion engines. It is then taken up by statement (9) and from it a research programme is built: that of the one-pore model. However, in sentence (10) the matter-of-fact tone of (8) is not borrowed. More exactly, it shows that (8) has not always been a matter of fact but is the result of a decision taken by specific people whose training in metallurgy and whose ignorance are outlined. The same sentence then proposes another line of research using another discipline and other laboratories in the same company.

It is important to understand that statement (10) does not in any way dispute that the company should get at fast and efficient fuel cells; it extracts this part of sentence (8) which it takes as a fact, and contests only the idea of studying the electrode as the best way of reaching that undisputed goal. If the reader believes in claim (9), then the belief in (8) is reinforced; the whole is taken as a package and goes where it leads the research programme, deep inside the metallurgy section of the company, looking at one-pore models of electrodes and spending years there expecting the breakthrough. If the reader believes in claim (10), then it is realised that the original sentence (8) was not one black box but at least two; the first is kept closed—fuel cells are the right goal; the other is opened—the one-pore model is an absurdity; in order to maintain the first, then the company should get into quantum physics and recruit new people. Depending on who is believed, the company may go broke or not; the consumer, in the year 2000, may drive a fuel cell electric car or not.

From these three much simpler and much less prestigious examples than the ones we saw in the Introduction, we may draw the following conclusions. A sentence may be made more of a fact or more of an artefact depending on how it is inserted into other sentences. By itself a given sentence is neither a fact nor a fiction; it is made so by others, later on. You make it more of a fact if you insert it as a closed, obvious, firm and packaged premise leading to some other less closed, less obvious, less firm and less united consequence. The final shape of the MX is less determined in sentence (2) than in the history of Soviet missiles; the cure for dwarfism is not yet as well settled in sentence (6) as in the GHRH structure; although in sentence (9) it is certain that the right path towards fuel cells is to look at electrodes, the one-pore model is less certain than this indisputable fact. As a consequence, listeners make sentences less of a fact if they take them back where they came from, to the mouths and hands of whoever made them, or more of a fact if they use it to reach another, more uncertain goal. The difference is as great as going up or down a river. Going downstream, listeners are led to a demonstration against the Russians—see (2), to clinical studies of dwarfism—see (6), to metallurgy—see (9). Upstream, they are directed to probe the CIA—see (3), to do research in Dr Schally’s laboratory—see (7), or to investigations on what quantum physics can tell us about fuel cells—see (10).

We understand now why looking at earlier stages in the construction of facts and machines is more rewarding than remaining with the final stages. Depending on the type of modalities, people will be made to go along completely different paths. If we imagine someone who has listened to claims (2), (6) and (9), and believed them, his behaviour would have been the following: he would have voted for pro-MX congressmen, bought shares in GHRH-producing companies, and recruited metallurgists. The listener who believed claims (3), (4), (7) and (10) would have studied the CIA, contested the purification of brain extracts, and would have recruited quantum physicists. Considering such vastly different outcomes, we can easily guess that it is around modalities that we will find the fiercest disputes since this is where the behaviour of other people will be shaped.

There are two added bonuses for us in following the earlier periods of fact construction. First, scientists, engineers and politicians constantly offer us rich material by transforming one another’s statements in the direction of fact or fiction. They break the ground for our analysis. We, laymen and citizens, would be unable to discuss sentences (1) on the accuracy of Soviet missiles, (5) on the amino acid structure of growth hormone releasing factor, and (8) on the right way of making fuel cells. But since others dispute them and push them back into their conditions of production, we are effortlessly led to the processes of work that extract information from spies, brain soup or electrodes—processes of work we would never have suspected before. Secondly, in the heat of the controversy, specialists may themselves explain why their opponents think otherwise: sentence (3) claims that the MX partisans are
interested in believing the accuracy of Soviet missiles; in sentence (10) the belief of the others in one absurd research project is imputed to their training as metallurgists. In other words, when we approach a controversy more closely, half of the job of interpreting the reasons behind the beliefs is already done!

(2) The collective fate of fact-making

If the two directions I outlined were so clearly visible to the eyes of someone approaching the construction of facts, there would be a quick end to most debates. The problem is that we are never confronted with such clear intersections. The three examples I chose have been arbitrarily interrupted to reveal only two neatly distinct paths. If you let the tape go on a bit longer the plot thickens and the interpretation becomes much more complicated.

Sentences (3) and (4) denied the reports about the accuracy of the Soviet missiles. But (4) did so by using a police story that exposed the inner workings of the CIA. A reply to this exposition can easily be imagined:

(11) The CIA's certainty concerning the 100-metre accuracy of Russian missiles is not based on the agent 009's report, but on five independent sources. Let me suggest that only groups subsidised by Soviets could have an interest in casting doubts on this incontrovertible fact.

Now the readers are not sure any more where they should go from here. If sentence (4), denying the truth of sentence (1), is itself denied by (11), what should they do? Should they protest against the disinformation specialists paid by the KGB who forged sentence (4) and go on with the MX project with still more determination? Should they, on the contrary, protest against the disinformation specialists paid by the CIA who concocted (11), and continue their hearings on the intelligence gathering network with more determination? In both cases, the determination increases, but does the uncertainty! Very quickly, the controversy becomes as complex as the arms race: missiles (arguments) are opposed by anti-ballistic missiles (counter-arguments) which are in turn counter-attacked by other smarter weapons (arguments).

If we now turn to the second example, it is very easy to go on after sentence (7), which criticised Dr Schally's handling of GHRH, and retort:

(12) If there is a 'troubling coincidence', it is in the fact that criticisms against Schally's discovery of GHRH are again levelled by his old foe, Dr Guillemot... As to the homonymy of structure between haemoglobin and GHRH, so what? It does not prove Schally mistook a contaminant for a genuine hormone, no more than 'he had a fit' may be taken for 'he was fit'.

Reading (6) that assumed the existence of GHRH, you, the reader, might have decided to invest money in pharmaceutical companies; when learning of (7), you would have cancelled all plans and might have started investigations on how the Veterans Administration could support such inferior work with public funds.

But after reading the counter claims in (12), what do you do? To make up your mind you should now assess Dr Guillemot's personality. Is he a man wicked enough to cast doubt on a competitor's discovery out of sheer jealousy? If you believe so, then (7) is cancelled, which frees the original sentence (5) from doubts. If, on the contrary, you believe in Guillemot's honesty, then it is sentence (12) which is in jeopardy, and then the original claim (5) is again in danger...

In this example the only thing that stands firm is this point about homonymy. At this point, to make up your mind you have to dig much further into physiology: is it possible for the blood to carry two homonymous messages to the cells without wreaking havoc in the body?

Asking these two questions - about Guillemot's integrity and about a principle of physiology - you might hear the retort (to the retort of the retort):

(13) Impossible! It cannot be an homonymy. It is just a plain mistake made by Schally. Anyway, Guillemot has always been more credible than him. I wouldn't trust this GHRH an inch, even if it is already manufactured, advertised in medical journals, and even sold to physicians!

With such a sentence the reader is now watching a game of billiards: if (13) is true, then (12) was badly wrong, with the consequence that (7), that disputed the very existence of Schally's substance, was right, which means that (5) - the original claim - is disallowed. Naturally, the question would now be to assess the credibility of sentence (13) above. If it is uttered by an uncritical admirer of Guillemot or by someone who knows nothing of physiology, then (12) might turn out to be quite credible, which would knock (7) off the table and would thus establish (5) as an ascertained fact!

To spare the reader's patience I will stop the story here, but it is now obvious that the debate could go on. The first important lesson, here, is this: were the debate to continue, we would delve further into physiology, further into Schally's and Guillemot's personalities, and much further into the details through which hormone structures are obtained. The number of new conditions of production to tackle will take us further and further from dwarves and hospital wards. The second lesson is that, with every new retort added to the debate, the status of the original discovery made by Schally in claim (5) will be modified. Inserted in (6) it becomes more of a fact; less when it is dislocated in (7); more with (12) that destroys (7); less again with (13); and so on. The fate of the statement, that is the decision about whether it is a fact or a fiction, depends on a sequence of debates later on. The same thing happens not only for (5), which I artificially chose as the origin of the debate, but also with each of the other sentences that qualifies or modifies it. For instance (7), which disputed Schally's ability, is itself made more of a fact with (13) that established Guillemot's honesty, but less with (12) that doubted his judgment. These two lessons are so important that this book is simply, I could argue, a development of this essential point: _the status of a statement depends on later statements_. It is made more of a certainty or less of a certainty depending on the next sentence that takes it up; this retrospective
attraction is repeated for this next new sentence, which in turn might be made more of a fact or more of a fiction by a third, and so on....

The same essential phenomenon is visible in the third example. Before a machine is built many debates take place to determine its shape, function, or cost. The debate about the fuel cells may be easily rekindled. Sentence (10) was disputing that the right avenue to fuel cells was the one-pore electrode mode, but not that fuel cells were the right path towards the future of electric cars. A retort may come:

(14) And why get into quantum mechanics anyway? To spend millions helping physicists with their pet projects? That’s bootlegging, not technological innovation, that’s what it is. The electric automobile’s only future is all very simple: batteries; they are reliable, cheap and already there. The only problem is weight, but if research were done into that instead of into physics, they would be lighter pretty soon.

A new pathway is proposed to the company. Physics, which for sentence (10) was the path to the breakthrough, is now the archetypical dead end. The future of fuel cells, which in statements (8), (9) and (10) were packaged together with the electric car in one black box, now lies open to doubt. Fuel cells are replaced by batteries. But in sentence (14) electric cars are still accepted as an undisputable premise. This position is denied by the next claim:

(15) Listen, people will always use internal combustion engines, no matter what the cost of petrol. And you know why? Because it has got go. Electric cars are sluggish; people will never buy them. They prefer vigorous acceleration to everything else.

Suppose that you have a place on the company board that has to decide whether or not to invest in fuel cells. You would be rather puzzled by now. When you believed (9) you were ready to invest in the one-pore electrode model as it was convincingly defined by metallurgists. Then you shifted your loyalties when listening to (10) that criticised metallurgists and wished to invest in quantum physics, recruiting new physicists. But after listening to (14), you decided to buy shares in companies manufacturing traditional batteries. After listening to (15), though, if you believe it, you would be better not selling any of your General Motor shares. Who is right? Whom should you believe? The answer to this question is not in any one of the statements, but in what everyone is going to do with them later on. If you wish to buy a car, will you be stopped by the high price of petrol? Will you shift to electric cars, more sluggish but cheaper? If you do so, then sentence (15) is wrong, and (8), (9) or (10) was right, since they all wanted electric cars. If the consumer buys an internal combustion engine car without any hesitation and doubts, then claim (15) is right and all the others were wrong to invest millions in useless technologies without a future.

This retrospective transformation of the truth value of earlier sentences does not happen only when the average consumer at the end of the line gets into the picture, but also when the Board of Directors decides on a research strategy. Suppose that you ‘bought the argument’ presented in statement (10). You go for electric cars, you believe in fuel cells, and in quantum physics as the only way to get at them. All the other statements are made more wrong by this decision. The linkages between the future of the automobile, the electric engine, the fuel cells, and electrophysics are all conflated in one single black box which no one in the company is going to dispute. Everyone in the company will start from there: “Since sentence (10) is right then let’s invest so many millions.” As we will see in Chapter 3, this does not mean that your company will win. It means that, as far as you could, you shaped the other machines and facts of the past so as to win: the internal combustion engine is weakened by your decision and made more of an obsolete technology; by the same token electrophysics is strengthened, while the metallurgy section of the company is gently excluded from the picture. Fuel cells now have one more powerful ally: the Board of Directors.

Again I interrupt the controversy abruptly for practical reasons; the company may go broke, become the IBM of the twenty-first century or linger for years in limbo. The point of the three examples is that the fate of what we say and makes is in later users’ hands. Buying a machine without question or believing a fact without question has the same consequence: it strengthens the case of whatever is bought or believed, it makes it more of a black box. To disbelieve or, so to speak, ‘dis-buy’ either a machine or a fact is to weaken its case, interrupt its spread, transform it into a dead end, reopen the black box, break it apart and reallocate its components elsewhere. By themselves, a statement, a piece of machinery, a process are lost. By looking only at them and at their internal properties, you cannot decide if they are true or false, efficient or wasteful, costly or cheap, strong or frail. These characteristics are only gained through incorporation into other statements, processes and pieces of machinery. These incorporations are decided by each of us, constantly. Confronted with a black box, we take a series of decisions. Do we take it up? Do we reject it? Do we reopen it? Do we let it drop through lack of interest? Do we make it more solid by grasping it without any further discussion? Do we transform it beyond recognition? This is what happens to others’ statements, in our hands, and what happens to our statements in others’ hands. To sum up, the construction of facts and machines is a collective process. (This is the statement I expect you to believe; its fate is in your hands like that of any other statements.) This is so essential for the continuation of our travel through technoscience* that I will call it our first principle: the remainder of this book will more than justify this rather portentous name.

*In order to avoid endless ‘science and technology’ I forged this word, which will be fully defined in Chapter 4 only.
From one statement to another

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**Figure 1.9**

- a measure of the distance between the original claim and the new ones, as we saw for instance between Schally’s sentence (5) about GHRH made in 1971, and Guillemin’s claim made in 1982 about the same substance named GRF and with a completely different amino acid sequence. This drift will provide us with our second bearing, our longitude.

Finally, the two dimensions put together will draw:

- the front line of the controversy as summarized in Figure 1.9.

**Conclusion**

**Numbers, more numbers**

Having reached the end of this chapter, it should be clear now why most people do not write and do not read scientific texts. No wonder! It is a peculiar trade in a merciless world. Better read novels! What I will call fact-writing in opposition to fiction-writing limits the number of possible readings to three: giving up, going along, working through. Giving up is the most usual one. People give up and do not read the text, whether they believe the author or not, either because they are pushed out of the controversy altogether or because they are not interested in reading the article (let us estimate this to be 90 per cent of the time). Going along is the rare reaction, but it is the normal outcome of scientific rhetoric: the reader believes the author’s claim and helps him to turn it into a fact by using it further with no dispute (maybe 9 per cent of the time?). There is still one more possible outcome, but such a rare and costly one that it is almost negligible as far as numbers are concerned: re-enacting everything that the authors went through. This last issue remains open because there is always at least one flaw even in the best written scientific text: many resources mobilised in it are said to come from instruments, animals, pictures, from things out of the text. The adamant objector could then try to put the text in jeopardy by unravelling these supply lines. He or she will then be led from the text to where the text claims to come from: Nature or the laboratory. This is possible on one condition: that the dissenter is equipped with a laboratory or with ways to get straight at Nature more or less similar to that of the author. No wonder this way of reading a scientific paper is rare! You have to have a whole machinery of your own. Resuming the controversy, reopening the black box is achieved at this price, and only at this price. It is this rare remaining strategy that we will study in the next chapter.

The peculiarity of the scientific literature is now clear: the only three possible readings all lead to the demise of the text. If you give up, the text does not count and might as well not have been written at all. If you go along, you believe it so much that it is quickly abstracted, abridged, stylised and sinks into tacit practice. Lastly, if you work through the authors’ trials, you quit the text and enter the laboratory. Thus the scientific text is chasing its readers away whether or not it is successful. Made for attack and defence, it is no more a place for a leisurely stay than a bastion or a bunker. This makes it quite different from the reading of the Bible, Stendhal or the poems of T.S. Eliot.

Yes, Galileo was quite mistaken when he purported to oppose rhetoric and science by putting big numbers on one side and one ‘average man who happened to hit upon the truth’ on the other. Everything we have seen since the beginning indicates exactly the opposite. Any average man starting off a dispute ends up being confronted with masses of resources, not just 2000, but tens of thousands. So what is the difference between rhetoric, so much despised, and science, so much admired? Rhetoric used to be despised because it mobilised external allies in favour of an argument, such as passion, style, emotions, interests, lawyers’ tricks and so on. It has been hated since Aristotle’s time because the regular path of reason was unfairly distorted or reversed by any passing sophist who invoked passion and style. What should be said of the people who invoke so many more external allies besides passion and style in order to reverse the path of common reasoning? The difference between the old rhetoric and the new is not that the first makes use of external allies which the second refrains from using; the difference is that the first uses only a few of them and the second very many. This distinction allows me to avoid a wrong way of interpreting this chapter which would be to say that we studied the ‘rhetorical aspects’ of technical literature, as if the other aspects could be left to reason, logic and technical details. My contention is that on the contrary we must eventually come to call scientific the rhetoric able to mobilise on one spot more resources than older ones (see Chapter 6).

It is because of this definition in terms of the number of allies that I abstained from defining this literature by its most obvious trait: the presence of numbers, geometrical figures, equations, mathematics, etc. The presence of these objects will be explained only in Chapter 6 because their form is impossible to understand when separated from this mobilisation process made necessary by the intensity of the rhetoric. So the reader should not be worried either by the
presence or by the absence of figures in the technical literature. So far it is not the relevant feature. We have to understand first how many elements can be brought to bear on a controversy; once this is understood, the other problems will be easier to solve.

By studying in this chapter how a controversy gets fiercer, I examined the anatomy of technical literature and I claimed that it was a convenient way to make good my original promise to show the heterogeneous components that make up technoscience, including the social ones. But I'd rather anticipate the objection of my (semiotic) reader: 'What do you mean "social"?' it indignantly says. 'Where is capitalism, the proletarian classes, the battle of the sexes, the struggle for the emancipation of the races, Western culture, the strategies of wicked multinational corporations, the military establishment, the devious interests of professional lobbies, the race for prestige and rewards among scientists? All these elements are social and this is what you did not show with all your texts, rhetorical tricks and technicalities!'

I agree, we saw nothing of that sort. What I showed, however, was something much more obvious, much less far-fetched, much more pervasive than any of these traditional social actors. We saw a literature becoming more technical by bringing in more and more resources. In particular, we saw a dissonant-driven into isolation because of the number of elements the authors of scientific articles mustered on their side. Although it sounds counterintuitive at first, the more technical and specialised a literature is, the more 'social' it becomes, since the number of associations necessary to drive readers out and force them into accepting a claim as a fact increase. Mr Anybody's claim was easy to deny; it was much harder to shrug off Schally's article on GHRH, sentence (16), not because the first is social and the second technical, but because the first is one man's word and the second is many well-equipped men's words; the first is made of a few associations, the second of many. To say it more bluntly, the first is a little social, the second extremely so. Although this will become understandable much later, it is already clear that if being isolated, besieged, and left without allies and supporters is not a social act, then nothing is. The distinction between the technical literature and the rest is not a natural boundary; it is a border created by the disproportionate amount of linkages, resources and allies locally available. This literature is so hard to read and analyse not because it escapes from all normal social links, but because it is more social than so-called normal social ties.

CHAPTER 2

Laboratories

We could stop our enquiry where we left it at the end of the previous chapter. For a layperson, studying science and technology would then mean analysing the discourse of scientists, or counting citations, or doing various bibliometric calculations, or performing semiotic studies of scientific texts and of their iconography, that is, extending literary criticism to technical literature. No matter how interesting and necessary these studies are, they are not sufficient if we want to follow scientists and engineers at work; after all, they do not draft, read and write papers twenty-four hours a day. Scientists and engineers invariably argue that there is something behind the technical texts which is much more important than anything they write.

At the end of the previous chapter, we saw how the articles forced the reader to choose between three possible issues: giving up (the most likely outcome), going along, or working again through what the author did. Using the tools we devised in Chapter 1, it is now easy to understand the first two issues, but we are as yet unable to understand the third. Later, in the second part of this book, we will see many other ways to avoid this issue and still win over in the course of a controversy. For the sake of clarity, however, I make the supposition in this part that the dissenter has no other escape but to work through what the author of the paper did. Although it is a rare outcome, it is essential for us to visit the places where the papers are said to originate. This new step in our trip through technoscience is much more difficult, because, whilst the technical literature is accessible in libraries, archives, patent offices or corporate documentation centres, it is much less easy to sneak into the few places where the papers are written and to follow the construction of facts in their most intimate details. We have no choice, however, if we want to apply our first rule of method: if the scientists we shadow go inside laboratories, then we too have to go there, no matter how difficult the journey.
Part A
From texts to things: a showdown

'You doubt what I wrote? Let me show you.' The very rare and obstinate dissenter who has not been convinced by the scientific text, and who has not found other ways to get rid of the author, is led from the text into the place where the text is said to come from. I will call this place the laboratory, which for now simply means, as the name indicates, the place where scientists work. Indeed, the laboratory was present in the texts we studied in the previous chapter: the articles were alluding to 'patients', to 'tumours', to 'HPLC', to 'Russian spies', to 'engines'; dates and times of experiments were provided and the names of technicians acknowledged. All these allusions however were made within a paper world; they were a set of semiotic actors presented in the text but not present in the flesh; they were alluded to as if they existed independently from the text; they could have been invented.

(1) Inscriptions

What do we find when we pass through the looking glass and accompany our obstinate dissenter from the text to the laboratory? Suppose that we read the following sentence in a scientific journal and, for whatever reason, do not wish to believe it:

(1) 'Fig. 1 shows a typical pattern. Biological activity of endorphin was found essentially in two zones with the activity of zone 2 being totally reversible, or statistically so, by naloxone.'

We, the dissenters, question this figure 1 so much, and are so interested in it, that we go to the author's laboratory (I will call him 'the Professor'). We are led into an air-conditioned, brightly lit room. The Professor is sitting in front of an array of devices that does not attract our attention at first. 'You doubt what I wrote? Let me show you.' This last sentence refers to an image slowly produced by one of these devices (Figure 2.1):

Figure 2.1

(2)

'OK. This is the base line; now, I am going to inject endorphin, what is going to happen? See?'

(Figure 2.2)

Figure 2.2

'Immediately the line drops dramatically. And now watch naloxone. See? Back to base line levels. It is fully reversible.'

We now understand that what the Professor is asking us to watch is related to the figure in the text of sentence (1). We thus realise where this figure comes from. It has been extracted from the instruments in this room, cleaned, redrawn, and displayed. We now seem to have reached the source of all these images that we saw arrayed in the text as the final proofs of all the arguments in Chapter 1. We also realise, however, that the images that were the last layer in the text, are the end result of a long process in the laboratory that we are now starting to observe. Watching the graph paper slowly emerging out of the physiograph, we understand that we are at the junction of two worlds: a paper world that we have just left, and one of instruments that we are just entering. A hybrid is produced at the interface: a raw image, to be used later in an article, that is emerging from an instrument.

For a time we focus on the stylus pulsating regularly, inking the paper, scribbling cryptic notes. We remain fascinated by this fragile film that is in between text and laboratory. Soon, the Professor draws our attention beneath and beyond the traces on the paper, to the physiograph from which the image is slowly being emitted. Beyond the stylus a massive piece of electronic hardware records, calibrates, amplifies and regulates signals coming from another instrument, an array of glassware. The Professor points to a glass chamber in which bubbles are regularly flowing around a tiny piece of something that looks like elastic. It is indeed elastic, the Professor intones. It is a piece of gut, guinea pig gut ('myenteric plexus-longitudinal muscle of the guinea pig ileum', are his words). This gut has the property of contracting regularly if maintained alive. This regular pulsation is easily disturbed by many chemicals. If one hooks the gut up so that each contraction sends out an electric pulse, and if the pulse is made to move a stylus over graph paper, then the guinea pig gut will be induced to produce regular scribbles over a long period. If you then add a chemical to the chamber you see the peaks drawn by the inked stylus slow down or accelerate at the other end. This perturbation, invisible in the chamber, is visible on paper: the
chemical, no matter what it is, is given a shape on paper. This shape 'tells you something' about the chemical. With this set-up you may now ask new questions: if I double the dose of chemical will the peaks be doubly decreased? And if I triple it, what will happen? I can now measure the white surface left by the decreasing scribbles directly on the graph paper, thereby defining a quantitative relation between the dose and the response. What if, just after the first chemical is added, I add another one which is known to counteract it? Will the peaks go back to normal? How fast will they do so? What will be the pattern of this return to the base line level? If two chemicals, one known, the other unknown, trace the same slope on the paper, may I say, in this respect at least, that they are the same chemicals? These are some of the questions the Professor is tackling with endorphin (unknown), morphine (well known) and naloxone (known to be an antagonist of morphine).

We are no longer asked to believe the text that we read in Nature; we are now asked to believe our own eyes, which can see that endorphin is behaving exactly like morphine. The object we looked at in the text and the one we are now contemplating are identical except for one thing. The graph of sentence (1) which was the most concrete and visual element of the text, is now in (2) the most abstract and textual element in a bewildering array of equipment. Do we see more or less than before? On the one hand we can see more, since we are looking at not only the graph but also the physiograph, and the electronic hardware, and the glassware, and the electrodes, and the bubbles of oxygen, and the pulsating ileum, and the Professor who is injecting chemicals into the chamber with his syringe, and is writing down in a huge protocol book the time, amount of and reactions to the doses. We can see more, since we have before our eyes not only the image but what the image is made of.

On the other hand we see less because now each of the elements that makes up the final graph could be modified so as to produce a different visual outcome. Any number of incidents could blur the tiny peaks and turn the regular writing into a meaningless-doodle. Just at the time when we feel comforted in our belief and start to be fully convinced by our own eyes watching the image, we suddenly feel uneasy because of the fragility of the whole set up. The Professor, for instance, is swearing at the gut saying it is a 'bad gut'. The technician who sacrificed the guinea pig is held responsible and the Professor decides to make a fresh start with a new animal. The demonstration is stopped and a new scene is set up. A guinea pig is placed on a table, under surgical floodlights, then anaesthetised, crucified and sliced open. The gut is located, a tiny section is extracted, useless tissue peeled away, and the precious fragment is delicately hooked up between two electrodes and immersed in a nutrient fluid so as to be maintained alive. Suddenly, we are much further from the paper world of the article. We are now in a puddle of blood and viscera, slightly nauseated by the extraction of the ileum from this little furry creature. In the last chapter, we admired the rhetorical abilities of the Professor as an author. Now, we realize that many other manual abilities are required in order to write a convincing paper later on. The guinea pig alone would not have been able to tell us anything about the similarity of endorphin to morphine; it was not mobilisable into a text and would not help to convince us. Only a part of its gut, tied up in the glass chamber and hooked up to a physiograph, can be mobilised in the text and add to our conviction. Thus, the Professor's art of convincing his readers must extend beyond the paper to preparing the ileum, to calibrating the peaks, to tuning the physiograph.

After hours of waiting for the experiment to resume, for new guinea pigs to become available, for new endorphin samples to be purified, we realize that the invitation of the author ('let me show you') is not as simple as we thought. It is a slow, protracted and complicated staging of tiny images in front of an audience. 'Showing' and 'seeing' are not simle flashes of intuition. Once in the lab we are not presented outright with the real endorphin whose existence we doubted. We are presented with another world in which it is necessary to prepare, focus, fix and rehearse the vision of the real endorphin. We came to the laboratory in order to settle our doubts about the paper, but we have been led into a labyrinth.

This unexpected unfolding makes us shiver because it now dawns on us that if we disbelieve the traces obtained on the physiograph by the Professor, we will have to give up the topic altogether or go through the same experimental chores all over again. The stakes have increased enormously since we first started reading scientific articles. It is not a question of reading and writing back to the author any more. In order to argue, we would now need the manual skills required to handle the scalps, peel away the guinea pig ileum, interpret the decreasing peaks, and so on. Keeping the controversy alive has already forced us through many difficult moments. We now realize that what we went through is nothing compared to the scale of what we have to undergo if we wish to continue. In Chapter 1, we only needed a good library in order to dispute texts. It might have been costly and not that easy, but it was still feasible. At this present point, in order to go on, we need guinea pigs, surgical lamps and tables, physiographs, electronic hardware, technicians and morphine, not to mention the scare flasks of purified endorphin; we also need the skills to use all these elements and to turn them into a pertinent objection to the Professor's claim. As will be made clear in Chapter 4, longer and longer detours will be necessary to find a laboratory, buy the equipment, hire the technicians and become acquainted with the ileum assay. All this work just to start making a convincing counter-argument to the Professor's original paper on endorphin. (And when we have made this detour and finally come up with a credible objection, where will the Professor be?)

When we doubt a scientific text we do not go from the world of literature to Nature as it is. Nature is not directly beneath the scientific article; it is there indirectly at best (see Part C). Going from the paper to the laboratory is going from an array of rhetorical resources to a set of new resources devised in such a way as to provide the literature with its most powerful tool: the visual display. Moving from papers to labs is moving from literature to convoluted ways of getting this literature (or the most significant part of it).

This move through the looking glass of the paper allows me to define an instrument, a definition which will give us our bearings when entering any
laboratory. I will call an instrument (or inscription device) any set-up, no matter what its size, nature and cost, that provides a visual display of any sort in a scientific text. This definition is simple enough to let us follow scientists’ moves. For instance an optical telescope is an instrument, but so is an array of several radio-telescopes even if its constituents are separated by thousands of kilometers. The guinea pig ileum assay is an instrument even if it is small and cheap compared to an array of radiotelescopes or the Stanford linear accelerator. The definition is not provided by the cost nor by the sophistication but only by this characteristic: the set-up provides an inscription that is used as the final layer in a scientific text. An instrument, in this definition, is not every set-up which ends with a little window that allows someone to take a reading. A thermometer, a watch, a Geiger counter, all provide readings but are not considered as instruments as long as these readings are not used as the final layer of technical papers (but see Chapter 6). This point is important when watching complicated contrivances with hundreds of intermediary readings taken by dozens of white-coated technicians. What will be used as visual proof in the article will be the few lines in the bubble chamber and not the piles of printout making the intermediate readings.

It is important to note that the use of this definition of instrument is a relative one. It depends on time. Thermometers were instruments and very important ones in the eighteenth century, so were Geiger counters between the First and Second World Wars. These devices provided crucial resources in papers of the time. But now they are only parts of larger set-ups and are only used so that a new visual proof can be displayed at the end. Since the definition is relative to the use made of the ‘window’ in a technical paper, it is also relative to the intensity and nature of the associated controversy. For instance, in the guinea pig ileum assay there is a box of electronic hardware with many readings that I will call ‘intermediate’ because they do not constitute the visual display eventually put to use in the article. It is unlikely that anyone will quibble about this because the calibration of electronic signals is now made through a black box produced industrially and sold by the thousand. It is a different matter with the huge tank built in an old gold mine in South Dakota at a cost of $600,000 (1964 dollars) by Raymond Davis to detect solar neutrinos. In a sense the whole set-up may be considered as one instrument providing one final window in which astrophysicists can read the number of neutrinos emitted by the sun. In this case all the other readings are intermediate ones. If the controversy is fiercer, however, the set-up is broken down into several instruments, each providing a specific visual display which has to be independently evaluated. If the controversy heats up a bit we do not see neutrinos coming out of the sun. We see and hear a Geiger counter that clicks when Argon decays. In this case the Geiger counter, which gave only an intermediate reading when there was no dispute, becomes an instrument in its own right when the dispute is raging.

The definition I use has another advantage. It does not make presuppositions about what the instrument is made of. It can be a piece of hardware like a telescope, but it can also be made of softer material. A statistical institution that employs hundreds of pollsters, sociologists and computer scientists gather all sorts of data on the economy is an instrument if it yields inscriptions for papers written in economic journals with, for instance, a graph of the inflation rate by month and by branch of industry. No matter how many people were made to participate in the construction of the image, no matter how long it took, no matter how much it cost, the whole institution is used as one instrument (as long as there is no controversy that calls its intermediate readings into question).

At the other end of the scale, a young primatologist who is watching baboons in the savannah and is equipped only with binoculars, a pencil and a sheet of white paper may be seen as an instrument if her coding of baboon behaviour is summed up in a graph. If you want to deny her statements, you might (everything else being equal) have to go through the same ordeals and walk through the savannah taking notes with similar constraints. It is the same if you wish to deny the inflation rate by month and industry, or the detection of endorphin with the ileum assay. The instrument, whatever its nature, is what leads you from the paper to what supports the paper, from the many resources mobilised in the text to the many more resources mobilised to create the visual displays of the texts. With this definition of an instrument, we are able to ask many questions and to make comparisons: how expensive they are, how old they are, how many intermediate readings compose one instrument, how long it takes to get one reading, how many people are mobilised to activate them, how many authors are using the inscriptions they provide in their papers, how controversial are those readings... Using this notion we can define more precisely than earlier the laboratory as any place that gathers one or several instruments together.

What is behind a scientific text? Inscriptions. How are these inscriptions obtained? By setting up instruments. This other world just beneath the text is invisible as long as there is no controversy. A picture of moon valleys and mountains is presented to us as if we could see them directly. The telescope that makes them visible is invisible and so are the fierce controversies that Galileo had to wage centuries ago to produce an image of the Moon. Similarly, in Chapter 1, the accuracy of Soviet missiles was just an obvious statement; it became the outcome of a complex system of satellites, spies, Kremilinologists and computer simulation, only after the controversy got started. Once the fact is constructed, there is no instrument to take into account and this is why the painstaking work necessary to tune the instruments often disappears from popular science. On the contrary, when science in action is followed, instruments become the crucial elements, immediately after the technical texts; they are where the dissenter is inevitably led.

There is a corollary to this change of relevance on the inscription devices depending on the strength of the controversy, a corollary that will become more important in the next chapter. If you consider only fully-fledged facts it seems that everyone could accept or contest them equally. It does not cost anything to contradict or accept them. If you dispute further and reach the frontier where facts are made, instruments become visible and with them the cost of continuing the discussion rises. It appears that arguing is costly. The equal world of citizens
having opinions about things becomes an unequal world in which dissent or consent is not possible without a huge accumulation of resources which permits the collection of relevant inscriptions. What makes the differences between author and reader is not only the ability to utilise all the rhetorical resources studied in the last chapter, but also to gather the many devices, people and animals necessary to produce a visual display usable in a text.

(2) Spokesmen and women

It is important to scrutinise the exact settings in which encounters between authors and dissenters take place. When we disbelieve the scientific literature, we are led from the many libraries around to the very few places where this literature is produced. Here we are welcomed by the author who shows us where the figure in the text comes from. Once presented with the instruments, who does the talking during these visits? At first, the authors: they tell the visitor what to see: 'see the endorphin effect', 'look at the neutrinos!' However, the authors are not lecturing the visitor. The visitors have their faces turned towards the instrument and are watching the place where the thing is writing itself down (inscription in the form of collection of specimens, graphs, photographs, maps – you name it). When the dissenter was reading the scientific text it was difficult for him or her to doubt, but with imagination, shrewdness and downright awkwardness it was always possible. Once in the lab, it is much more difficult because the dissenters see with their own eyes. If we leave aside the many other ways to avoid going through the laboratory that we will study later, the dissenter does not have to believe the paper nor even the scientist's word since in a self-effacing gesture the author has stepped aside. 'See for yourself' the scientist says with a subdued and maybe ironic smile. 'Are you convinced now?' Faced with the thing itself that the technical paper was alluding to, the dissenter now have a choice between either accepting the fact or doubting their own sanity – the latter is much more painful.

We now seem to have reached the end of all possible controversies since there is nothing left for the dissenter to dispute. He or she is right in front of the thing he or she is asked to believe. There is almost no human intermediary between thing and person; the dissenter is in the very place where the thing itself is said to happen and at the very moment when it happens. When such a point is reached it seems that there is no further need to talk of confidence: the thing impresses itself directly on us. Undoubtedly, controversies are settled once and for all when such a situation is set up – which again is very rarely the case. The dissenter becomes a believer, goes out of the lab, borrowing the author's claim and confessing that 'X' has incontrovertibly shown that A is B'. A new fact has been made which will be used to modify the outcome of some other controversies 'see Part B, Section 3).

If this were enough to settle the debate, it would be the end of this book. But... there is someone saying 'but, wait a minute...' and the controversy resumes!

What was imprinted on us when we were watching the guinea pig ileum assay? 'Endorphin of course,' the Professor said. But what did we see? This

![Figure 2.3](image)

Figure 2.3

With a minimum of training we see peaks; we gather there is a base line, and we see a depression in relation to one coordinate that we understand to indicate the time. This is not endorphin yet. The same thing occurred when we paid a visit to Davis's gold and neutrino mine in South Dakota. We saw, he said, neutrinos counted straight out of the huge tank capturing them from the sun. But what did we see? Splurges on paper representing clicks from a Geiger counter. Not neutrinos, yet.

When we are confronted with the instrument, we are attending an 'audio-visual' spectacle. There is a visual set of inscriptions produced by the instrument and a verbal commentary uttered by the scientist. We get both together. The effect on conviction is striking, but its cause is mixed because we cannot differentiate what is coming from the thing inscribed, and what is coming from the author. To be sure, the scientist is not trying to influence us. He or she is simply commenting, underlining, pointing out, dotting the i's and crossing the t's, not adding anything. But it is also certain that the graphs and the clicks by themselves would not have been enough to form the image of endorphin coming out of the brain or neutrinos coming out of the sun. Is this not a strange situation? The scientists do not say anything more than what is inscribed, but without their commentaries the inscriptions say considerably less! There is a word to describe this strange situation, a very important word for everything that follows, that is the word **spokesman** (or **spokeswoman**, or **spokesperson**, or mouthpiece). The author behaves as if he or she were the mouthpiece of what is inscribed on the window of the instrument.

The spokesperson is someone who speaks for others who, or which, do not speak. For instance a shop steward is a spokesman. If the workers were gathered
arguments we have analysed so far. What was the endorphin tried out by the
disserter in Part A, section 3? The superimposition of the traces obtained by:
the sacrificing guinea pig whose gut was then hooked up to electric wires
and regularly stimulated; a hypothalamus soup extracted after many trials from
slaughtered sheep and then forced through HPLC columns under a very high
pressure.

Endorphin, before being named and for as long as it is a new object, is this list
readable on the instruments in the Professor's laboratory. So is a microbe long
before being called such. At first it is something that transforms sugar into
alcohol in Pasteur's lab. This something is narrowed down by the multiplication
of facts it is asked to do. Fermentation still occurs in the absence of air but stops
when air is reintroduced. This exploits a new hero that is killed by air but
breaks down sugar in its absence, a hero that will be called, like the Indians
above, 'Anaerobic' or 'Survivor in the Absence of Air'. Laboratories generate so
many new objects because they are able to create extreme conditions and because
each of these actions is obsessively inscribed.

This naming after what the new object does is in no way limited to actants like
hormones or radioactive substances, that is to the laboratories of what are often
called 'experimental sciences'. Mathematics also defines its subjects by what they
do. When Cantor, the German mathematician, gave a shape to his transfinite
numbers, the shape of his new objects was obtained by having them undergo the
simplest and most radical trial: 14 it is possible to establish a one-to-one
connection between, for instance, the set of points comprising a unit square and
the set of real numbers between 0 and 1. It seems absurd at first since it would
mean that there are as many numbers on one side of a square as in the whole
square. The trial is devised so as to see if two different numbers in the square have
different images on the side or not (thus forming a one-to-one correspondence)
or if they have only one image (thus forming a two-to-one correspondence). The
written answer on the white sheet of paper is incredible: 'I see it but I don't believe
it,' wrote Cantor to Dedekind. There are as many numbers on the side as in the
square. Cantor creates his transfinites from their performance in these extreme,
scarcely conceivable situations.

The act of defining a new object by the answers it inscribes on the window of an
instrument provides scientists and engineers with their final source of strength. It
constitutes our second basic principle, as important as the first in order to
understand science in the making: scientists and engineers speak in the name of
new allies that they have shaped and enrolled; representatives among other
representatives, they add these unexpected resources to tip the balance of force in
their favour. Guillemin now speaks for endorphin and somatostatin, Pasteur for
visible microbes, the Curies for polonium, Payen and Persoz for enzymes, Cantor
for transfinites. When they are challenged, they cannot be isolated, but on the
contrary their constituency stands behind them arrayed in tiers and ready to say
the same thing.

(4) Laboratories against laboratories

Our good friend, the disserter, has now come a long way. He or she is no longer
the shy listener to a technical lecture, the timid onlooker of a scientific
experiment, the polite contradictror. He or she is now the head of a powerful
laboratory utilising all available instruments, forcing the phenomena, supporting
the competitors to support him or her instead, and shaping all sorts of
unexpected objects by imposing harsher and longer trials. The power of this
laboratory is measured by the extreme conditions it is able to create: huge
accelerators of millions of electron volts; temperatures approaching absolute
zero; arrays of radio-telescopes spanning kilometres; furnaces heating up to
thousands of degrees; pressures exerted at thousands of atmospheres; animal
quarters with thousands of rats or guinea pigs; gigantic number crunchers able to
do thousands of operations per millisecond. Each modification of these
conditions allows the disserter to mobilise one more actant. A change from
micro to photogram, from million to billion electron volts; lenses going from
metres to tens of metres; tests going from hundreds to thousands of animals; and
the shape of a new actant is thus redefined. All else being equal, the power of the
laboratory is thus proportionate to the number of actants it can mobilise on its
behalf. At this point, statements are not borrowed, transformed or disputed by
empty-handed laypeople, but by scientists with whole laboratories behind them.

However, to gain the final edge on the opposing laboratory, the disserter must
carry out a fourth strategy: he or she must be able to transform the new objects
into, so to speak, older objects and feed them back into his or her lab.

What makes a laboratory difficult to understand is not what is presently going
on in it, but what has been going on in it and in other labs. Especially difficult to
grasp is the way in which new objects are immediately transformed into
something else. As long as somatostatin, polonium, transfinite numbers, or
anaerobic microbes are shaped by the list of trials I summarised above, it is easy
to relate to them: tell me what you go through and I will tell you what you are.
This situation, however, does not last. New objects become things: 'somatostatin',
'polonium', 'anaerobic microbes', 'transfinite numbers', 'double helix' or 'Eagle
computers', things isolated from the laboratory conditions that shaped them,
things with a name that now seem independent from the trials in which they
proved their mettle. This process of transformation is a very common one and
occurs constantly both for laypeople and for the scientist. All biologists now take
'protein' for an object; they do not remember the time, in the 1920s, when protein
was a whitish stuff that was separated by a new ultracentrifuge in Svedberg's
laboratory. 15 At the time protein was nothing but the action of differentiating cell
contents by a centrifuge. Routine use however transforms the naming of an
actant after what it does into a common name. This process is not mysterious
or special to science. It is the same with the can opener we routinely use in our
kitchen. We consider the opener and the skill to handle it as one black box
which means that it is unproblematic and does not require planning and
attention. We forget the many trials we had to go through (blood, scars, spilled beans and ravioli, shouting parent) before we handled it properly, anticipating the weight of the can, the reactions of the opener, the resistance of the tin. It is only when watching our own kids still learning it the hard way that we might remember how it was when the can opener was a 'new object' for us, defined by a list of trials so long that it could delay dinner for ever.

This process of routinisation is common enough. What is less common is the way the same people who constantly generate new objects to win in a controversy are also constantly transforming them into relatively older ones in order to win still faster and irreversibly. As soon as somatostatin has taken shape, a new bioassay is devised in which somatostatin takes the role of a stable, unproblematic substance in a trial set up for tracking down a new problematic substance, GRF. As soon as Svedberg has defined protein, the ultracentrifuge is made a routine tool of the laboratory bench and is employed to define the constituents of proteins. No sooner has polonium emerged from what it did in the list of ordeals above than it is turned into one of the well-know radioactive elements with which one can design an experiment to isolate a new radioactive substance further down in Mendeleev's table. The list of trials becomes a thing; it is literally reified.

This process of reification is visible when going from new objects to older ones, but it is also reversible although less visible when going from younger to older ones. All the new objects we analysed in the section above were framed and defined by stable black boxes which had earlier been new objects before being similarly reified. Endorphin was made visible in part because the ileum was known to go on pulsating long after guinea pigs are sacrificed: what was a new object several decades earlier in physiology was one of the black boxes participating in the endorphin assay, as was morphine itself. How could the new unknown substance have been comparèd if morphine had not been known? Morphine, which had been a new object defined by its trials in Seguin's laboratory sometime in 1804, was used by Guillemin in conjunction with the guinea pig ileum to set up the conditions defining endorphin. This also applies to the physiograph, invented by the French physiologist Marey at the end of the nineteenth century. Without it, the transformation of gut pulsation would not have been made graphically visible. Similarly for the electronic hardware that enhanced the signals and made them strong enough to activate the physiograph stylus. Decades of advanced electronics during which many new phenomena had been devised were mobilised here by Guillemin to make up another part of the assay for endorphin. Any new object is thus shaped by simultaneously importing many older ones in their reified form. Some of the imported objects are from young or old disciplines or pertain to harder or softer ones. The point is that the new object emerges from a complex set-up of sedimented elements each of which has been a new object at some point in time and space. The genealogy and the archaeology of this sedimented past is always possible in theory but becomes more and more difficult as time goes by and the number of elements mustered increases.

It is just as difficult to go back to the time of their emergence as it is to contest them. The reader will have certainly noticed that we have gone full circle from the first section of this part (borrowing more black boxes) to this section (blackboxing more objects). It is indeed a circle with a feedback mechanism that creates better and better laboratories by bringing in as many new objects as possible in as reified a form as possible. If the dissenters quickly re-import somatostatin, endorphin, polonium, transfinite numbers as so many uncontroversial black boxes, his or her opponent will be made all the weaker. His or her ability to dispute will be decreased since he or she will now be faced with piles of black boxes, obliged to untie the links between more and more elements coming from a more and more remote past, from harder disciplines, and presented in a more reified form. Has the shift been noticed? It is now the author who is weaker and the dissenters stronger. The author must now either build a better laboratory in order to dispute the dissenters' claim and tip the balance of power back again, or quit the game – or apply one of the many tactics to escape the problem altogether that we will see in the second part of this book. The endless spiral has travelled one more loop. Laboratories grow because of the number of elements fed back into them, and this growth is irreversible since no dissenters/author is able to enter into the fray later with fewer resources at his or her disposal – everything else being equal. Beginning with a few cheap elements borrowed from common practice, laboratories end up after several cycles of contest with costly and enormously complex set-ups very remote from common practice.

The difficulty of grasping what goes on inside their walls thus comes from the sediment of what has been going on in other laboratories earlier in time and elsewhere in space. The trials currently being undergone by the new object they give shape to are probably easy to explain to the layperson – and we are all laypeople so far as disciplines other than our own are concerned – but the older objects capitalised in the many instruments are not. The layman is awed by the laboratory set-up, and rightly so. There are not many places under the sun where so many and such hard resources are gathered in so great numbers, sedimented in so many layers, capitalised on such a large scale. When confronted earlier by the technical literature we could brush it aside; confronted by laboratories we are simply and literally impressed. We are left without power, that is, without resource to contest, to reopen the black boxes, to generate new objects, to dispute the spokesmen's authority.

Laboratories are now powerful enough to define reality. To make sure that our travel through technoscience is not stifled by complicated definitions of reality, we need a simple and sturdy one able to withstand the journey: reality as the Latin word *res* indicates, is what resists. What does it resist? *Trials of strength.* If, in a given situation, no dissenters is able to modify the shape of a new object, then that's it, it is reality, at least for as long as the trials of strength are not modified. In the examples above so many resources have been mobilised in the last two chapters by the dissenters to support these claims that, we must admit, resistance will be vain: the claim has to be true. The minute the contest stops, the minute I
write the word ‘true’, a new, formidable ally suddenly appears in the winner’s camp, an ally invisible until then, but behaving now as if it had been there all along: Nature.

Part C
Appealing to Nature

Some readers will think that it is about time I talked of Nature and the real objects behind the texts and behind the labs. But it is not I who am late in finally talking about reality. Rather, it is Nature who always arrives late, too late to explain the rhetoric of scientific texts and the building of laboratories. This beloved, sometimes faithful and sometimes fickle ally has complicated the study of technoscience until now so much that we need to understand it if we wish to continue our travel through the construction of facts and artefacts.

(1) ‘Natur mit uns’

‘Belated?’ ‘Fickle?’ I can hear the scientists I have shadowed so far becoming incensed by what I have just written. ‘All this is ludicrous because the reading and the writing, the style and the black boxes, the laboratory set-ups – indeed all existing phenomena – are simply means to express something, vehicles for conveying this formidable ally. We might accept these ideas of “inscriptions”, your emphasis on controversies, and also perhaps the notions of “ally”, “new object”, “actor” and “supporter”, but you have omitted the only important one, the only supporter who really counts, Nature herself. Her presence or absence explains it all. Whoever has Nature in their camp wins, no matter what the odds against them are. Remember Galileo’s sentence, “1000 Demosthenes and 1000 Aristotles may be routed by any average man who brings Nature in.” All the flowers of rhetoric, all the clever contraptions set up in the laboratories you describe, all will be dismantled once we go from controversies about Nature to what Nature is. The Goliath of rhetoric with his laboratory set-up and all his attendant Philistines will be put to flight by one David alone using simple truths about Nature in his slingshot! So let us forget all about what you have been writing for a hundred pages – even if you claim to have been simply following us – and let us see Nature face to face!

Is this not a refreshing objection? It means that Galileo was right after all. The dreadnoughts I studied in Chapters 1 and 2 may be easily defeated in spite of the many associations they knit, weave and knot. Any dissenter has got a chance. When faced with so much scientific literature and such huge laboratories, he or she has just to look at Nature in order to win. It means that there is a supplement, something more which is nowhere in the scientific papers and nowhere in the labs which is able to settle all matters of dispute. This objection is all the more refreshing since it is made by the scientists themselves, although it is clear that this reification of the average woman or man, of Ms or Mr Anybody, is also an indictment of these crowds of allies mustered by the same scientists.

Let us accept this pleasant objection and see how the appeal to Nature helps us to distinguish between, for instance, Schally’s claim about GHRH and Guillemin’s claim about GRF. They both wrote convincing papers, arraying many resources with talent. One is supported by Nature – so his claim will be made a fact – and the other is not – it ensues that his claim will be turned into an artefact by the others. According to the above objections, readers will find it easy to give the casting vote. They simply have to see who has got Nature on his side.

It is just as easy to separate the future of fuel cells from that of batteries. They both contend for a slice of the market; they both claim to be the best and most efficient. The potential buyer, the investor, the analyst are lost in the mist of a controversy, reading stacks of specialised literature. According to the above objection, their life will now be easier. Just watch to see on whose behalf Nature will talk. It is as simple as in the struggles sung in the libretto: wait for the goddess to tip the balance in favour of one camp or the other.

A fierce controversy divides the astrophysicists who calculate the number of neutrinos coming out of the sun and Davis, the experimentalist who obtains a much smaller figure. It is easy to distinguish them and put the controversy to rest. Just let us see for ourselves in which camp the sun is really to be found. Somewhere the natural sun with its true number of neutrinos will close the mouths of dissenters and force them to accept the facts no matter how well written these papers were.

Another violent dispute divides those who believe dinosaurs to have been cold-blooded (lazy, heavy, stupid and sprawling creatures) and those who think that dinosaurs were warm-blooded (swift, light, cunning and running animals). If we support the objection, there would be no need for the ‘average man’ to read the piles of specialised articles that make up this debate. It is enough to wait for Nature to sort them out. Nature would be like God, who in medieval times judged between two disputants by letting the innocent win.

In these four cases of controversy generating more and more technical papers and bigger and bigger laboratories or collections, Nature’s voice is enough to stop the noise. Then the obvious question to ask, if I want to do justice to the objection above, is ‘what does Nature say?’

Schally knows the answer pretty well. He told us in his paper, GHRH is this amino-acid sequence, not because he imagined it, or made it up, or confused a piece of haemoglobin for this long-sought-after hormone, but because this is what the molecule is in Nature, independently of his wishes. This is also what Guillemin says, not of Schally’s sequence which is a mere artefact, but of his substance, GRF. There is still doubt as to the exact nature of the real hypothalamic GRF compared with that of the pancreas, but on the whole it is certain that GRF is indeed the amino-acid sequence cited in Chapter 1. Now, we have got a problem. Both contenders have Nature in their camp and say what it
says. Hold it! The challengers are supposed to be refereed by Nature, and not to start another dispute about what Nature’s voice really said.

We are not going to be able to stop this new dispute about the referee, however, since the same confusion arises when fuel cells and batteries are opposed. ‘The technical difficulties are not insurmountable,’ say the fuel cell’s supporters. It’s just that an infinitesimal amount has been spent on their research compared to the internal combustion engine’s. Fuel cells are Nature’s way of storing energy; give us more money and you’ll see.’ Wait, wait! We were supposed to judge the technical literature by taking another outsider’s point of view, not to be driven back inside the literature and deeper into laboratories.

Yet it is not possible to wait outside, because in the third example also, more and more papers are pouring in, disputing the model of the sun and modifying the number of neutrinos emitted. The real sun is alternately on the side of the theoreticians when they accuse the experimentalists of being mistaken and on the side of the latter when they accuse the former of having set up a fictional model of the sun’s behaviour. This is too unfair. The real sun was asked to tell the two contenders apart, not to become yet another bone of contention.

More bones are to be found in the paleontologists’ dispute where the real dinosaur has problems about giving the casting vote. No one knows for sure what it was. The ordeal might end, but is the winner really innocent or simply stronger or luckier? Is the warm-blooded dinosaur more like the real dinosaur, or is it just that its proponents are stronger than those of the cold-boned one? We expected a final answer by using Nature’s voice. What we got was a new fight over the composition, content, expression and meaning of that voice. That is, we get more technical literature and larger collections in bigger Natural History Museums, not less; more debates and less.

I interrupt the exercise here. It is clear by now that applying the scientists’ objection to any controversy is like pouring oil on a fire, it makes it flare anew. Nature is not outside the fighting camps. She is, much like God in not-so-ancient wars, asked to support all the enemies at once. ‘Natur mit uns’ is embroidered on all the banners and is not sufficient to provide one camp with the winning edge. So what is sufficient?

(2) The double-talk of the two-faced Janus

I could be accused of having been a bit disingenuous when applying scientists’ objections. When they said that something more than association and numbers is needed to settle a debate, something outside all our human conflicts and interpretations, something they call ‘Nature’ for want of a better term, something that eventually will distinguish the winners and the losers, they did not mean to say that we know what it is. This supplement beyond the literature and laboratory trials is unknown and this is why they look for it, call themselves ‘researchers’, write so many papers and mobilise so many instruments.

‘It is ludicrous,’ I hear them arguing, ‘to imagine that Nature’s voice could stop Guillenin and Schally from fighting, could reveal whether fuel cells are superior to batteries or whether Watson and Crick’s model is better than that of Pauling. It is absurd to imagine that Nature, like a goddess, will visibly tip the scale in favour of one camp or that the Sun God will barge into an astrophysics meeting to drive a wedge between theoreticians and experimentalists; and still more ridiculous to imagine real dinosaurs invading a Natural History Museum in order to be compared with their plaster models! What we meant, when contesting your obsession with rhetoric and mobilisation of black boxes, was that once the controversy is settled, it is Nature the final ally that has settled it and not any rhetorical tricks and tools or any laboratory contraptions.’

If we still wish to follow scientists and engineers in their construction of technoscience, we have got a major problem here. On the one hand scientists herald Nature as the only possible adjudicator of a dispute, on the other they recruit countless allies while waiting for Nature to declare herself. Sometimes David is able to defeat all the Philistines with only one slingshot; at other times, it is better to have swords, chariots and many more, better drilled soldiers than the Philistines!

It is crucial for us, laypeople who want to understand technoscience, to decide which version is right, because in the first version, as Nature is enough to settle all disputes, we have nothing to do since no matter how large the resources of the scientists are, they do not matter in the end — only Nature matters. Our chapters may not be all wrong, but they become useless since they merely look at trifles and addenda and it is certainly no use going on for four other chapters to find still more trivia. In the second version, however, we have a lot of work to do since, by analysing the allies and resources that settle a controversy we understand everything that there is to understand in technoscience. If the first version is correct, there is nothing for us to do apart from catching the most superficial aspects of science; if the second version is maintained, there is everything to understand except perhaps the most superficial and flashy aspects of science. Given the stakes, the reader will realise why this problem should be tackled with caution. The whole book is in jeopardy here. The problem is made all the more tricky since scientists simultaneously assert the two contradictory versions, displaying an ambivalence which could paralyse all our efforts to follow them.

We would indeed be paralysed, like most of our predecessors, if we were not used to this double-talk or the two-faced Janus (see introduction). The two versions are contradictory but they are not uttered by the same face of Janus. There is again a clear-cut distinction between what scientists say about the cold settled part and about the warm unsettled part of the research front. As long as controversies are rife, Nature is never used as the final arbiter since no one knows what she is and says. But once the controversy is settled, Nature is the ultimate referee.

This sudden inversion of what counts as referee and what counts as being refereed, although counter-intuitive at first, is as easy to grasp as the rapid
passage from the 'name of action' given to a new object to when it is given its name as a thing (see above). As long as there is a debate among endocrinologists about GRF or GHRH, no one can intervene in the debates by saying, 'I know what it is, Nature told me so. It is that amino-acid sequence.' Such a claim would be greeted with derisive shouts, unless the proponent of such a sequence is able to show his figures, cite his references, and quote his sources of support, in brief, write another scientific paper and equip a new laboratory, as in the case we have studied. However, once the collective decision is taken to turn Schally's GHRH into an artefact and Guillemin's GRF into an incontrovertible fact, the reason for this decision is not imputed to Guillemin, but is immediately attributed to the independent existence of GRF in Nature. As long as the controversy lasted, no appeal to Nature could bring any extra strength to one side in the debate (it was at best an invocation, at worst a bluff). As soon as the debate is stopped, the supplement of force offered by Nature is made the explanation as to why the debate did stop (and why the bluff, the frauds and the mistakes were at last unmasked).

So we are confronted with two almost simultaneous suppositions:

Nature is the final cause of the settlement of all controversies, once controversies are settled.

As long as they last Nature will appear simply as the final consequence of the controversies.

When you wish to attack a colleague's claim, criticise a world-view, modalise a statement you cannot just say that Nature is with you; 'just' will never be enough. You are bound to use other allies besides Nature. If you succeed, then Nature will be enough and all the other allies and resources will be made redundant. A political analogy may be of some help at this point. Nature, in scientists' hands, is a constitutional monarch, much like Queen Elizabeth the Second. From the throne she reaets with the same tone, majesty and conviction, a speech written by Conservative or Labour prime ministers depending on the election outcome. Indeed she adds something to the dispute, but only after the dispute has ended; as long as the election is going on she does nothing but wait.

This sudden reversal of scientists' relations to Nature and to one another is one of the most puzzling phenomena we encounter when following their trails. I believe that it is the difficulty of grasping this simple reversal that has made technoscience so hard to probe until now.

The two faces of Janus talking together make, we must admit, a startling spectacle. On the left side Nature is cause, on the right side consequence of the end of controversy. On the left side scientists are realists, that is they believe that representations are sorted out by what really is outside, by the only independent referee there is. Nature. On the right side, the same scientists are relativists, that is, they believe representations to be sorted out among themselves and the actants they represent, without independent or impartial referees lending their weight to any one of them. We know why they talk two languages at once: the left mouth speaks about settled parts of science, whereas the right mouth talks about unsettled parts. On the left side polonium was discovered long ago by the Curies; on the right side there is a long list of actions effected by an unknown actant in Paris at the Ecole de Chimie which the Curies propose to call 'polonium'. On the left side all scientists agree, and we hear only Nature's voice, plain and clear; on the right side scientists disagree and no voice can be heard over theirs.

Figure 2.5

(3) The third rule of method

If we wish to continue our journey through the construction of facts, we have to adapt our method to scientists' double-talk. If not, we will always be caught on the wrong foot: unable to withstand either their first (realist) or their second (relativist) objection. We will then need to have two different discourses depending on whether we consider a settled or an unsettled part of technoscience. We too will be relativists in the latter case and realists in the former. When studying controversy - as we have so far - we cannot be less relativist than the very scientists and engineers we accompany; they do not use Nature as the external referee, and we have no reason to imagine that we are more clever than they are. For these parts of science our third rule of method will read: since the settlement of a controversy is the cause of Nature's representation not the consequence, we can never use the outcome - Nature - to explain how and why a controversy has been settled.

This principle is easy to apply as long as the dispute lasts, but is difficult to bear in mind once it has ended, since the other face of Janus takes over and does the talking. This is what makes the study of the past of technoscience so difficult and unrewarding. You have to hang onto the words of the right face of Janus - now barely audible - and ignore the clamours of the left side. It turned out for instance that the N-rays were slowly transformed into artefacts much like Schally's GHRH. How are we going to study this innocent expression 'it turned out'?
Using the physics of the present day there is unanimity that Blondlot was badly mistaken. It would be easy enough for historians to say that Blondlot failed because there was 'nothing really behind his N-rays' to support his claims. This way of analysing the past is called Whig history, that is, a history that crowns the winners, calling them the best and the brightest and which says the losers like Blondlot lost simply because they were wrong. We recognise here the left side of Janus' way of talking where Nature herself discriminates between the bad guys and the good guys. But, is it possible to use this as the reason why in Paris, in London, in the United States, people slowly turned N-rays into an artefact? Of course not, since at that time today's physics obviously could not be used as the touchstone, or more exactly since today's state is, in part, the consequence of settling many controversies such as the N-rays!

Whig historians had an easy life. They came after the battle and needed only one reason to explain Blondlot's demise. He was wrong all along. This reason is precisely what does not make the slightest difference while you are searching for truth in the midst of a polemic. We need, not one, but many reasons to explain how a dispute stopped and a black box was closed.\(^1\)

However, when talking about a cold part of technoscience we should shift our method like the scientists themselves who, from hard-core relativists, have turned into dyed-in-the-wool realists. Nature is now taken as the cause of accurate descriptions of herself. We cannot be more relativist than scientists about these parts and keep on denying evidence where no one else does. Why? Because the cost of dispute is too high for an average citizen, even if he or she is a historian and sociologist of science. If there is no controversy among scientists as to the status of facts, then it is useless to go on talking about interpretation, representation, a biased or distorted world-view, weak and fragile pictures of the world, unfaithful spokespersons. Nature talks straight, facts are facts. Full stop. There is nothing to add and nothing to subtract.

This division between relativists and realist interpretation of science has caused analysts of science to be put off balance. Either they went on being relativists even about the settled parts of science—which made them look ludicrous; or they continued being realists even about the warm uncertain parts—and they made fools of themselves. The third rule of method stated above should help us in our study because it offers us a good balance. We do not try to undermine the solidity of the accepted parts of science. We are realists as much as the people we travel with and as much as the left side of Janus. But as soon as a controversy starts we become as relativist as our informants. However we do not follow them passively because our method allows us to document both the construction of fact and of artefact, the cold and the warm, the demonised and the modulated statements, and, in particular, it allows us to trace with accuracy the sudden shifts from one face of Janus to the other. This method offers us, so to speak, a stereophonic rendering of fact-making instead of its monophonic predecessors!
CHAPTER 3

Machines

Introduction: The quandary of the fact-builder

In the first part of this book we have learned how to travel through technoscience without being intimidated either by the technical literature or by the laboratories. When any controversy heats up, we know how to follow the accumulation of papers and how to take our bearings through the laboratories that stand behind the papers. To acquire this knowledge, though, we had to pay a price which can be summed up by the three principles of method I presented: first we had to give up any discourse or opinion about science as it is made, and follow scientists in action instead; second, we had to give up any decision about the subjectivity or the objectivity of a statement based simply on the inspection of this statement, and we had to follow its tortuous history instead, as it went from hand to hand, everyone transforming it into more of a fact or more of an artefact; finally, we had to abandon the sufficiency of Nature as our main explanation for the closure of controversies, and we had instead to count the long heterogeneous list of resources and allies that scientists were gathering to make dissent impossible.

The picture of technoscience revealed by such a method is that of a weak rhetoric becoming stronger and stronger as time passes, as laboratories get equipped, articles published and new resources brought to bear on harder and harder controversies. Readers, writers and colleagues are forced either to give up, to accept propositions or to dispute them by working their way through the laboratory again. These three possible outcomes could be explored in much more detail by more studies of the scientific literature and laboratories.1 These studies however, no matter how necessary, would not overcome one of the main limitations of the first part of this book: dissenters are very rarely engaged in a confrontation such that, everything else being equal, the winner is the one with the bigger laboratory or the better article. For the sake of clarity, I started with the three outcomes above as if technoscience was similar to a boxing match. There is, in practice, a fourth set of outcomes, which is much more common: everything
not being equal, it is possible to win with many other resources than articles and laboratories. It is possible, for instance, never to encounter any dissenter, never to interest anyone, never to accept the superior strength of the others. In other words, the possession of many strongholds has first to be secured for the stronger rhetoric of science to gain any strength at all.

To picture this preliminary groundwork we have to remember our first principle: the fate of a statement depends on others' behaviour. You may have written the definitive paper proving that the earth is hollow and that the moon is made of green cheese but this paper will not become definitive if others do not take it up and use it as a matter of fact later on. You need them to make your paper a decisive one. If they laugh at you, if they are indifferent, if they shrug it off, that is the end of your paper. A statement is thus always in jeopardy, much like the ball in a game of rugby. If no player takes it up, it simply sits on the grass. To have it move again you need an action, for someone to seize and throw it; but the throw depends in turn on the hostility, speed, deftness or tactics of the others. At any point, the trajectory of the ball may be interrupted, deflected or diverted by the other team—playing here the role of the dissenters—and interrupted, deflected or diverted by the players of your own team. The total movement of the ball, of a statement, of an artefact, will depend to some extent on your action but to a much greater extent on that of a crowd over which you have little control. The construction of facts, like a game of rugby, is thus a collective process.

Each element in the chain of individuals needed to pass the black box along may act in multifarious ways: the people in question may drop it altogether, or accept it as it is, or shift the modalities that accompany it, or modify the statement, or appropriate it and put it in a completely different context. Instead of being conductors, or semi-conductors, they are all multi-conductors, and unpredictable ones at that. To picture the task of someone who wishes to establish a fact, you have to imagine a chain of the thousands of people necessary to turn the first statement into a black box and where each of them may or may not unpredictably transmit the statement, modify it, alter it or turn it into an artefact. How is it possible to master the future fate of a statement that is the outcome of the behaviour of all these faithless allies?

This question is all the more difficult since all the actors are doing something to the black box. Even in the best of cases they do not simply transmit it, but add elements of their own by modifying the argument, strengthening it and incorporating it into new contexts. The metaphor of the rugby game soon breaks down since the ball remains the same—apart from a few abrasions—all along, whereas in this technoscience game we are watching, the object is modified as it goes along from hand to hand. It is not only collectively transmitted from one actor to the next, it is collectively composed by actors. This collective action then raises two more questions. To whom can the responsibility for the game be attributed? What is the object that has been passed along?

An example will make the fact-builder's problem easier to grasp. Diesel is known as the father of the diesel engine. This fatherhood, however, is not as direct as that of Athena from Zeus' head. The engine did not emerge one morning from Diesel's mind. What emerged was an idea of a perfect engine working according to Carnot's thermodynamic principles. This was an engine where ignition could occur without an increase in temperature, a paradox that Diesel solved by inventing new ways of injecting and burning fuel. At this point in the story, we have a book he published and a patent he took out; thus, we have a paper world similar to those we studied earlier. A few reviewers, including Lord Kelvin, were convinced while others found the idea impracticable.

Diesel is now faced with a problem. He needs others to transform the two-dimensional project and patent into the form of a three-dimensional working prototype. He ferrets out a few firms that build machines—Maschinenfabrik Augsburg-Nürnberg known as MAN, and Krupp, which are interested because of the hope of increased efficiency and versatility of a perfect Carnot machine, the efficiency of the steam engine in the 1890s being pitifully low. As we will see, reality has many hues, like objectivity, and entirely depends on the number of elements tied to the claim. For four years, Diesel tried to get one engine working, building it with the help of a few engineers and machine tools from MAN. The progressive realisation of the engine was made by importing all available resources into the workshop, just as in any laboratory. The skills and tools for making pistons and valves were the result of thirty years of practice at MAN and were all locally available as a matter of routine. The question of fuel combustion soon turned out to be more problematic, since air and fuel have to be mixed in a fraction of a second. A solution entailing confined air injection was found, but this required huge pumps and new cylinders for the air; the engine became large and expensive, unable to compete in the market of small versatile engines. By modifying the whole design of the engine many times, Diesel drifted away from the original patent and from the principles presented in his book.

The number of elements tied to Diesel's engine is increasing. First, we had Carnot's thermodynamics plus a book plus a patent plus Lord Kelvin's encouraging comments. We now have in addition MAN plus Krupp plus a few prototypes plus two engineers helping Diesel plus local know-how plus a few interested firms plus a new air injection system, and so on. The second series is much larger, but the perfect engine of the first has been transformed in the process; in particular, constant temperature has been abandoned. It is now a constant pressure engine and in a new edition of his book Diesel has to struggle to reconcile the drift from the first more 'theoretical' engine to the one being slowly realised.

But how real is real? In June 1897 the engine is solemnly presented to the public. The worries of a black box builder now take on a new dimension. Diesel needs others to take up his engine and to turn it into a black box that runs smoothly in thousands of copies all over the world, incorporated as an unproblematic element in factories, ships and lorries. But what are these others going to do with it? How much should the prototype be transformed before being transferred from Augsburg to Newcastle, Paris or Chicago? At first, Diesel thinks
black box safely concealed beneath the bonnet of a car, activated at the turn of a key by a driver who does not have to know anything about Carnot's thermodynamics. MAN's know-how or Diesel's suicide?

A series of terms are traditionally used to tell these stories. First, one may consider that all diesel engines lie along one trajectory going through different phases from ideas to market. These admittedly fuzzy phases are then given different names. Diesel's idea of a perfect engine in his mind is called invention. But since, as we saw, the idea needs to be developed into a workable prototype, this new phase is called development — hence the expression Research and Development that we will see in Chapter 4. Innovation is often the word used for the next phase through which a few prototypes are prepared so as to be copied in thousands of exemplars sold throughout the world.

However, these terms are of no great use. Right from the start, Diesel had an overall notion not only of his engine, but also of the economic world in which it should work, of the way to sell licenses, of the organization of the research, of the companies to be set up to build it. In another book Diesel even designed the type of society, based on solidarity, that would be best fit for the sort of technical novelties he wished to introduce. So no clear-cut distinction may be made between invention and innovation. In 1897 the MAN manager, Diesel and the first investors all thought that development had ended and that innovation was starting, even though it took ten more years to reach such a stage, and in the meantime Diesel went bankrupt. Thus this distinction between phases is not immediately given. On the contrary, making separations between the phases and enforcing them is one of the inventor's problems: is the black box really black? When is the dissenting going to stop? Can I now find believers and buyers? Finally, it is not even sure that the first invention should be sought in Diesel's own mind. Hundreds of engineers were looking for a more efficient combustion engine at the same time. The first flash of intuition might not be in one mind, but in many minds.

If the notion of discrete phases is useless, so, too, is that of trajectory. It does not describe anything since it is again one of the problems to be solved. Diesel indeed claimed that there was one trajectory which links his seminal patent to real engines. This is the only way for his patents to be 'seminal'. But this was disputed by hundreds of engineers claiming that the engine's ancestry was different. Anyway, if Diesel was so sure of his offspring, then why not call it a Carnot engine since it is from Carnot that he took the original idea? But since the original patent never worked, why not call it a MAN engine, or, a constant pressure air injection engine? We see that talking of phases in a trajectory is like taking slices from a pâté made from hundreds of morsels of meat. Although it might be palatable, it has no relation whatsoever to the natural joints of the animal. To use another metaphor, employing these terms would be like watching a rugby game on TV where only a phosphorescent ball was shown. All the running, the cunning, the excited players would be replaced by a meaningless zigzagging spot.
No matter how clumsy these traditional terms are in describing the building of facts, they are useful in accounting, that is for measuring how much money and how many people are invested (as we will see in the next chapter). From invention to development and from there to innovation and sale, the money to be invested increases exponentially, as does the time to be spent on each phase and the number of people participating in the construction. The spread in space and time of black boxes is paid for by a fantastic increase in the number of elements to be tied together. Bragg, Diesel or West (see Introduction) may have quick and cheap ideas that keep a few collaborators busy for a few months. But to build an engine or a computer for sale, you need more people, more time, more money. The object of this chapter is to follow this dramatic increase in numbers.

This increase in numbers is necessarily linked to the problem of the fact-builder: how to spread out in time and space. If Schally is the only person who believes in GRF, then GRF remains in one place in New Orleans, under the guise of a lot of words in an old reprint. If Diesel is the only person who believes in his perfect engine, the engine sits in an office drawer in Augsburg. In order to spread in space and time to become long-lasting they all need (we all need) the actions of others. But what are these actions be? Many things, most of them unpredictable, which will transform the transported object or statement. So we are now in a quandary: either the others will not take up the statement or they will. If they don’t, the statement will be limited to a point in time and space, myself, my dreams, my fantasies. But if they do take it up, they might transform it beyond recognition.

To get out of this quandary we need to do two things at once:

- to enrol others so that they participate in the construction of the fact;
- to control their behaviour in order to make their actions predictable.

At first sight, this solution seems so contradictory as to look unfeasible. If others are enrolled they will transform the claims beyond recognition. Thus the very action of involving them is likely to make control more difficult. The solution to this contradiction is the central notion of translation. I will call translation the interpretation given by the fact-builders of their interests and that of the people they enrol. Let us look at these strategies in more detail.

Part A
Translating interests

(1) Translation one: I want what you want

We need others to help us transform a claim into a matter of fact. The first and easiest way to find people who will immediately believe the statement, invest in the project, or buy the prototype, is to tailor the object in such a way that it caters for these people’s explicit interests. As the name ‘interesse’ indicates, ‘interests’ are what lie in between actors and their goals, thus creating a tension that will make actors select only what, in their own eyes, helps them reach these goals amongst many possibilities. In the preceding chapters, for instance, we saw many contenders engaged in polemics. In order to resist their opponents’ challenges they needed to fasten their position to less controvertible arguments, to simpler black boxes, to less disputable fields, gathering around themselves huge and efficient laboratories. If you were able to provide a contender with one of these black boxes, it is likely it will be eagerly seized and more rapidly transformed into a fact. Suppose, for instance, that while Diesel tinkers with his prototype, someone comes along with a new instrument that depicts on a simple indicator card how pressure changes with changing volume as the piston moves inside the cylinder so that the area on the diagram measures the work done. Diesel will jump at it, because it offers a neater way of ‘seeing’ how the invisible piston moves and because it graphically depicts, for everyone to see, that his engine covers a larger area than any other. The point is that, by borrowing the indicator card in order to further his goals, Diesel lends his force to its inventor, fulfilling the latter’s goals. The more such elements Diesel is able to link himself to, the more likely he is to transform his own prototype into a working engine. But this movement does the same for the indicator card, which now becomes a routine part of the testing bench. The two interests are moving in the same direction.

Suppose, to take another example, that Boas, the American anthropologist, is engaged in a fierce controversy against eugenicists, who have so convinced the United States Congress of biological determinism that it has cut off the immigration of those with ‘defective’ genes. Suppose, now, that a young anthropologist demonstrates that, at least in one Samoan island, biology cannot be the cause of crisis in adolescent girls because cultural determinism is too strong. Is not Boas going to be ‘interested’ in Mead’s report – all the more so since he sent her there? Every time eugenicists criticise his cultural determinism, Boas will fasten his threatened position to Mead’s counter-example. But every time Boas and other anthropologists do so, they turn Mead’s story into more of a fact. You may imagine Mead’s report interesting nobody, being picked up by no one, and remaining for ever in the (Pacific) limbo. By linking her thesis to Boas’s struggle, Mead forces all the other cultural determinists to become her fellow builders: they all willingly turn her claims into one of the hardest facts of anthropology for many decades. When Freeman, another anthropologist, wished to undermine Mead’s fact, he also had to link his struggle to a wider one, that of the sociobiologists. Until then, every time the sociobiologists fought against cultural determinism, they stumbled against this fact of Mead’s, which had been made formidable by the collective action of successive generations of anthropologists. Sociobiologists eagerly jumped at Freeman’s thesis since it allowed them to get rid of this irritating counter-example, and lent him their formidable forces (their publishing firms, their links with the media). With their help what could have been a ‘ludicrous attack’ became ‘a courageous revolution’ that threatened to destroy Mead’s reputation.

As I stress in Chapter 2, none of these borrowings will be enough alone to stop
the controversy: people may contest the indicator card borrowed by Diesel, or Mead’s report, or Freeman’s ‘courageous revolution’. The point here is that the easiest means to enrol people in the construction of facts is to let oneself be enrolled by them! By pushing their explicit interests, you will also further yours. The advantage of this piggy-back strategy is that you need no other force to transform a claim into a fact; a weak contender can thus profit from a vastly stronger one.

Figure 3.1

There are disadvantages as well. First, since so many people are helping you to build your claim, how will your own contribution be evaluated? Will it not be made marginal? Or worse, will it not be appropriated by others who say they did most of the work, as happened with Diesel? Second, since the contenders are the ones who have to go out of their way to follow the direction of the others (see Figure 3.1, Translation 1) they have no control on what the crowd they follow is going to do with their claims. This is especially difficult when others are so easily convinced that they turn your tentative statements into claims of gigantic size. When Pasteur elaborated a vaccine against fowl cholera that cured a few hens, he interested so many powerful groups of health officers, veterinary surgeons and farm interests that they jumped to the conclusion that ‘this was the beginning of the end of all infectious diseases in men and animals’.4 This new claim was a composition made in small measure from Pasteur’s study of a few hens and in much larger measure from the interests of the enrolled groups. The proof that this extension was not due to Pasteur’s study but rather to separate interests is that many other professions that Pasteur had not yet succeeded in interesting – the average physician for instance – found the very same experiments to be deficient, doubtful, premature and inconclusive.

Riding piggy-back is thus precarious: sometimes you have to overcome the indifference of the other groups (they refuse to believe you and to lend you their forces), and sometimes you have to restrain their sudden enthusiasm. For instance, one of the people who was not convinced by Pasteur was Koch, his German rival. But later in his career Koch had to give a lecture at the 1890 International Medical Association meeting in Berlin. He had been so successful in interesting everyone in his study of tuberculosis, so clever in linking his science to the nationalism of Kaiser William, that everyone was ready to believe him. So ready indeed that when during his speech he alluded to a possible vaccine against tuberculosis everyone heard him saying that he had his vaccine. Everyone jumped to their feet and applauded frantically and Koch, puzzled by this collective transformation of his claim into a fact, did not dare say that he had not got a vaccine at all. When patients with tuberculosis flocked to Berlin for injections, they were bitterly disappointed, because Koch could not deliver on his ostensible promise... Catering to other explicit interests is not a safe strategy. There must be better ways.

(2) Translation two: I want it, why don’t you?

It would be much better if the people mobilised to construct our claims were to follow us rather than the other way around. A good idea indeed, but there seems to be no reason on earth why people should go out of their way and follow yours instead (Figure 3.1, Translation 2) especially if you are small and powerless while they are strong and powerful. In fact, there is only one reason: it is if their usual way is cut off.

For instance, a rich businessman with an interest in philosophy wishes to establish a Foundation to study the origins of logical abilities in man. His pet project is to have scientists discover the specific neurons for induction and deduction. Talking to scientists he soon realises that they consider his dream as premature, they cannot help him reach his goal yet: but they nevertheless ask him to invest his money – now without a goal – into their research. He then opens a private Foundation where people study neurons, children’s behaviour, rats in mazes, monkeys in tropical forests and so on... Scientists do what they want with his money, and not what he wanted.

This strategy, as you may see from Figure 3.1 is symmetrical with the former. The millionaire, shifting his interests, takes up those of the scientists. Such a displacement of explicit interest is not very feasible and is rare. Something else is needed to make it practical.

(3) Translation three: if you just make a short detour...

Since the second strategy is only rarely possible, a much more powerful one needs to be devised, as irresistible as the advice of the serpent to Eve: ‘You cannot reach your goal straight away, but if you come my way, you will reach it faster, it would be a short cut.’ In this new rendering of others’ interests, the contenders do not try to shift them away from their goals. They simply offer to guide them through a short cut. This is appealing if three conditions are fulfilled: the main
road is clearly cut off: the new detour is well signposted: the detour appears short.

The brain scientists would never have answered in the way I suggested when
probed by the businessman above. On the contrary, they would have argued that
the millionaire’s goal is indeed attainable, but not right now. A little detour
through their neurology is necessary for a few years before the neurons of
induction and deduction which he is aiming at are eventually discovered. If he
agrees to finance studies on acetylcholine behaviour in two synapses, he will
soon be able to understand human logical abilities. Just follow the guide and be
confident.

At the beginning of this century naval architects had learned to build bigger
and stronger battleships by using more and more steel. However, the magnetic
compasses of these dreadnoughts went wild with so much iron around. Even
though they were stronger and bigger, the battleships were on the whole weaker
than before since they got lost at sea. It was at this point, that a group with a
solution, led by Sperry, suggested that naval architects give up the magnetic
compass and use instead gyrocompasses that did not depend on magnetic fields.
Did they have the gyrocompass? Not quite. It was not yet a black box offered for
sale: this is why a detour had to be negotiated. The Navy must invest in Sperry’s
research in order to convert his idea into a workable gyroscope, so that, in the
end, their battleships can steer a straight course again. Sperry has positioned
himself so that a common translation of his interests and that of the Navy now
reads: ‘You cannot navigate your ships properly: I can’t make my gyrocompass
a real thing: wait a little, come my way, and after a while your ships will make full
use of their terrifying powers again and my gyrocompasses will spread in ships
and planes in the form of well closed black boxes.’

This community of interests is the result of a difficult and tense negotiation
that may break down at any point. In particular, it is based on a sort of implicit
contract: there should be a return to the main road, and the detour should be
short. What happens if it becomes long, so long indeed that it now appears in the
eyes of the enrolled groups as a deviation rather than a short cut? Imagine that for
a decade the millionaire keeps reading papers on the firing of synapses, expecting
the discovery of the neurons for induction and deduction any day. He might die
of boredom before seeing his dreams fulfilled. He might think that this is not the
detour they had agreed upon, but a new direction altogether. He might even
realise that it is the second strategy which has been practised, not the third and
then decide to sever the negotiations, to cut the money off, and to dismiss the
scientists who were not only pulling his leg but also using his money.

This is what occurred with Diesel. MAN was ready to wait for a few years, to
lend engineers, with the idea that they would soon resume their usual business of
manufacturing engines but on a larger scale. If the return is delayed, the
management may feel cheated, as if they were perceiving the second type of
translation through the veil of the third. If they start thinking this way, then
Diesel is taken as a parasite on MAN diverting its resources to further his own
egotistical dreams. Interests are elastic, but like rubber, there is a point where

they break or spring back.

So, even if this third way of translating the interest of others is better than the
second, it has its shortcomings. It is always open to the accusation of bootlegging – to use the expression of American scientists – that is, the size of the
detour and the length of the delay being fuzzy, a detour might be seen as an
outright diversion, or even as a hijacking. Support may thus be cut off before
Watson and Crick discover the double helix structure, Diesel has time to make
his engine, West to build his Eagle computer, Sperry his gyrocompass, and the
brain scientists to find how a synapse fires. There is no accepted standard for
measuring detours because the ‘acceptable’ length of the detour is a result of negotiation. MAN, for instance, became worried after only a few years, but the private medical foundations that invested in Lawrence’s huge
accelerators at Berkeley did not, even though Lawrence was furthering particle
physics by arguing that he was building bigger radiation sources for cancer
therapy! Depending on the negotiators’ abilities, a few hundred dollars may
appear to be an intolerable waste of money, while building cyclotrons looks like
the only straight path to a cure for cancer.

There are two other limitations to this third strategy. First, whenever the usual
road is not blocked, whenever it is not clearly apparent to the eyes of a group that
they cannot follow their usual route, it becomes impossible to convince them to
make a detour. Second, once the detour has been completed and everyone is
happy, it is very hard to decide who is responsible for the move. Since the Navy
helped Sperry, it can claim credit for the whole gyrocompass which would
otherwise have remained a vague sketch or an engineer’s blueprint. But since
without his gyrocompass the Navy fear that its dreadnoughts will be lost at sea.
Sperry may very well claim to be the active force behind the Navy. There may be
a bitter struggle to allocate credit, even when everything goes well.

(4) Translation four: reshuffling interests and goals.

A fourth strategy is needed to overcome the shortcomings of the third:
(a) the length of the detour should be impossible to evaluate for those who are
enlisted;
(b) it should be possible to enrol others even if their usual course is not obviously
cut off;
(c) it should be impossible to decide who is enlisted and who does the enlisting;
(d) nevertheless, the fact-builders should appear as the only driving force.

To carry off what would seem to be a quite impossible task, there is one
obstacle that seems at first to be unsurmountable: people’s explicit interests. So
far I have used the term ‘explicit interest’ in a non-controversial way: the Navy has
interests, so has the millionaire, so has MAN, so have all the other actors we have
followed. All of them know more or less what they want, and a list of their goals
may, at least in principle, be set up, either by them or by observers. As long as the goals of all these actors are explicit, the fact-builder's degree of freedom is limited to the narrow circle delineated by the three strategies above. The enlisted groups know that they are a group: know where they want to go; know if their usual way is interrupted. Know how far they are ready to deviate from it: know when they have returned to it; and finally, know how much credit should go to those who helped them for a while. They know a lot. They know too much because this knowledge limits the moves of the contenders and paralyses negotiations. As long as a group possesses such knowledge, it will be extremely hard to enrol it in the fact-building and, still more, to control its behaviour. But how to bypass this obstacle? The answer is simple and radical. By following fact-builders in action we are going to see one of their most extraordinary feats: they are going to do away with explicit interests so as to increase their margin for manoeuvre.

(A) TACTIC ONE: DISPLACING GOALS

Even if they are explicit, the meanings of people's goals may be differently interpreted. A group with a solution is looking for a problem but no one has a problem.... Well, why not make them have a problem? If a group feels that its usual way is not at all interrupted, is it not possible to offer it another scenario in which it has got a big problem?

When Leo Szilard first entered into discussion with the Pentagon in the early 1940's, the generals were not interested in his proposal to build an atomic weapon. They argued that it always takes a generation to invent a new weapon system, that putting money into this project might be good for physicists for doing physics but not soldiers for waging war. Thus they saw Szilard's proposal as a typical case of bootlegging; physicists would be better occupied perfecting older weapon systems. Since they did not feel their usual way of inventing weapons was cut off, the generals had no reason to see Szilard's proposal as a solution to a non-existent problem. Then Szilard started to work on the officers' goals. 'What if the Germans got the atom bomb first? How will you manage to win the war - your explicit aim - with all your older and obsolete weapons?' The generals had to win a war - 'a war' in its usual rendering means a classical one: after Szilard's intervention they still had to win the war - meaning now a new atomic one. The shift in meaning is slight but sufficient to change the standing of the atomic physicists: useless in the first version, they become necessary in the second. The war machine is not being invaded by bootlegging physicists any more. It is now geared full speed towards the progressive realisation of Szilard's vague patent into a not so vague bomb...

(B) TACTIC TWO: INVENTING NEW GOALS

Displacing the goals of the groups to be enlisted so as to create the problem and then offering a possible solution is nice, but still limited by the original aims. Thus, in this example, Szilard could convince the Pentagon to wage a nuclear war, but not to lose it or to support classical dance. The margin of freedom would be much increased if new goals could be devised.

When George Eastman tried to move into the business of selling photographic plates, he soon realised that he could convince only a few, well-equipped amateurs to buy his plates and his paper. They were used to working in semi-professional laboratories built in their homes. Others were uninterested in taking pictures themselves. They did not want to buy costly and cumbersome black boxes - this time in the literal sense of the word! Eastman then devised the notion of 'amateur photography': everyone from 6 to 96 years old might, could, should, want to take photographs. Having this idea of a mass market, Eastman and his friends had to define the object that would convince everybody to take photographs. Only a few people were ready for a long detour through expensive laboratories. The Eastman Company had to make the detour extremely small to enlist everyone. So that no one should hesitate to take a picture, the object should be cheap, and easy, so easy that, as Eastman put it: 'You press the button, we do the rest', or as we say in French, 'Clac, clac, merci Kodak'. The camera was not yet there, but Eastman already sensed the contours of the object which would make his company indispensable. Previously few people had had the goal of taking photographs. If Eastman was successful, everyone would have this goal, and the only way to fulfil this craving would be to buy camera and films from the local Eastman Company dealer.

(C) TACTIC THREE: INVENTING NEW GROUPS

This is easier said than done. Interests are the consequence of whatever groups have been previously engaged to do. MAN builds steam engines; it may be persuaded to build diesel engines, but not easily persuaded to make yoghurt. The Pentagon wishes to win the war; they might be persuaded to win an atomic one, but not easily to dance, and so on. The ability to invent new goals is limited by the existence of already defined groups. It would be much better to define new groups that could then be endowed with new goals, goals which could, in turn, be reached only by helping the contenders to build their facts. At first sight, it seems impossible to invent new groups; in practice, it is the easiest and by far the most efficient strategy. For instance, Eastman could not impose a new goal - taking pictures - without devising a new group from scratch, the amateur photographer from age 6 to 96.

In the mid-nineteenth century, rich and poor, capitalist and proletariat were some of the most solidly defined groups because of the class struggle. Health officers who wished to overhaul European and American cities to make them safe and hygienic were constantly stalled by class hostility between poor and rich. The simplest regulation for health was considered either to be too radical, or, on the contrary, to be one more stick for the rich to beat the poor with. When
Pasteur and the hygienists introduced the notion of a microbe as the essential cause of infectious disease, they did not take the society to be made up of rich and poor, but of a rather different list of groups: sick contagious people, healthy but dangerous carriers of the microbes, immunised people, vaccinated people, and so on. Indeed, they added a lot of non-human actors to the definition of the groups as well: mosquitoes, parasites, rats, fleas, plus the millions of fermenters, bacteria, micrococci and other little bugs. After this reshuffling, the relevant groups were not the same: a very rich man’s son could die simply because the very poor maid was carrying typhoid. As a consequence, a different type of solidarity emerged. As long as society was made up just of classes, hygienists did not know how to become indispensable. Their advice was not followed, their solutions were not applied. As soon as newly formed groups were threatened by the newly invented enemy, common interest was created, and so was a craving for the biologists’ solutions; hygienists allied with microbiologists were positioned at the centre of all regulations. Vaccines, filters, antiseptics, know-how that had until then been confined to a few laboratories spread to every household.

(C) TACTIC FOUR: RENDERING THE DETOUR INVISIBLE

The third tactic has its shortcomings as well. As long as a group—even made up—is able to detect a widening gap between its goals—even displaced—and that of the enrolling groups the margin of negotiation of the latter is much restrained. People can still see the difference between what they wanted and what they got, they still can feel they have been cheated. A fourth move is thus necessary that turns the detour into a progressive drift, so that the enrolled group still thinks that it is going along a straight line without ever abandoning its own interests.

In Chapter 1 we studied such a drift. The managers of a big company were after new, more efficient, cars. They had been convinced by their research group that electric cars using fuel cells were the key to the future. This was the first translation: ‘more efficient cars’ equals ‘fuel cells’. But since nothing was known of fuel cells they were convinced by the research director that the crucial enigma to be tackled was the behaviour of electrodes in catalysis. This provided the second translation. The problem, they were later told by engineers, was that the electrode is so complex that they should study a single pore of a single electrode. The third translation now reads: ‘study of catalysis’ = ‘study of one pore’ (see Chapter 1, sentence (8)). But since the series of translations is a transitive relation the final version upheld by the Board of Directors was: ‘new efficient cars’ = ‘research into the one-pore model’. No matter how far the drift might appear, it is not felt as a detour any more. On the contrary, it has become the only straight way to get at the car. The Board’s interests have to go through this one pore like the camel through the eye of the needle!

To take another example, a French columnist argued, in 1871, after the Franco-Prussian war, that if the French had been beaten, this was due to the German soldiers’ better state of health. This is the first translation that offers a

Figure 3.2

new rendering of the military disaster. Then he goes on by arguing that this better health was due to German superiority in science. Translation two expounds a new interpretation of the usefulness of basic science. He then explained that science was superior in Germany because it was better funded. Third translation. He next tells the reader that the French Assembly was, at that moment, cutting funds for basic science. This makes for a fourth displacement: no revenge would ever be possible if we had no money, since there is no science without money, no healthy soldiers without science and no revenge without soldiers. In the end he suggests to the reader what to do: write to your representative to make him change his vote. All the slight displacements are smoothly nested, one in another, so that the same reader who was ready to pick up his rifle and march on the Alaskan frontier to beat the Germans, was now, with the same energy, and without having eschewed his goal, writing an indignant letter to his representative!

It should now be clear why I used the word translation. In addition to its linguistic meaning (relating versions in one language to versions in another one) it has also a geometric meaning (moving from one place to another). Translating interests means at once offering new interpretations of these interests and channelling people in different directions. ‘Take your revenge’ is made to mean ‘write a letter’; ‘build a new car’ is made to really mean ‘study one pore of an electrode’. The results of such renderings are a slow movement from one place to another. The main advantage of such a slow mobilisation is that particular issues (like that of the science budget or of the one-pore model) are now solidly tied to much larger ones (the survival of the country, the future of cars), so well tied indeed that threatening the former is tantamount to threatening the latter. Subtly woven and carefully thrown, this very fine net can be very useful at keeping groups in its meshes.
E) TACTIC FIVE: WINNING TRIALS OF ATTRIBUTION

All the above moves enormously increase the contender's room for manoeuvre, especially the latter which dissolves the notion of explicit interest. It is no longer possible to tell who is enrolled and who is enrolling, who is going out of his way and who is not. But this success brings its own problems with it. How can we decide who did the job, or indeed, how can the fact-builders determine if the facts eventually built are their own? All along we meet this problem: with Diesel's engine, with Pasteur's vaccine, with Sperry's gyrocompass. The whole process of enrolment, no matter how cleverly managed, may be wasted if others gain credit for it. Conversely, enormous gains may be made simply by solving it, even if the process of enrolment has been badly managed.

After reading a famous work by Pasteur on fermentation, an English surgeon, Lister, 'had the idea' that wound infections—that killed most if not all of his patients—might be similar to fermentation. Imitating Pasteur's handling of fermenting wine, Lister then imagined that by killing the germs in the wounds and by letting oxygen pass through the dressing, infection would stop and the wound heal cleanly. After many years of trials, he invented asepsis and antisepsis. Hold on! Did he invent them? A new discussion starts. So he did not, because many surgeons had had the idea of linking infection and fermentation before, and of letting air through the bandage; many colleagues worked with and against him for many years before asepsis became a routine black box in all surgical wards. Besides, in many lectures Lister gratefully attributed his original ideas to Pasteur's memoir. So, in a sense he 'simply developed' what was in germ, so to speak, in Pasteur's invention. But Pasteur never made asepsis and antisepsis a workable practice in surgery. Lister did. So, in another sense, Lister did everything. Historians, as much as the actors themselves, delight in deciding who influenced whom, who had only a marginal contribution and who made the most significant contribution. With each new witness, someone else, or some other group, takes credit for part or for all of the move.

So as not to be confused, we should distinguish the recruiting of allies so as to build a fact or a machine collectively, from the attributions of responsibility to those who did most of the work. By definition, and according to our first principle, since the construction of facts is collective, everyone is as necessary as anyone else. Nevertheless, it is possible, in spite of this necessity, to make everyone accept a few people, or even one person, as the main cause for their collective work. Pasteur, for instance, not only recruited many sources of support, but also strove to maintain his laboratory as the source of the general movement that was made up of many scientists, officials, engineers and firms. Although he had to accept their views and follow their moves—so as to extend his lab—he also had to fight so that they all appeared as simply 'applying' his ideas and following his leads. The two movements must be carefully distinguished because, although they are complementary for a successful strategy, they lead in opposite directions: the recruitment of allies supposes that you go as far and make as many compromises as possible, whereas the attribution of responsibility requires you to limit the number of actors as much as possible. The question of knowing who follows and who is followed should in no way be asked if the first movement is to succeed, and nevertheless should be settled for the second movement to be completed. Although Diesel followed many of the people he recruited, translating their common interest in an ambiguous mixture, in the end he had to make them consider his science as the leader they followed.

I will call the primary mechanism that which makes it possible to solve the enrolment problem and make the collective action of many people turn from 'germs' into reality asepsis, gyrocompasses, GRF or diesel engines. To this mechanism a secondary mechanism has to be added which might have no relation at all with the first and which is as controversial and as bitter as the others.

A military metaphor will help us remember this essential point. When an historian says that Napoleon leads the Great Army through Russia every reader knows that Napoleon with his own body is not strong enough to win, say, the battle of Borodino. During the battle half a million people are taking initiatives, mixing up the commands, ignoring orders, fleeing or courageously dying. This gigantic mechanism is much bigger than what Napoleon can handle or even see from the top of a hill. Nevertheless, after the battle, his soldiers, the Tsar Kutuzov who commands the Russian army, the people of Paris, the historians, all attribute to him and only to him the responsibility for the victory—which in this case turned out later to be a defeat. Everyone will agree that there may be some relation between what Napoleon did during the battle and the hundreds of thousands of others did, but they will also agree that these relations cannot be captured by the sentence 'Napoleon won because he had the power and the others obeyed'. Exactly the same is true of the relations between the handful of scientists and the millions of others. Their complicated and unpredictable relations cannot be captured by a simple order of command that would go from basic science to the rest of society via applied science and development.

Other people will decide that Diesel was a mere precursor, or that Pasteur did all the basic work on asepsis, or that Sperry had only a marginal input into the gyrocompass. Even when all these questions are later tackled by historians, their research adds an important expert testimony to the trials, but it does not end the trials and does not take the place of the court. In practice, however, people make versions more credible than others. Everyone may finally accept that Diesel 'had the idea' of his engine, that Lister 'invented' asepsis with the help of Pasteur's memoirs, or that Napoleon 'led' the Great Army. For a reason that will become still clearer in Part C, this secondary distribution of flags and medals should never be confused with the primary process.

(5) Translation five: becoming indispensable

The contenders now have a lot of leeway with these five tactics in their attempts
to interest people in the outcome of their claims. With guile and patience it should be possible to see everyone contributing to the spread of a claim in time and space—which will then become a routine black box in everyone’s hands. If such a point were reached, then no further strategy would be necessary: the contenders would have simply become indispensable. They would not have to cater to others’ interests—first translation—nor to convince them that their usual ways were cut off—second translation—nor seduce them through a little detour—third translation; it would no longer even be necessary to invent new groups, new goals, to surreptitiously bring about drift in interests, or to fight bitter struggles for attribution of responsibilities. The contenders would simply sit at a particular place, and the others would flow effortlessly through them, borrowing their claims, buying their products, willingly participating in the construction and spread of black boxes. People would simply rush to buy Eastman Kodak cameras, to have Pasteur’s injections, to try Diesel’s new engines, to install new gyrocompasses, to believe Schally’s claims without a shadow of a doubt, and to dutifully acknowledge the ownership rights of Eastman, Pasteur, Diesel, Sperry and Schally.

The quandary of the fact-builder would not simply be precariously patched up. It would be entirely resolved. No negotiation, no displacement would be necessary since the others would do the moving, the begging, the compromising and the negotiation. They are the ones who would go out of their way. In Figures 3.1 and 3.2 I pictured the four translations. They all lead to the fifth translation that literally sums them up. In the geometric sense of translation it means that whatever you do, and wherever you go, you have to pass through the contenders’ position and to help them further their interests. In the linguistic sense of the word translation, it means that one version translates every other, acquiring a sort of hegemony: whatever your want, you want this as well. The diagram makes clear that, from the first to the last, the contenders have shifted from the most extreme weakness—that forced them to follow the others—to the greatest strength—that forces all the others to follow them.

Is such a strategy feasible? Shadowing scientists and engineers will show us that it is common practice, but that, in order to succeed, other allies have to be brought in and most of them do not look like men or women.

Part B
Keeping the interested groups in line

We saw in the introduction to this chapter that two things are needed in order to build a black box: first it is necessary to enrol others so that they believe it, buy it and disseminate it across time and space; second, it is necessary to control them so that what they borrow and spread remains more or less the same. If people are not interested, or if they do something entirely different with the claim, the spread of a fact or of a machine in time and space does not take place. A few people toy with an idea for a few days, but it soon disappears, to be replaced by another. Projects which trigger enthusiasm are quickly put back into a drawer. Theories that had started to infect the world shrink back to become the idée fixe of some lunatic in an asylum. Even colleagues who had been ‘unalterably’ convinced by a laboratory demonstration can change their minds a month later. Established facts are quickly turned into artefacts, and puzzled people ask, ‘How could we have believed such an absurdity?’ Established industries that looked as if they were to last for ever suddenly become obsolete and start falling apart, displaced by newer ones. Dissenters who interrupt the spread of any fact or artefact proliferate.

In Part A we have seen how to do half the job, that is, how to interest others. Now we have to tackle the other half: how to make their behaviour predictable. This is a much harder task.

(1) A chain is only as strong as its weakest link

Let us first assess the difficulty of the task. When Diesel succeeded in interesting MAN in his project for a perfect engine, he was lent money, workshops, assistants, and was granted some time. His problem was to hold those elements together with the ones he was bringing into the contract: Carnot’s thermodynamics, the principle of ignition at constant temperature and his own views on the future market. Initially all these elements are simply assembled in one place at Augsburg. What could bind them more firmly together? A working prototype which might later be used as a single piece of standard equipment in
other settings—a submarine or a truck, for example. What will happen if Diesel cannot hold all these elements at once? The answer is simple: they will be disbanded as easily as they have been assembled. Each of the elements will go its own way: MAN will go on building steam engines, assistants will be moved to other jobs, money will flow elsewhere, Carnot’s thermodynamics will remain a cryptic piece of basic physics, ignition at constant temperature will be remembered as a technological dead end, and Diesel will occupy himself with other tasks, leaving little trace in the history books.

So the number of enlisted interests is important but far from enough, because knitting and tying them together may be undone. Pasteur had been able to convince farmers who raised cattle that the only way to solve the terrible anthrax plague was to pass through his laboratories at the Ecole Normale Supérieure in Rue d’Ulm in Paris. Breathing down Pasteur’s neck were thousands of interests nested into one another, all ready to accept his short cut through the microscope, the artificial culture of microbes, and the promised vaccine. However, there is a considerable drift between an interest in raising cattle on a farm and watching microbes grow in Petri dishes: the gathering crowd might disband rapidly. After a few months of hope they might all leave disappointed, bitterly accusing Pasteur of having fooled them by creating artefacts in his laboratory of little relevance to farms and cattle. Pasteur would then become a mere precursor for the anthrax vaccine, his role in history being accordingly diminished. Something else is needed to tie the diverted resources and invested interests together in a durable way.

Eastman had the bright idea of inventing a new group of 6–to-96-year-olds that was endowed with a craving for taking pictures. This enlistment depended on a camera that was simple to operate, which meant a camera with film and not the expensive fragile and cumbersome glass plates then used. But what would happen if the film slackened so much that all the pictures were fuzzy? What if the coating of the film blistered? No matter how many people found photography appealing, no matter how big the Eastman Company, not matter how clever and interested Eastman might be, the associated interests would disassociate. Eastman, with his dream of a mass market, would become one of the many precursors in the long history of popular photography. Others would take up his patents, even perhaps buy his company.

Something more is needed to turn the temporary juxtaposition of interests into a durable whole. Without this ‘little something’, the assembly of people necessary to turn a claim into a black box will behave unpredictably: they will dissent, they will open it, tinker with it; worse, they will lose interest and drop it altogether. This ‘dangerous’ behaviour should be made impossible; even better, it should be made unthinkable.

We know the answer since we have been talking about it for three chapters: the only way to keep the dissenters at bay is to link the fate of the claim with so many assembled elements that it resists all trials to break it apart.

The first prototype that Diesel assembles is much like Schally’s GRF or Blondlot’s ill-fated N-Rays: each new trial makes it falter. At the start, Diesel ties the fate of his engine to that of any fuel, thinking that they would all ignite at a very high pressure. This, to him, is what made his engine so versatile. He needs very high pressure to obtain such a result, with pistons, cylinders and valves strong enough to withstand more than 33 atmospheres. MAN was able to provide him with excellent machine tools and know-how so that it soon became possible to obtain such high pressure. But then, nothing happened. Not every fuel ignited. This ally which he had expected to be unproblematic and faithful betrayed him. Only kerosene ignited, and then only erratically. How could the ignition of kerosene be kept in line? Diesel discovered that it depended on the right mixture of air and fuel. To keep this mixture constant he had to introduce the fuel and the air into the cylinder at a very high pressure. But Diesel had to add powerful pumps, sturdy valves and a lot of extra plumbing to his original design to obtain such a result. His engine may run, but it becomes large and expensive.

So what is happening? Diesel has to shift his system of alliances: high pressure plus any fuel plus solid injection lead to engines of any size which interest everyone and spread everywhere. But this series of associations is dismantled in the Augsburg workshop, as soon as it is tried out. The engine does not even turn one stroke. So, a new series of alliances is tried out: high pressure plus kerosene plus air injection which means a large and costly engine that idles for a few seconds.

I hear the reader’s objection: ‘But do we really have to go into these details to understand how others are to be controlled?’ Yes, because with these little details others are not controlled! Like the dissenter of Chapter 2, they apply pressure to the new design, and the whole thing breaks apart. To resist dissent, that is to resist trials of force, Diesel has to invent an injection pump that holds air and kerosene together, allows the high pressure to ignite the mixture, makes the engine run, and thus keeps MAN in line. But if the kerosene, the air, and MAN are kept in line, this is not the case for the vast market anticipated by Diesel. This has to be given up. Groping in the dark inside his workshop, Diesel has to choose alliances. He has to decide what he most wishes to keep in line. There is at first no engine that can ally itself to air, to any fuel and to everyone’s needs. *Something has to give way*: a fuel, the kerosene, solid injection, Carnot’s principles, the mass market, Diesel’s stamina, MAN’s patience, rights to patents . . . Something.

The same choice goes on in Pasteur’s laboratory. Is there anything that can be used to tie in the farmers’ interests before they all go away bitter and scornful? A tiny bacillus inside a urine medium will not do, even if it is visible under the microscope. It is only of marginal interest to people who have been attracted to the lab by the promise that they will soon be back on their farms, milking healthier cows and shearing healthier sheep. If Pasteur was using his bacillus to do biochemistry or taxonomy, deciding if it was an animal or a lichen, others like biochemists or taxonomists would be interested, but not the farmers. When Pasteur shows that sheep fed older cultures of the bacillus resist the disease even when they are later fed virulent cultures, biochemists and taxonomists are only
faithful? Is this other one reliable? Is this one a credible spokesperson? But what did not occur to Machiavelli is that these alliances can cut across the boundaries between human beings and 'things'. Every time an ally is abandoned, replacements need to be recruited; every time a sturdy link disrupts an alliance that would be useful, new elements should be brought in to break it apart and make use of the dismantled elements. These 'machiavellian' strategies are made more visible when we follow scientists and engineers. Rather, we call 'scientists' and 'engineers' those subtle enough to include in the same repertoire of ploys human and non-human resources, thus increasing their margin for negotiation.

Take for instance the Bell Company. Telephone lines in the early days were able to carry a voice only a few kilometres. Beyond this limit the voice became garbled, full of static, inaudible. The message was corrupted and not transmitted. By 'boosting' the signals every thirteen kilometres, the distance could be increased. In 1910, mechanical repeaters were invented to relay the message. But these costly and unreliable repeaters could be installed only on a few lines. The Bell Company was able to expand, but not very far, and certainly not through the desert, or the great plains of the United States where all sorts of small companies were thriving in the midst of complete chaos. Ma Bell, as it is nicknamed by Americans, was indeed in the business of linking people together, but with the mechanical repeater many people who might wish to pass through her network could not do so. An exhibition in San Francisco in 1913 offered Bell a challenge. What if we could link the West and the East Coast with one telephone line? Can you imagine that? A transcontinental line tying the US together and rendering Bell the indispensable go-between of a hundred million people, eliminating all the small companies? Alas, this is impossible because of the cost of the old repeater. It becomes the missing link in this new alliance planned between Ma Bell and everyone in the US. The project falls apart, becomes a dream. No transcontinental line for the time being. Better send your messages through the Post Office.

Jewett, one of the directors of Bell, looks for new possible alliances that will help the company out of its predicament. He remembers that he was taught by Millikan, when the latter was a young lecturer. Now a famous physicist, Millikan works on the electron, a new object at the time, that is slowly being built up in his laboratory like all the other acts we saw in Chapter 2. One of the features of the electron is that it has no inertia. Jewett, who himself has a doctorate in physics, is ready for a little detour. Something which has no inertia loses little energy. Why not ask Millikan about a possible new repeater? Millikan's laboratory has nothing to offer, yet. Nothing ready for sale. No black box repeating long-distance messages cheaply and safely. What Millikan can do, however, is to lend Jewett a few of his best students, to whom Bell offers a well-equipped laboratory. At this point Millikan's physics is in part connected with Bell's fate, which is partly connected with the challenge of the San Francisco fair, according to a chain of translations like the ones we studied above. Through a series of slight displacements, electrons, Bell, Millikan and the continental line.

(2) Tying up with new unexpected allies

We now start to understand that there is no way of tying together interested groups - mobilised in part A - unless other elements are tied with them: piston, air, kerosene, urine medium, microbes, roller, coating, celluloid, etc. But we also understand that it is not possible to tie any element to another at random. Choices have to be made. Diesel's decision to go in for air injection means that many potential buyers have to be abandoned and that Carnot's principles may not be that easily applied. Pasteur's search for a new medium for his vaccine entails the abandonment of other interests in biochemistry and taxonomy. Amateurs may be captured by Eastman's new Kodak camera, but the semi-professionals who do their own plates and development are left to one side and the new film coating had better not blister. As in Machiavelli's Prince, the progressive building up of an empire is a series of decisions about alliances: With whom can I collaborate? Whom should I write off? How can I make this one...
are closer to one another than they were before. But it is still a mere juxtaposition. The Bell Company managers may soon realise that basic physics is good for physicists but not for businessmen; electrons may refuse to jump from one electrode of the new triodes to the next when the tension gets too high, and fill the vacuum with a blue cloud; the urge for a transcontinental line may no longer be felt by the Board of Directors.

This mere juxtaposition is transformed when Arnold, one of the recruited physicists, transforms a triode patented by another inventor. In a very high vacuum, even at very high tension, the slightest vibration at one end triggers a strong vibration at the other. A new object is then created through new trials in the newly opened laboratory: electrons that greatly amplify signals. This new electronic repeater is soon transformed into a black box by the collective work of Ma Bell and incorporated as a routine piece of equipment in six locations along the 5500 kilometres of cable laid across the continent. In 1914, the transcontinental line, impossible with the other repeater, becomes real.

Alexander Bell calls Mr Watson, who is no longer downstairs but thousands of miles away. The Bell Company is now able to expand over the whole continent: consumers who had not before had the slightest interest in telephoning the other coast now routinely do so, passing through the Bell network and contributing to its expansion - as anticipated from the fifth translation described above. But the boundaries of physics have been transformed as well, from a few modestly equipped laboratories in universities to many well-endowed laboratories in industry; from now on many students could make a career in industrial physics. And Millikan? He has changed too, since many effects first stabilised in his lab are now routinely used along telephone lines, everywhere, thus providing his laboratory with a fantastic expansion. Something else has moved too. The electrons. The list of actions that defined their being has been dramatically increased when all these laboratories submitted them to new and unexpected trials. Domesticated electrons have been made to play a role in a convoluted alliance that allows the Bell Company to triumph over its rivals. In the end, each actor in this little story has been pushed out of its usual way and made to be different, because of the new alliances it has been forced to enter.

We, the laypeople, far away from the practice of science and the slow build-up of artefacts, have no idea of the versatility of the alliances scientists are ready to make. We keep nice clean boundaries that exclude 'irrelevant' elements: electrons have nothing to do with big business; microbes in bacteriology have nothing to do with farms and cattle; Carnot's thermodynamics is infinitely far from submarines. And we are right. There is at first a vast distance between these elements; at the beginning they are indeed irrelevant. But 'relevance', like everything else, can be made. How? By the series of translations I have sketched. When Jewett first fetches Millikan, the electrons are too feeble to have any easy connection with Ma Bell. At the end, inside the triode redesigned by Arnold, they reliably transmit Alexander Bell's orders to Mr Watson. The smaller companies might have thought that Ma Bell would never beat them since it was impossible to build a transcontinental line. This was counting without the electrons. By adding electrons and Millikan and his students and a new lab to the list of its allies, Ma Bell modifies the relations of forces. Where it was weak over longer distances, it is now stronger than anyone else.

We always feel it is important to decide on the nature of the alliances: are the elements human or non-human? Are they technical or scientific? Are they objective or subjective? Whereas the only question that really matters is the following: is this new association weaker or stronger than that one. Veterinary science had not the slightest relation with the biology done in laboratories when Pasteur began his study. This does not mean that this connection cannot be built. Through the establishment of a long list of allies, the tiny bacillus attenuated by the culture has a sudden bearing on the interests of farmers. Indeed, it is what definitively reverses the balance of power. Vets with all their science now have to pass through Pasteur's laboratory and borrow his vaccine as an uncontrollable black box. He has become indispensable. The fulfilment of the strategies presented in Part A is entirely dependent on the new unexpected allies that have been made to be relevant.

The consequence of these bold moves that enrol newly formed actors (microbes, electrons) in our human affairs is that there is no way to counteract them except by tackling these 'technical details'. Like the proof race described in Chapter 1, once it has started there is no way of avoiding the nitty-gritty since this is what makes the difference. Without building expensive laboratories that they could not afford in an attempt to attract physics and electrons back into their own camp, the small companies eliminated by Bell could not resist. The laboratories studied in Chapter 2 now occupy the centre of these strategies through which new actors constituting a vast reservoir of forces are mobilised. The spokespersons able to talk on behalf of new and invisible actors are now the linchpins on which the balance of power rests: a new characteristic of electrons, one more degree in the culture medium, and the whole assembled crowd either breaks up or is irreversibly bound.

The intimate details of an obscure science may become a battlefield like a hitherto modest hamlet became the stage for the battle of Waterloo. In Edinburgh, for instance, at the beginning of the nineteenth century, the rising middle class was chafing under the social superiority of high society. Applying the above strategy, they looked for unexpected allies to reverse this situation. They seized on a movement in brain science called phrenology that allowed almost anyone to read off people's qualities by carefully considering the bumps on their skulls and the shape of their faces. This use of cranial characteristics threatened to reshape Scottish class fabric entirely, exactly like the hygienists did above with the microbes (p.113). To evaluate the moral worth of someone the questions were no longer: Who are his parents? How ancient is his lineage? How vast are his propeties? But only: Does his skull possess the shape that expresses virtue and honesty? By allying themselves with phrenology the middle class could change its position in relation to the upper class, which at first was uninterested in
brain science, by reallocating everybody into newly relevant groups. To resist brain scientists, other brain scientists had to be enlisted, hook, line, and sinker. Thus a controversy started not about social classes, but about neurology. As the controversy heated, the discussion shifted inside brain science; in fact, it shifted literally inside the brain. Atlases were printed, skulls cut open, dissections performed, to decide whether the inner structure of the brain could be predicted from the outer shape of the skull, as argued by phrenologists. Like the dissenters in Chapter 2 the newly recruited brain scientists tried out the connections established by phrenologists. The more they tried, the deeper they were led inside the brain, straining their eyes to discern whether the cerebellum, for instance, was linked to the rest of the body from the top or from the bottom. Moving slowly through the various translations, the contenders ended up in the cerebellum; and they did so because this latter proved the weak link.

(3) Machinations of forces

Interested groups may therefore be kept in line as, moving through a series of translations, they end up being trapped by a completely new element that is itself so strongly tied that nothing can break it up. Without exactly understanding how it all happened, people start placing transcontinental phone calls, taking photographs, having their cats and children vaccinated, and believing in phrenology. The quandary of the fact-builder is thus resolved, since all these people willingly contribute to the further expansion of these many black boxes. A new and deeper problem arises, however, caused by the very success of all the plots discussed above. These new and unexpected allies brought in to keep the first groups in line, how can they in turn, be kept in line? Are they not another provisional juxtaposition of helping hands, ready to disband? Is not the flask of Pasteurian vaccine likely to be spoiled? What keeps the new prototype triodes from switching off after a few hours? What if the cerebellum turns out to be a shapeless mass of brain tissue? As to the diesel engine, we know how unreliable it is; it has to be debugged for longer years than the Eagle computer. How should these disordered assemblies be turned into such a tightly glued whole that it can link the enrolled groups together durably? Machiavelli knew perfectly well that the alliances binding towns and crowns are shifting and uncertain. But we are considering much more shifting and uncertain alliances between brains, microbes, electrons and fuels, than those necessary to bind together towns and crowns. If there is no way to render the new allies more reliable than the older ones, then the whole enterprise is spoiled and claims will shrink back to a single place and a single time.

We take the answer so much for granted that we no longer feel how simple and original it is. The simplest means of transforming the juxtaposed set of allies into a whole that acts as one is to tie the assembled forces to one another. That is, to build a machine. A machine, as its name implies, is first of all, a machination, a stratagem, a kind of cunning, where borrowed forces keep one another in check so that none can fly apart from the group. This makes a machine different from a tool which is a single element held directly in the hand of a man or a woman. Useful as tools are, they never turn Mr or Ms Anybody into Mr or Ms Manybodies! The trick is to sever the link each tool has with each body and tie them to one another instead. The pestle is a tool in the woman's hand; she is stronger with it than with her hands alone, for now she is able to grind corn. However if you tie the grinder to a wooden frame and if this frame is tied to the sails of a mill that profits from the wind, this is a machine, a windmill, that puts into the miller's hands an assembly of forces no human could ever match.

It is essential to note that the skills required to go from the pestle to the windmill are exactly symmetrical to the ones we saw in Part A. How can the wind be borrowed? How can it be made to have a bearing on corn and bread? How can its force be translated so that, whatever it does or does not do, the corn is reliably ground? Yes, we may use the words translation and interest as well, because it is no more and no less difficult to interest a group in the fabrication of a vaccine than to interest the wind in the fabrication of bread. Complicated negotiations have to go on continuously in both cases so that the provisional alliances do not break off.

For instance, the assembled groups of farmers may, as I showed, lose interest. And the wind, what can it do? Simply blow the fragile windmill away, tearing the sails and the wings off. What should the mechanic do to hold the wind in his system of alliances, in spite of the way it shifts direction and changes strength? He has to negotiate. He has to tailor a machine that can stay open to the wind and still be immune to its deleterious effects. Severe the association between the sail mechanism and the tower on which the mill is built, will do the trick. The top of the mill now revolves. Of course, there is a price to pay, for now you need more cranks and a complicated system of wheels, but the wind has been made into a reliable ally. No matter how much the winds shift, no matter what the winds want, the whole windmill will act as one piece, resisting dissociation in spite of the increasing number of pieces it is now made of. What happens to the people gathered round the mill? They too are definitely 'interested' in the mill. No matter what they want, no matter how good they were at handling the pestle, they now have to pass through the mill. Thus they are kept in line just as much as the wind is. If the wind had toppled the mill, then they could have abandoned the miller and gone their usual ways. Now that the top of the mill revolves, thanks to a complicated assembly of nuts and bolts, they cannot compete with it. It is a clever machination, isn't it, and because of it the mill has become an obligatory passage point for the people, for the corn and for the wind. If revolving windmills cannot do the job alone, then one can make it illegal to grind corn at home. If the new law does not work immediately, use fashion or taste, anything that will habituate people to the mill and forget their pestle. I told you the alliances were 'machiavellian'!
Still it is hard to see how a profusion of forces can be kept in line by relatively simple machinations like windmills. One snag becomes obvious: the process of recruiting and maintaining allies involves increasing complexity in the machine. Even the best mechanic will find it difficult to regulate the machine—check the wind, mend the sails, enforce the law—so that all the allies stay content. When you get to more complex machines, it's just a question of who/what breaks down first.

It would be better if the assembled forces could check one another by playing the role of mechanic for each other; if this were feasible, then the mechanic could withdraw and still benefit from the collective work of all the assembled elements, each conspiring with one another to fulfill the machine's goal. This would mean that, in practice, the assembled forces would move by themselves! This at first seems ludicrous, since it would mean that non-human elements would play the role of inspector, surveyor, checker, analyst and reporter in order to keep the assembled forces in line. It would mean another confusion of boundaries, the extension of social ploys to nature.

We are again so used to accepting the solution, that is hard for us to imagine how original the stratagems that generated automats were. For instance, in the earlier Newcomen steam engine, the piston followed the condensing steam, pushed by atmospheric pressure, that was thus made to lend its strength to the pump that extracted the water, that flooded the coal mine, that made the pit useless.21 A long series of associations, like those discussed in Part A, were made that linked the fate of coal mines to the weight of the atmosphere through the steam engine. The point here is that, when it reached the end of the cylinder, a new flow of steam had to be injected through a valve opened by a worker who then closed it again when the piston reached the top of its stroke. But why leave the opening and closing of the valve to a weary, underpaid and unreliable worker, when the piston moves up and down and could be made to tell the valve when to open and when to close? The mechanic who linked the piston with a cam to the valve transformed the piston into its own inspector—the story is that he was a tired, lazy boy. The piston is more reliable than the boy since it is, via the cam, directly interested, so to speak, in the right timing of the flow of steam. Certainly, it is more directly interested than any human being. An automatism is born, one of the first in a long series.

The engineer's ability lies in multiplying the tricks that make each element interested in the working of the others. These elements may be freely chosen among human or non-human actors.22 For instance, in the early British cotton-spinning industry, a worker was attached to the machine in such a way that any failure of attention resulted not in a small deficiency in the product that could be hidden, but in a gross and obvious disruption which led to a loss of piecework earnings. In this case, it is part of the machine that is used to supervise the worker. A system of pay, detection of error, a worker, a cotton-spinning machine, were all tied together in order to transform the whole lash-up into a smoothly running automaton. The assembly of disorderly and unreliable allies is thus slowly turned into something that closely resembles an organised whole. When such a cohesion is obtained we at last have a black box.

Up to now I have used this term both too much and too loosely to mean either a well-estabilshed fact or an unproblematic object. I could not define it properly before we had seen the final machinations that turn a gathering of forces into a whole that then may be used to control the behaviour of the enrolled-groups. Until it can be made into an automaton, the elements that the fact-builder want to spread in time and space is not a black box. It does not act as one. It can be dissociated, dismantled, renegotiated, reappropriated. The Kodak camera is made of bits and pieces, of wood, of steel, of coating, of celluloid. The semi-professionals of the time open up their camera and do their own coating and developing, they manufacture their own paper. The object is dismembered each time a new photograph is taken, so that it is not one but rather a bunch of disconnected resources that others may plunder. Now the new Kodak automatic cannot be opened without going wrong. It is made up of many more parts and it is handled by a much more complex commercial network, but it acts as one piece. For the newly convinced user it is one object, no matter how many pieces there are in it and no matter how complex the commercial system of the Eastman Company is. So it is not simply a question of the number of allies. Numbers unified whole. However, with automatism, a large number of elements is made to act as one, and Eastman benefits from the whole assembly. When many elements are made to act as one, this is what I will now call a black box.

It is now understandable why, since the beginning of this book, no distinction has been made between what is called a 'scientific' fact and what is called a 'technical' object or artefact. This division, although traditional and convenient, artificially cuts through the question of how to ally oneself to resist controversies. The problem of the builder of 'fact' is the same as that of the builder of 'objects': how to convince others, how to control their behaviour, how to gather sufficient resources in one place, how to have the claim or the object spread out in time and space. In both cases, it is others who have the power to transform the claim or the object into a durable whole. Indeed, as we saw previously (Chapter 2) each time a fact starts to be undisputed it is fed back to the other laboratoris as fast as possible. But the only way for new undisputed facts to be fed back, the only way for a whole stable field of science to be mobilised in other fields, is for it to be turned into an automaton, a machine, one more piece of equipment in a lab, another black box. Technics and sciences are so much the same phenomenon that I was right to use the same term black box, even loosely, to designate their outcome.

Yet, despite this impossibility of distinguishing between science and technics, it is still possible to detect, in the process of enrolling allies and controlling their behaviour, two moments that will allow the reader to remain closer to common sense by retaining some difference between 'science' and 'technology'. The first moment is when new and unexpected allies are recruited—and this is most often visible in laboratories, in scientific and technical literature, in heated discussions;
the second moment is when all the gathered resources are made to act as one unbreakable whole—and this is more often visible in engines, machines and pieces of hardware. This is the only distinction that may be drawn between 'sciences' and 'technics' if we want to shadow scientists and engineers as they build their subtle and versatile alliances.

Part C

The model of diffusion versus the model of translation

The task of the fact-builders is now clearly outlined: there is a set of strategies to enlist and interest the human actors, and a second set to enlist and interest the non-human actors so as to hold the first. When these strategies are successful the fact which has been built becomes indispensable; it is an obligatory passage point for everyone if they want to pursue their interests. From a few helpless people occupying a few weak points they end up controlling strongholds. Everyone happily borrows the claims or the prototypes from the successful contenders' hands. As a result, claims become well-established facts and prototypes are turned into routinely used pieces of equipment. Since the claim is believed by one more person, the product bought by one more customer, the argument incorporated in one more article or textbook, the black box encapsulated in one more engine, they spread in time and space.

If everything goes well it begins to look as if the black boxes were effortlessly gliding through space as a result of their own impetus, that they were becoming durable by their own inner strength. In the end, if everything goes really well, it seems as if there are facts and machines spreading through minds, factories and households, slowed down only in a handful of far-flung countries and by a few dimwits. Success in building black boxes has the strange consequence of generating these UFOs: the 'irreversible progress of science', the 'irresistible power of technology', more mysterious than flying saucers floating without energy through space and lasting for ever without ageing or decaying! Is this a strange consequence? Not for us since, in each chapter, we have learned to recognise the yawning gap that separates ready made science from science in the making. Once more, our old friend Janus is talking two languages at once: the right side is speaking in terms of translations about still undecided controversies, while the left side speaks of established facts and machines with the language of diffusion. If we want to benefit from our travels through the construction sites of science, it is crucial for us to distinguish between the two voices.

(1) Vis inertia . . .

In our examples we observed that the chain of people who borrowed claims varied from time to time because of the many elements the claims were tied to. If people wished to open the boxes, to renegotiate the facts, to appropriate them, masses of allies arrayed in tiers would come to the rescue of the claims and force the dissenters into assent; but the allies will not even think of disputing the claims, since this would be against their own interests which the new objects have so neatly translated. Disinterest has been made unthinkable. At this point, these people do not do anything more to the objects, except pass them along, reproduce them, buy them, believe them. The result of such smooth borrowing is that there are simply more copies of the same object. This is what happened to the double-helix after 1952, to the Eclipse MV/8000 after 1982, to Diesel's engine after 1914, to the Curie's polonium after 1900, to Pasteur's vaccine after 1881, to Guillemin's GRF after 1982. So many people accept them that they seem to flow as effortlessly as the voice of Alexander Bell through the thousands of miles of the new transcontinental line, even though his voice is amplified every thirteen miles and completely broken down and recomposed six times over! It also seems that all the work is now over. Spewed out by a few centres and laboratories, new things and beliefs are emerging, free floating through minds and hands, populating the world with replicas of themselves.

I will call this description of moving facts and machines the diffusion model. It has a number of strange characteristics which, if taken seriously, make the argument of this book exceedingly difficult to grasp.

First, it seems that as people so easily agree to transmit the object, it is the object itself that forces them to assent. Is it then that the behaviour of people is caused by the diffusion of facts and machines? It is forgotten that the obedient behaviour of people is what turns the claims into facts and machines; the careful strategies that give the object the contours that will provide assent are also forgotten. Cutting through the many machiavellian strategies of this chapter, the model of diffusion invents a technical determinism, paralleled by a scientific determinism. Diesel's engine leaps with its own strength at the consumer's throat, irresistibly forcing itself into trucks and submarines, and as to the Curies' polonium, it freely pollinates the open minds of the academic world. Facts now have a vis inertia of their own. They seem to move even without people. More fantastic, it seems they would have existed even without people at all.

The second consequence is as bizarre as the first. Since facts are now endowed with an inertia that does not depend on the action of people on or on that of their many non-human allies, what propels them? To solve this question adepts of the diffusion model have to invent a new mating system. Facts are supposed to reproduce another! Forgotten are the many people who carry them from hand to hand, the crowds of acting entities that shape the facts and are shaped by them, the complex negotiations to decide which association is stronger or weaker; forgotten are the three chapters above, as from now on we reach the realm of ideas begetting ideas begetting ideas. Despite the fact that it is hard to picture Diesel's engines or bicycles or atomic plants reproducing themselves through mating, trajectories (see p.107) are drawn that look like lineages and genealogies
of ‘purely technical’ descent. The history of ideas, or the conceptual history of science, or epistemology, these are the names of the discipline—that often should be X-rated—that explains the obscure reproduction habits of these pure breeds.

The problem with the mating system of facts that diffuse through their own force is novelty. Facts and machines are constantly changing and are not simply reproduced. Nobody shapes science and technologies except at the beginning, so, in the diffusion model, the only reasonable explanation of novelty lies with the initiators, the first men and women of science. Thus, in order to reconcile inertia and novelty the notion of discovery has been invented; what was there all along (microbes, electrons, Diesel’s engine) needs a few people, not to shape it, but to help it to appear in public.23 This new bizarre ‘sexual reproduction’ is made half by a history of ideas and half by a history of great inventors and discoverers, the Diesels, the Pasteurs, the Curies. But then there is a new problem. The initiators, in all the stories I have told, are only a few elements in a crowd. They cannot be the cause of such a general movement. In particular, they cannot be the cause of the people who believe them and are interested in their claims! Pasteur has not enough strength to propel his vaccine across the world, nor Diesel his engine, nor Eastman his Kodak. This is not a problem for our ‘diffusionists’. They simply make the inventors so big that they now have the strength of giants with which to propel all these things! Blown out of proportion, great men and women of science are now geniuses of mythological size. What neither Pasteur nor Diesel could do, these new figures also named ‘Pasteur’ and ‘Diesel’ can. With their fabulous strength it is a cinch for these Supermen to make facts hard and machines efficient!

Great initiators have become so important for the diffusion model that its advocates, taken in by their own maniac logic, have now to ferret out who really was the first. This quite secondary question becomes crucial here since the winner takes all. The question of how to allocate influence, priority and originality among great scientists is taken as seriously as that of discovering the legitimate heir of an empire! Labels of ‘precursor’, or ‘unknown genius’, or ‘marginal figure’, or ‘catalyst’, or ‘driving force’ are the object of punctilios as ornate as etiquette at Versailles at the time of Louis XIV; historians rush forward to provide genealogies and coats of arms. The secondary mechanism takes precedence over the primary mechanism.

The funniest thing about this fairy tale is that, no matter how carefully these labels are attributed, the great men and women of science are always a few names in a crowd that cannot be annihilated even by the most enthusiastic advocates of the diffusion model. Diesel, as we saw, did not make everything of the engine that bears his name. Pasteur is not the one that made asepsis a workable practice, or stopped millions of people from spitting, or distributed the doses of vaccine. Even the most fanatic diffusionists have to grant that. However this does not bother them. Going further and further into their fantasies, they invent geniuses who did it all, but only ‘in the abstract’, only ‘seminally’, only ‘in theory’.

Sweeping away the crowds of actors, they now picture geniuses that have ideas. The rest, they argue, is mere development, a simple unfolding of the ‘original principles’ that really count. Thousands of people are at work, hundreds of thousands of new actors are mobilised in these works, but only a few are designated as the motors that move the whole thing. Since it is obvious that they did not do that much, they are endowed with ‘seminal ideas’. Diesel ‘had the idea’ of his engine, Pasteur ‘had the idea of asepsis’. It is ironic to see that the ‘ideas’, which are so valued when people talk of science and technology, are a trick to get away from the absurd consequences of the diffusion model, and to explain—away how it is that the few people who did everything nevertheless did so little.

The model of diffusion would be rather quaint and insignificant if it were not for its final consequence which is taken seriously even by those who are willing to study the inner workings of technoscience.

Attentive readers who accept what we have argued so far might think it is easy to question the diffusion model. If the interpretation given by the model is ludicrous, the impression from which it springs is genuine. It seems to work in the few cases when facts and artefacts convince people, and, for this reason, seem to flow. Thus, readers may think that the diffusion model will break apart when the facts are interrupted, deflected, ignored or corrupted. The action of many people will necessarily irrupt into the picture, since there is no one at hand to ‘diffuse’ the facts any more. Well, if they think so, it simply means that these readers are still naive and that they underestimate the ability of an interpretation to hold out against all contrary evidences. When a fact is not believed, when an innovation is not taken up, when a theory is put to a completely different use, the diffusion model simply says that ‘some groups resist’.

In the story of Pasteur, for instance, adepts of the diffusion model have to admit that physicians were not very interested in his results; they thought that these were premature, unscientific, and of little use. Indeed, they did not have much use for vaccines since preventive medicine was taking business away from them. Instead of looking at how the research program of the Institut Pasteur was being constantly modified by dozens of people in order to convince almost every physician, the diffusion model simply says that Pasteur’s ideas were blocked by certain groups which were stupid or had ‘vested interests’ in older techniques. They picture the physicians as corporatists, as selfish, as a backward and reactionary group, that slowed down the spread of Pasteur’s idea for a generation. So the diffusion model traces a dotted line along the path that the ‘idea’ should have followed, and then, since the idea did not go very far and very fast, they make up groups that resist. With this last invention, both the principle of inertia and the fantastic force that triggers it at the beginning are maintained, and the gigantic stature of the great men and women that gave momentum to the whole is amplified. Diffusionists simply add passive social groups to the picture that may, because of their own inertia, slow down the path of the idea or absorb the impact of the technics. In other words, the diffusion model now invents a
society to account for the uneven diffusion of ideas and machines. In this model, society is simply a medium of different resistances through which ideas and machines travel. For instance, the Diesel engine that has spread through the developed countries because of the momentum given to it by Diesel might slow down or even stop in some underdeveloped country where it rusts on a dock in the tropical rain. In the diffusion model, this would be accounted for in terms of the resistance, the passivity or the ignorance of the local culture. Society or 'social factors' would appear only at the end of the trajectory, when something went wrong. This has been called the principle of asymmetry: there is appeal to social factors only when the true path of reason has been 'distorted' but not when it goes straight. 24

The society invented to maintain the diffusion model has another strange characteristic. The 'groups' that make it up do not always interrupt or deflect the normal and logical path of ideas; they may suddenly switch from being resistors or semiconductors to conductors. For instance, the same physicians who were not very happy with Pasteur until 1894 then became all of a sudden interested in the Pasteurians' work. This is not a difficulty in the diffusion model: they simply altered their position. They switched open. The resistors began to conduct, the reactionaries to progress, from being backward they suddenly moved forward! You see that there is no limit to the fairy tale. Forgotten is the careful co-production between Pasteurians and physicians of a new object, a serum against diphtheria that, unlike the preventive vaccine, was at last one that helped to cure. The long translations necessary to convince horses, diphtheria, hospitals and physicians to associate with one another in this new object are forgotten. Cutting across the complicated systems of associations, the diffusion model simply extracts a serum—that was there all along, at least 'in principle'—and then invents groups which at first resisted and finally 'turned out' to accept the discovery.

(2) Weaker and stronger associations

Let us go back to Diesel in order to understand the differences between the diffusion model and the translation model. We saw that Diesel's engine was a sketch in his patent, then a blueprint, then one prototype, then a few prototypes, then nothing, then again a single new prototype, then no longer a prototype but a type that was reproducible in several copies, then thousands of engines of different sub-types. So there was indeed a proliferation. First, following the translations, we learned that this increase in the number of copies had to be paid for by an increase in the number of people made to be interested in its fate. Second, we realised that this increase in copies and people had to be obtained through a deep transformation of the design and principles of the engine; the engine moved, but it was not the same engine. Third, we learnt that it had been transformed so much during the translation that there was a dispute about whose engine it actually was. And fourth, we saw that in about 1914 there had been a point when people could accept the engine not as a prototype but as a copy, and take it away from the Augsburg shop without deeply transforming it or dragging with them dozens of mechanics and patent lawyers; the engine was a black box for sale at last and it was able to interest not only engineers and researchers but also 'simple customers'. It is at this point that we left the story, but it is also at this point that the diffusion model seems better than the translation one because no one is necessary any more to shape the black box. There exist only customers who buy it.

How simple is a 'simple customer'? The customer is 'simple' because he or she does not have to redesign the engine by shifting from air injection back to solid injection, or moving the valves around, or boring new cylinders and running the engine on the test bench. But the customer cannot be so 'simple' as not to tend the engine, feeding it oil and fuel, cooling it, overhauling it regularly. Even when the phases of development and innovation have ended, the big black box still has to be maintained in existence by not so simple customers. We can easily picture endless situations in which an ill-informed or a stupid consumer makes one engine falter, or stall or blow apart. As engineers say, no device is idiot-proof. This particular copy of the engine at least will not run any more, but will slowly rust.

There is another problem with 'simple' customers. Let us remember Eastman's Kodak camera. It was simpler to operate than anything before. 'Push the button, we'll do the rest,' they said. But they had to do the rest, and that was quite a lot. The simplification of the camera that made it possible to interest everyone in its dissemination in millions of copies had to be obtained by the extension and complication of Eastman's commercial network. When you push the button you do not see the salesmen and the machinery that make the long strips of celluloid films and the trouble-shooters that make the coating stick properly at last; you do not see them, but they have to be there none the less. If they are not, you push the button and nothing happens. The more automatic and the blacker the black box is, the more it has to be accompanied by people. In many situations, as we all know all too well, the black box stops pitifully because there is no salesperson, no repairer, no spare part. Every reader who has lived in an underdeveloped country or used a newly developed machine will know how to evaluate the hitherto unknown number of people necessary to make the simplest device work! So, in the most favourable cases, even when it is a routine piece of equipment, the black box requires an active customer and needs to be accompanied by other people if it is to be maintained in existence. By itself it has no inertia.

If we have understood this, then we may draw the conclusions from the two first parts of this chapter: the black box moves in space and becomes durable in time only through the actions of many people; if there is no one to take it up, it stops and falls apart however many people may have taken it up for however long before. But the type, the number and the qualifications of the people in the chain will be modified: inventors like Diesel or Eastman, engineers, mechanics, salesmen, and maybe 'ignorant consumers' in the end. To sum up, there are
always people moving the objects along but they are not the same people all along. Why are they not the same? Because the first ones have tied the engine's fate to other elements so that the engine may be put in different hands and more easily spread. You will then see a few copies of the Diesel engine slowly move through its constant redesign at the test bench, and suddenly you will observe many copies of the same design that are bought and sold by many people. There are always people, but they are not the same. Thus, the diesel engine story may be analysed either by looking at the changing shape of the engine—tied to different people—or by looking at the changing type of people—linked to the engine. It is the same story viewed either from the standpoint of the enrolled people of Part A or from the enrolling things of Part B.

Similarly, the Curie’s’ polonium was first a claim redesigned after every trial in a single laboratory in Paris in 1898. To convince dissenters that this was indeed a new substance, the Curie’s had to modify the trials and renegotiate the definition of their object. For each suspicion that it might be an artefact, they devised a trial that linked its fate to a more remote and less disputable part of physics. There is a moment in this story when the claim becomes a new object, and even a part of Nature. At this point the type of people necessary to provide the fact with durability and extension is to be modified. Polonium may now travel from the Curie’s hands into many more, but much less informed, hands. It is now a routine radioactive element in a sturdy lead container, one more box filled up in freshly printed versions of the periodic table; it is no longer believed by only a few bright sparks in a few laboratories, but also by hundreds of enthusiastic physicists; soon it will be learned by ‘simple students’. A continuous chain of people using, testing and believing in polonium is necessary to maintain it in existence; but they are not the same people nor are their qualifications the same. So the story of polonium—like all that have so far been told in this book—may be told either by looking at the people who are convinced, or by looking at the new associations made to convince them. It is the same analysis from two different angles since, all along, polonium is constituted by these people convinced that these associations are unbreakable.

We may now generalise a bit from what we have learned. If you take any black box and make a freeze-frame of it, you may consider the system of alliances it knits together in two different ways: first, by looking at who it is designed to enrol; second, by considering what it is tied to so as to make the enrolment inescapable. We may on the one hand draw its sociogram, and on the other its technogram. Every piece of information you obtain on one system is also information on the other. If you tell me that Diesel’s engine now has a stable shape, I will tell you how many people at MAN had to work on it and about the new system of solid injection they had to devise so that the engine might be bought by ‘mere consumers’. If you tell me that you think polonium is really bismuth (see p.38), I can tell you that you work in the Curie’s lab in Paris around 1900. If you show me a serum for diphtheria, I’ll understand how far you drifted from the original research programme that aimed at making vaccines and I’ll tell you who are the physicians who will get interested. If you show me an electric vehicle running on fuel cells, I’ll know who has to be won over in the company. If you propose to build a 16-bit computer to compete with the DEC’s VAX 11/780 machine I’ll know who, when and where you are. You are West at Data General in the late 1970s. I know this, because there are very few places on earth where anyone has the resources and the guts to disaggregate the black box DEC has assembled and to come up with a brand new make of computer. I similarly learn a lot about you if you explain to me that you are waiting for the repair man to fix your Apple computer, or that you believe the moon to be made of green cheese, or that you do not really think that the second amino acid in the GHRH structure is histidine.

Carefully take note that the black box is in between these two systems of alliances, that it is the obligatory passage point that holds the two together and that, when it is successful, it concentrates in itself the largest number of hardest associations, especially if it has been turned into an automaton. This is why we call such black boxes ‘hard facts’, or ‘highly sophisticated machines’, or ‘powerful theories’, or ‘indisputable evidence’. All these adjectives that allude to strength and power rightly point out the disproportionate number of associations gathered in these black boxes, so disproportionate indeed that they are what keep the multitude of allies in place. However this disproportion often leads us to forget that they hold things and people tightly together only as long as all the other strategies are successful. Do these products of science and techniques escape from the system of complicated alliances with which politics are managed, for instance? Are they less ‘social’ as people often naively say? Most unlikely; if they had to be qualified in these terms—which they don’t—they would have to be described as more, much more ‘social’.

If you now let the frozen-frame move, you observe a black box that simultaneously changes what it is made of and whom it is convincing. Each modification in one system of alliances is visible in the other. Each alteration in
the technogram is made to overcome a limitation in the sociogram, or vice versa. Everything happens as if the people we have to follow were in between two sets of constraints and were appealing from one to the other whenever the negotiations get stalled. On one side there are people who are either going in the same direction, or are against it, or are indifferent, or, although indifferent and hostile, may be convinced to change their minds. On the other side, there are non-human actors in all colours and shades: some are hostile, others indifferent, some are already docile and convenient, still others. although hostile or useless, may be persuaded to follow another path. The inventor of Post-it, a yellow sticky paper for marking books, which has now become so widely used, makes the point very well.

Having found a glue that does not adhere was seen as a failure in the 3-M company whose job is usualy to make very sticky glues. This failure to glue was turned to advantage when the inventor realised that it could mark Psalms books without smearing or wearing them. Unfortunately, this advantage was not admitted by the marketing department who had decided that this invention had no market and no future. Situated exactly at the middle of the technochronology of the sociograms, the inventor has a choice: either to modify the invention or to modify the marketing department. Choosing to keep the invention as it is, he then applies subtle tactics to sway the marketing department, distributing prototypes of his invention to all the secretaries, and then asking the secretaries, when they wanted more of it, to call the marketing department directly! It is the same subtlety that goes on in devising a glue that does not glue or in making a marketing department sell what they do not want to sell. Rather, Post-it is shaped by the two sets of strategies, one for enrolling others, the other to control their behaviour.

We may go a bit further. We are all multi-conductors and we can either drop, transfer, deflect, modify, ignore, corrupt or appropriate the claims that need our help if they are to spread and last. When -- very rarely -- the multi-conductors, acting as conductors, simply transmit a belief without delay and corruption, what does this mean? That many elements accompany the moving claims or objects and literally keep the successive hands necessary for their survival in line. When -- more often -- multi-conductors interrupt the spread of the claims that had until then been passed along without qualms by everyone, it also teaches us something. Since they are able to interrupt, these people must be tied to new interests and new resources that counteract the others. And the same lessons may be drawn when -- as is almost always the case -- people ignore, deflect, modify or appropriate the black boxes. Does the reader now see the conclusion? Understanding what facts and machines are is the same task as understanding who the people are. This essential tenet will constitute our third principle.

(3) The fourth rule of method

Among all the features that differ in the two models, one is especially important, that is society. In the diffusion model society is made of groups which have interests; these groups resist, accept or ignore both facts and machines, which have their own inertia. In consequence we have science and technics on the one hand, and a society on the other. In the translation model, however, no such distinction exists since there are only heterogeneous chains of associations that, from time to time, create obligatory passage points. Let us go further: belief in the existence of a society separated from technoscience is an outcome of the diffusion model. Once facts and machines have been endowed with their own inertia, and once the collective action of human and non-human actors tied together has been forgotten or pushed aside, then you have to make up a society to explain why facts and machines do not spread. An artificial division is set up between the weaker and stronger associations: facts are tied with facts; machines with machines; social factors with social factors. This is how you end up with the idea that there are three spheres of Science, Technology and Society, where the influence and impact of each on the other have to be studied.

But worse is yet to come. Now that a society has been invented by artificially cutting through the associations and the translations, and by squeezing social factors into tiny ghettos, some people try to explain science and technology by the influence of these social factors! A social or a cultural or an economic determinism is now added to the technical determinism above. This is the meaning of the word social in expressions like 'social studies of science' or the 'social construction of technology'. Analysts who use groups endowed with interests in order to explain how an idea spreads, a theory is accepted, or a machine rejected, are not aware that the very groups, the very interests that they use as causes in their explanations are the consequence of an artificial extraction and purification of a handful of links from these ideas, theories or machines. Social determinism courageously fights against technical determinism, whereas neither exist except in the fanciful description proposed by the diffusion model.

Although there is no point in spending too much time on the diffusion model it is crucial, if we wish to continue our voyage through technoscience, to be immunised against the notion that there is a society and 'social factors' able to shape, influence, direct or slow down the path of pure science and pure technics. At the end of Chapter 2, I presented our third rule of method: Nature cannot be used to account for the settlement of controversies, because it is only after the controversies have been settled that we know what side she is on. 'Nature settles only the settled claims,' so speaks the left side of our Janus who does not sense the contradiction. As for the unsettled ones on which the right side of Janus is
working, we do not yet know what settles them but it is not Nature. Nature thus lies behind the facts once they are made; never behind facts in the making.

If we want to go on without being bothered by the diffusion model, we have to offer a fourth rule of method, as basic to the third one, and symmetrical to it, which applies this time to Society.

Right from the first pages of this book the reader may have noticed the shocking absence of the entities that traditionally make up Society, an absence that may be even more shocking than the delayed appearance of Nature until the end of Chapter 2. After three chapters there has been not a word yet on social classes, on capitalism, on economic infrastructure, on big business, on gender, not a single discussion of culture, not even an allusion to the social impact of technology. This is not my fault. I suggested that we follow scientists and engineers at work and it turns out that they do not know what society is made of, any more than they know the nature of Nature beforehand. It is because they know about neither that they are so busy trying out new associations, creating an inside world in which to work, displacing interests, negotiating facts, reshuffling groups and recruiting new allies.

In their research work, they are never quite sure which association is going to hold and which one will give way. Diesel was confident at first that all fuels would ignite at high temperatures and that every group of users would be interested in his more efficient engine. But most fuels rejected his engine and most consumers lost interest. Starting from a stable state of Nature and of Society, he had to

struggle through another engine tying kerosene, air injection and a tiny number of users together. Hygienists also started with a fixed state of Society—the class struggle—and a determined state of Nature—the miasmatic diseases. When Pasteurians offered them the microbes, this was a new and unpredictable definition both of Nature and of Society: a new social link, the microbe, tied men and animals together, and tied them differently. There was nothing in the stable state of either Society or Nature that made an alliance of big business at Bell with electrons necessary or predictable. The Bell Company was deeply modified by its alliance with Millikan’s physics, it was not the same Bell, but neither was it the same physics, the same Millikan nor, indeed, the same electrons. The versatility and the heterogeneity of the alliances is precisely what makes it possible for the researchers to get over the quandary of the fact-builder: how to interest people and to control their behaviour. When we study scientists and engineers at work, the only two questions that should not be raised are: What is Nature really like? What is Society really made of?

Figure 3.6

To raise these questions we have to wait until scientists and their allies—among whom social scientists should of course be included—have finished their work! Once the controversies have ended, then a stable state of Society, together with a stable rendering of the interests of its members, will emerge. If we study all made facts and groups, then interests and Nature will be clearly articulated by the left face of Janus. Not so, when we follow facts in the making. It might seem a strange consequence but it is a necessary one: to follow scientists and engineers we do not need to know what Society is made of and what Nature is; more exactly, we need not to know them. The stable state of Society is three chapters away! The premature introduction of a fully-fledged Society would be as damaging for our trip as would a complete picture of Nature. More exactly the same arguments that have been made about Nature have to be made symmetrically about Society. How could we take so many precautions in not believing directly what scientists
and engineers say about objectivity and subjectivity, and readily believe what other scientists (social this time) say about society, culture and economy? At this point we are in great need of a rule of symmetry that does not grant Society privileges refused to Nature. Our fourth rule of method thus reads exactly like the third—the word ‘Society’ replacing the word ‘Nature’—and then fuses the two together: since the settlement of a controversy is the cause of Society’s stability, we cannot use Society to explain how and why a controversy has been settled. We should consider symmetrically the efforts to enrol and control human and non-human resources.

CHAPTER 4

Insiders Out

We now have a better idea of the amount of preliminary work necessary to secure enough strongholds to make relevant the added force offered by the technical literature and the laboratories. Without the enrolment of many other people, without the subtle tactics that symmetrically adjust human and non-human resources, the rhetoric of science is powerless. People escape, lose interest, do something else, are indifferent. Still, the stories told in the former chapter were all from the point of view of the enlisting scientists and engineers. Even if we had followed many more outcomes than the three we started with—giving up, going along, working through—we might have had the impression that scientists and engineers were at the centre of everything. This impression might create some new difficulties. Our first rule of method requires us to shadow scientists while they are engaged in their work of doing science. At face value this precept seems easy to put into practice; this is why, in all the chapters so far, I have pretended that we at least knew where to find the white-coated protagonist to begin our enquiry. But it was to simplify our trip that I took it for granted that West, Crick and Watson, Guillemin, the Professor, Diesel, Mead or Pasteur were able to gather resources, to talk with authority, to convince others of their strength and to equip laboratories or departments, thus beginning the various stories I told with fully-fledged scientists and engineers that others were taking seriously enough to grant them attention, money and confidence. To offer us a convenient departure point I invented a character whom I called the ‘dissenter’ to help us practise the difficult art of shadowing scientists in action; and indeed, since this dissenter was easy to detect and since his obstinacy made him easier to follow, it facilitated our peregrination through the technical literature and through laboratories. Later, the character of the ‘fact-builder’ was very convenient to map the various types of translations.

Nothing proves, however, that following real scientists and engineers is as easy as following these dummy dissenters or dummy fact-builders, especially when they very principles we uncovered hint at the opposite. Remember that the first basic principle states that facts are made collectively, the second that scientists and
Part A

Interesting others
in the laboratories

(1) When everyone can do without scientists or engineers

What happens to scientists and engineers who have not secured any strongholds? How strong will their rhetoric be? How capable will they be of keeping interest groups in line? Let me take two examples, one of a scientist in the past and one of an engineer in the present. In these examples no one is prepared to grant anything to the budding researchers and everyone does very well without their science.

(A) WHEN BEING A SCIENTIST IS NOT YET A JOB

In the late 1820s, Charles Lyell was reading for the Bar and living on a £400-a-year allowance from his upper-middle-class father. Lyell wished to study the 'history of the earth'. Do not jump to the conclusion that he wanted to be a geologist. Being able to be a geologist will be the result of the work of many people like Lyell. At the time there was no such thing in England as a full-time paid and secure job under the label 'geologist'. Moreover, 'geology' did not really exist either. The history of the earth pertained to the theology and biblical exegesis as well as to paleontology and other technical subjects. In other words, neither the discipline of geology nor the profession of geologist existed. One of the related and firmly established disciplines was that of the 'rational history of creation' and one of the related trades is a six-century-old profession, that of cleric in the universities - with compulsory celibacy, at least at Cambridge.

When he starts, there is no laboratory which Lyell can enter, no curriculum to follow and no grant for which to apply. Although Lyell needs others to help him build new and harder facts these 'others' are following different tracks. Can Lyell count on the dons and clerics of Oxford who teach the history of the earth and who have the libraries, the authority and the tenures? Not at all, because, if a controversy is triggered about, say, the age of the earth, Lyell's colleagues may very well interrupt his argument by appealing to God's word or to the Church's perennial teachings. Even if the dons Lyell is addressing are interested in a rational history of the earth and have agreed to talk about rocks and erosion without bringing in the location of the Garden of Eden, the size of Noah's Ark or the date of the Flood, what will happen if the controversy heats up a bit? Not much, simply because these colleagues have taken the chair as a first step toward becoming either bishop or teacher of a more prestigious subject, like ethics. No matter how many arguments Lyell has been able to muster in defence of his position, his opponents are in no way forced to take up his point. They may simply ignore him, or brush the arguments aside, or listen with bewilderment and go on teaching their usual course. For the disserter to exist more work has to be done.

The same thing might happen if Lyell sets up a controversy with the miscellaneous groups of people who write 'theories of the earth' on the side, but who do not make a living from geology, that is the amateurs. Many amateurs were busy at the time gathering rocks and fossils, visiting foreign landscapes, offering all sorts of reports to the many societies recently created to gather new collections. By definition, an amateur, even a devoted and a passionate one, may leave the discussion whenever it pleases him. So it is very hard for Lyell to win an argument and to force the amateur to borrow his claims as a black box, especially if they run against his feelings, interests and passion. Unconvinced, the amateurs may go on as usual, uninterested and unthreatened by the many allies that Lyell may have assembled in support of his position. Although they are necessary to collect the rocks and the fossils in many places where the few geologists could not possibly go, the amateurs form a most undisciplined crowd as far as helping Lyell produce new facts goes.

The situation would be much better for Lyell if the clerics would give up their chairs in universities and hand them over to people with no other ambitions than to stay inside geology all their lives. Geology would then become a career. When Lyell makes a point, his colleagues would have to either defeat him or accept it because they would have no other way to go. They could no longer ignore him or do something else such as becoming a bishop. It would also be better if the amateurs were still busy gathering materials and providing reports, but were not meddling in the debates. They would be forced to bring in their specimens, to offer their collections, but they would stay outside without adding their own commentaries and theories. A disordered crowd of helping hands would then become a disciplined workforce helping geologists produce more documented facts. Slowly an inside pocket of purely geological matters would be carved out of
CHAPTER 5

Tribunals of Reason

In the first part of this book we studied how to go from a weak rhetoric to a strong one, and in the second we followed the scientists and engineers in their many strategies as they go from weak points to the occupation of strongholds. If we wanted to summarise the first four chapters, we could say that they showed a fantastic increase in the number of elements tied to the fate of a claim – papers, laboratories, new objects, professions, interest groups, non-human allies – so many, indeed that if one wished to question a fact or to bypass an artefact one might be confronted by so many black boxes that it would become an impossible task: the claim is to be borrowed as a matter of fact, and the machine or the instrument put to use without further ado. Reality, that is what resists all efforts at modification, has been defined, at least for the time being, and the behaviour of some people has been made predictable, in certain ways at least.

Another way of summarising the same four chapters is to show the other side of the coin: such an increase in the number of elements tied to a claim is to be paid for and that makes the production of credible facts and efficient artefacts a costly business. This cost is not to be evaluated only in terms of money, but also by the number of people to be enrolled, by the size of the laboratories and of the instruments, by the number of institutions gathering the data, by the time spent to go from ‘seminal ideas’ to workable products, and by the complication of mechanisms piling black boxes onto one another. This means that shaping reality in this way is not within everybody’s reach, as we saw at length in Chapter 4.

Since the proof race is so expensive that only a few people, nations, institutions or professions are able to sustain it, this means that the production of facts and artefacts will not occur everywhere and for free, but will occur only at restricted places at particular times. This leads to a third way of summarising what we have learned in this book so far, a way that fuses together the two first aspects: technoscience is made in relatively new, rare, expensive and fragile places that garner disproportionate amounts of resources; these places may come to occupy strategic positions and be related with one another. Thus, technoscience may be described simultaneously as a demiurgic enterprise that multiplies the number of
allies and as a rare and fragile achievement that we hear about only when all the other allies are present. If technoscience may be described as being so powerful and yet so small, so concentrated and so dilute, it means it has the characteristics of a network. The word network indicates that resources are concentrated in a few places—the nodes and the nodes—which are connected with one another—the links and the mesh: these connections transform the scattered resources into a net that may seem to extend everywhere. Telephone lines, for instance, are minute and fragile, so minute that they are invisible on a map and so fragile that each may be easily cut; nevertheless, the telephone network ‘covers’ the whole world. The notion of network will help us to reconcile the two contradictory aspects of technoscience and to understand how so few people may seem to cover the world.

The task before us in the last part of this book is to explore all the consequences that this definition of technoscience as a network entails. The first question I will tackle concerns the people who are not part of the networks, who fall through the mesh of the net. So far, we have followed scientists and engineers at work; it is necessary for a while to turn our attention towards the multitudes who do not do science in order to evaluate how difficult it is for scientists to enrol them. Given the tiny size of fact production, how does the rest of humanity deal with ‘reality’? Since for most of history this peculiar system of convincing did not exist, how did the human race manage for so long without it? Since even in modern industrialised societies the vast majority does not get close to the process of negotiation of facts and artefacts, how do they believe, prove and argue? Since in most enterprises, there has been no scientist or engineer to occupy obligatory passage points, how do ordinary folk go about their daily business without science? In short, the question we have to study in this chapter is what is in between the mesh of the networks; then, in Chapter 6 we will tackle the question of how the networks are sustained.

Part A
The trials of rationality

(1) Peopling the world with irrational minds

How do the multitudes left out of the networks see the scientists and the engineers, and how do they themselves consider the outside of these networks? Take for example the case of weather forecasts. Every day, often several times a day, many millions of people talk about the weather, make predictions, cite proverbs, inspect the sky. Among them, a large proportion listen to weather forecasts or glance at satellite maps of their countries on TV and in newspapers; quite often, people make jokes about weathermen who are, they say, ‘always wrong'; many others, whose fate has been linked earlier to that of meteorologists, anxiously await forecasts before taking decisions about seeding plants, flying planes, fighting battles or going out for picnics. Inside the weather stations, running the huge data banks fed with satellite signals, controlling the reports of the many part-time weathermen scattered over the planet, sending balloons to probe the clouds, submitting computer models of the climate to new trials, a few thousand meteorologists are busy at work defining what the weather is, has been and will be. To the question ‘what will the weather be tomorrow?’ you get, on one side, billions of scattered commentaries and, on the other, a few claims confronted with one another through the teleexes of the international Meteorological Association. Do these two sets of commentaries have a common ground? Not really, because, on the one hand, the few claims of the meteorologists are utterly lost among billions of jokes, proverbs, evaluations, gut feelings and readings of subtle clues; and because, on the other hand, when time comes to define what the weather has been, the billions of other utterances about it count for nothing. Only a few thousand people are able to define what the weather is; only their opinions literally count when the question is to allocate the huge funds necessary to run the networks of computers, instruments, satellites, planes, ships and satellites that provide the necessary data.

This situation creates a rather curious balance account: the weather and its evolution is defined by everyone on earth and the few weathermen provide only a few scattered opinions among the multitudes of opinion, taken more seriously in only small sectors of the public—the military, the ship and air companies, agricultural concerns, tourists. However, when you put all these opinions in one balance of the scale and in the other the few claims of the meteorologists, the balance tips on the side of the latter. No matter how many things are said about the weather, no matter how many jokes are made about the weathermen, the weather of the weathermen is strong enough to discount all the other weather. If you ask the question ‘was it a normal summer or an exceptionally hot one?’ although everyone says, everyone feels that it has been a hot summer, the lived opinions of the multitude may be discounted inside the networks of the International Meteorological Association. ‘No,’ they say, ‘it was a summer only 0.01 degree above average.’ The certitudes of billions of people have become mere opinions about the weather whose essence is defined by the few thousand meteorologists. ‘You believed it was a hot summer, but it was really an average one.’

The balance of forces may be tipped in one direction or in another depending on whether we are inside or outside the network developed by weathermen. A handful of well-positioned men of science may rout billions of others. This will happen only, however, as long as they stay inside their own networks, because, no matter what the meteorologists think and do, every one of us will still think it was a hot summer and make jokes, the morning after, about the weather forecasts which were ‘wrong as usual’. This is where the notion of network is useful: meteorology ‘covers’ the world’s weather and still leaves out of its mesh almost every one of us. The problem for the meteorologists will then be to extend their networks, to make their predictions indisputable, to render the passage through
CHAPTER 6

Centres of Calculation

Prologue
The domestication of the savage mind

At dawn, 17 July 1787, Lapérouse, captain of L' Astrolabe, landed at an unknown part of the East Pacific, on an area of land that was called 'Segalien' or 'Sakhalin' in the older travel books he had brought with him. Was this land a peninsula or an island? He did not know, that is no one in Versailles at the court of Louis XVI, no one in London, no one in Amsterdam in the headquarters of the West Indies Company, could look at a map of the Pacific Ocean and decide whether the engraved shape of what was called 'Sakhalin' was tied to Asia or was separated by a strait. Some maps showed a peninsula, others showed an island; and a fierce dispute had ensued among European geographers as to how accurate and credible the travels books were and how precise the reconnaissances had been. It is in part because there were so many of these disputes - similar to the profusion we studied in Part I - on so many aspects of the Pacific Ocean, that the king had commissioned Lapérouse, equipped two ships, and ordered him to draw a complete map of the Pacific.¹

The two ships had been provided, as scientific satellites are today, with all the available scientific instruments and skill; they were given better clocks to keep the time, and thus measure the longitude more accurately; they were given compasses to measure the latitude; astronomers had been enlisted to mend and tend the clocks and to man the instruments; botanists, mineralogists and naturalists were on board to gather specimens; artists had been recruited to sketch and paint pictures of those of the specimens that were too heavy or too fragile to survive the return trip; all the books and travel accounts that had been written on the Pacific had been stocked in the ship's library to see how they compared with what the travellers would see; the two ships had been loaded with goods and bargaining chips in order to evaluate all over the world the relative prices of gold, silver, pelts, fish, stones, swords, anything that could be bought
and sold at a profit, thus trying out possible commercial routes for French shipping.

This morning in July, Lapérouse was very surprised and pleased. The few savages—a male—that had stayed on the beach and exchanged salmon for pieces of iron were much less 'savage' than many he had seen in his two years of travel. Not only did they seem to be sure that Sakhalin was an island, but they also appeared to understand the navigators' interest in this question and what it was to draw a map of the land viewed from above. An older Chinese sketched on the sand the country of the 'Mantchéoux', that is, China, and his island; then he indicated with gestures the size of the strait separating the two. The scale of the map was uncertain, though, and the rising tide soon threatened to erase the precious drawing. So, a younger Chinese took up Lapérouse's notebook and pencil and drew another map noting the scale by little marks, each signifying a day of travel by canoe. They were less successful in indicating the scale for the depth of the strait; since the Chinese had little notion of the ship's draught, the navigators could not decide if the islanders were talking of relative or of absolute size. Because of this uncertainty, Lapérouse, after having thanked and rewarded these most helpful informants, decided to leave the next morning and to sight the strait for himself, and, hopefully, to cross it and reach Kamchatka. The fog, adverse winds and bad weather made this sight impossible. Many months later, when they finally reached Kamchatka, they had not seen the strait, but relied on the Chinese to decide that Sakhalin was indeed an island. De Lesseps, a young officer, was asked by Lapérouse to carry the maps, the notebooks and the astronomical bearings they had gathered for two years back to Versailles. De Lesseps made the trip on foot and on horseback under the protection of the Russians, carrying with him these precious little notebooks; one entry among thousands in the notebooks indicated that the question of the Sakhalin island was settled and what the probable bearing of the strait was.

This is the kind of episode that could have been put to use, at the beginning of Chapter 5, in order to make the Great Divide manifest. At first sight, it seems that the differences between Lapérouse's enterprise and those of the natives is so colossal as to justify a deep distinction in cognitive abilities. In less than three centuries of travels such as this one, the nascent science of geography has gathered more knowledge about the shape of the world than had come in millennia. The implicit geography of the natives is made explicit by geographers; the local knowledge of the savages becomes the universal knowledge of the cartographers; the fuzzy, approximate and ungrounded beliefs of the locals are turned into a precise, certain and justified knowledge. To the partisans of the Great Divide, it seems that going from ethnography to geography is like going from childhood to adulthood, from passion to reason, from savagery to civilisation, or from first degree intuitions to second degree reflexion.

However, as soon as we apply the sixth rule of method, the Great Divide disappears and other little differences become visible. As I showed in the last chapter, this rule asks us not to take a position on rationality, but simply to consider the movement of the observer, its angle, direction and scale.

Lapérouse crosses the path of the Chinese fishermen at right angles; they have never seen each other before and the huge ships are not here to settle. The Chinese have lived here for as long as one can remember whereas the French fleet remains with them for a day. These families of Chinese, as far as one can tell, will remain around for years, maybe centuries; L'Astrolabie and La Boussole have to reach Russia before the end of the summer. In spite of this short delay, Lapérouse does not simply cross the path of the Chinese ignoring the people on shore. On the contrary, he learns from them as much as he can, describing their culture, politics and economics — after one day of observation! — sending his naturalists all over the forest to gather specimens, scribble notes, take the bearings of stars and planets. Why are they all in a hurry? If they were interested in the island could they not stay longer? No, because they are not so much interested in this place as they are in bringing this place back first to their ship, and second to Versailles.

But they are not only in a hurry, they are also under enormous pressure to gather traces that have to be of a certain quality. Why is it not enough to bring back to France personal diaries, souvenirs and trophies? Why are they all so hard-pressed to take precise notes, to obtain and double-check vocabularies from their informants, to stay awake at night writing down everything they have heard and seen, labelling their specimens, checking for the thousandth time the running of their astronomical clocks? Why don't they relax, enjoy the sun and the tender flesh of the salmon they catch so easily and cook on the beach? Because the people who sent them away are not so much interested in their coming back as they are in the possibility of sending other fleets later. If Lapérouse succeeds in his mission, the next ship will know if Sakhalin is a peninsula or an island, how deep the strait is, what the dominant winds are, what the mores, resources and culture of the natives are before sighting the land. On 17 July 1787, Lapérouse is weaker than his informants; he does not know the shape of the land, does not know where to go; he is at the mercy of his guides. Ten years later, on 5 November 1797 the English ship Neptune on landing again at the same bay will be much stronger than the natives since they will have on board maps, descriptions, log books, nautical instructions — which to begin with will allow them to know that this is the 'same' bay. For the new navigator entering the bay, the most important features of the land will all be seen for the second time — the first time was when reading in London Lapérouse's notebooks and considering the maps engraved from the bearings De Lesseps brought back to Versailles.

What will happen if Lapérouse's mission does not succeed? If De Lesseps is killed and his precious treasure scattered somewhere on the Siberian tundra? Or if some spring in the nautical clocks went wrong, making most of the longitudes unreliable? The expedition is wasted. For many more years a point on the map at the Admiralty will remain controversial. The next ship sent away will be as weak as L'Astrolabe, sighting the Segaliens (or is it Sakhalin?) island (or is it a peninsula?) for the first time, looking again for native informants and guides; the divide will remain as it is, quite small since the frail and uncertain crew of the
Neptuna will have to rely on natives as poor and frail as them. On the other hand, if the mission succeeds, what was at first a small divide between the European navigator and the Chinese fishermen will have become larger and deeper since the Neptuna crew will have less to learn from the natives. Although there is at the beginning not much difference between the abilities of the French and the Chinese navigators, the difference will grow if Lapérouse is part of a network through which the ethnography of the Pacific is accumulated in Europe. An asymmetry will slowly begin to take shape between the ‘local’ Chinese and the ‘moving’ geographer. The Chinese will remain savage (to the European) and as strong as the Neptuna crew, if Lapérouse’s notebooks do not reach Versailles. If they do, the Neptuna will be better able to domesticate the Chinese since everything of their land, culture, language and resources will be known on board the English ship before anyone says a word. Relative degrees of savagery and domestication are obtained by many little tools that make the wilderness known in advance, predictable.

Nothing reveals more clearly the ways in which the two groups of navigators talk at cross purposes, so to speak, than their interest in the inscription. The accumulation that will generate an asymmetry hinges upon the possibility for some traces of the travel to go back to the place that sent the expedition away. This is why the officers are all so much obsessed by bearings, clocks, diaries, labels, dictionaries, specimens, herbaries. Everything depends on them: L’Astrolabe can sink provided the inscriptions survive and reach Versailles. This ship travelling through the Pacific is an instrument according to the definition given in Chapter 2. The Chinese, on the other hand, are not all that interested in maps and inscriptions – not because they are unable to draw them (on the contrary their abilities surprise Lapérouse very much) but simply because the inscriptions are not the final goal of their travel. The drawings are no more than intermediaries for their exchanges between themselves, intermediaries which are used up in the exchange and are not considered important in themselves. The fishermen are able to generate these inscriptions at will on any surface like sand or even on paper when they meet someone stupid enough to spend only a day in Sakhalin who nevertheless wishes to know everything fast for some other unknown foreigner to come back later and safer. There is no point in adding any cognitive difference between the Chinese navigators and the French ones; the misunderstanding between them is as complete as between the mother and the child in Chapter 5 and for the same reason: what is an intermediary of no relevance has become the beginning and the end of a cycle of capitalisation. The difference in their movement is enough and the different emphasis they put on inscriptions ensues. The map drawn on sand is worthless for the Chinese who do not care that the tide will erase it; it is a treasure for Lapérouse, his main treasure. Twice, in his long travels, the captain was fortunate enough to find a faithful messenger who brought his notes back home. De Lesseps was the first: Captain Phillip, met at Botany Bay in Australia in January 1788, was the second. There was no third time. The two ships disappeared and the only traces that were found, well into the nineteenth century, were not maps and herbariums, but the hilt of a sword and a piece of the stern with a fleur-de-lis on it, that had become the door of a savage’s hut. On the third leg of their journey the French navigators had not been able to domesticate the savage lands and peoples; consequently, nothing is known with certainty about this part of their voyage.

Part A
Action at a distance

(1) Cycles of accumulation

Can we say that the Chinese sailors Lapérouse met did not know the shape of their coasts? No, they knew it very well; they had to since they were born there. Can we say that these Chinese did not know the shape of the Atlantic, of the Channel, of the river Seine, of the park of Versailles? Yes, we are allowed to say this, they had no idea of them and probably they could not care less. Can we say that Lapérouse knew this part of Sakhalin before landing there? No, it was his first encounter with it, he had to fumble in darkness, taking soundings along the coast. Are we allowed to say that the crew of the Neptuna knew this coast? Yes, we may say this, they could look at Lapérouse’s notes, and compare his drawings of the landings with what they saw themselves; less sounding, less fumbling in the dark. Thus, the knowledge that the Chinese fishermen had and that Lapérouse did not possess had, in some still mysterious way, been provided to the crew of the English ship. So, thanks to this little vignette, we might be able to define the word knowledge.

The first time we encounter some event, we do not know it; we start knowing something when it is at least the second time we encounter it, that is, when it is familiar to us. Someone is said to be knowledgeable when whatever happens is only one instance of other events already mastered, one member of the same family. However, this definition is too general and gives too much of an advantage to the Chinese fishermen. Not only have they seen Sakhalin twice, but hundreds and even thousands of times for the more elderly. So they will always be more knowledgeable than these white, ill-shaven, capricious foreigners who arrive at dawn and leave at dusk. The foreigners will die en route, wrecked by typhoons, betrayed by guides, destroyed by some Spanish or Portuguese ship, killed by yellow fever, or simply eaten up by some greedy canibals... as probably happened to Lapérouse. In other words, the foreigner will always be weaker than any one of the peoples, of the lands, of the climates, of the reefs, he meets around the world, always at their mercy. Those who go away from the lands in which they are born and who cross the paths of other people disappear without trace. In this case, there is not even time for a Great Divide to be drawn; no accusation process takes place, no trial of strength between different
supplement of forces it provides to those who develop them. This supplement gained from manipulating $n$th degree forms comes entirely from inside the centres and is probably better accounted for by the many new transversal connections it allows. Second, we do not have to lose our time finding empirical counterparts to explain these forms by simple, practical manipulations, similar to the ones done outside the centres. The handling of pebbles on Sakhalin beach will never give you set theory or topology. To be sure, the cascade of inscriptions is a practical and concrete manipulation of paper forms all along, but each end-product is a form that does not resemble anything on the level below—it if does, it means this rung in the ladder is useless, that at least that part of the translation has failed. Third, we do not have to waste any time looking for ‘social explanations’ of these forms, if by social is meant features of society mirrored by mathematics in some distorted way. Forms do not distort or misrepresent anything, they accelerate still more the movement of accumulation and capitalisation. As I have hinted all along, the link between society and mathematics is both much more distant and much more direct than expected: they explicitly attach firmly together all possible allies, constituting in effect what is probably the hardest and most ‘social’ part of society. Fourth, there is no reason to fall back on conventions that scientists would agree with one another in order to account for the bizarre existence of these forms that seem unrelated to anything else. They are no less real, no more sterile, no more piable than any other inscriptions devised to make the world mobile and to carry it to the centres. If anything, they resist more than anything else (by our definition of reality) since they multiply and enhance the relations of all the other elements of the networks. Fifth, to find our way, we have to take the grain of truth offered by each of these four traditional interpretations of forms (transcendentalism, empiricism, social determinism and conventionalism): $n$th order forms give an unexpected supplement—as if coming from another world; they are the result of a concrete work of purification—as if related to practical matters; they concentrate the associations still more—as if they were more social than society; they tie together more elements—as if they were more real than any other convention passed among men.

Frankly, I have not found one single study which could fulfill this fifth requirement. From this absence, one could draw the conclusion that forms cannot be studied through any sort of enquiry like the one I have portrayed in this book because they escape for ever what happens in the centres of calculation. But I draw a different conclusion; almost no one has had the courage to do a careful anthropological study of formalism. The reason for this lack of nerve is quite simple: a priori, before the study has even started, it is towards the mind and its cognitive abilities that one looks for an explanation of forms. Any study of mathematics, calculations, theories and forms in general should do quite the contrary: first look at how the observers move in space and time, how the mobility, stability and combinability of inscriptions are enhanced, how the networks are extended, how all the informations are tied together in a cascade of re-representation, and if, by some extraordinary chance, there is something still unaccounted for, then, and only then, look for special cognitive abilities. What I propose, here, as a seventh rule of method, is in effect a moratorium on cognitive explanations of science and technology! I’d be tempted to propose a ten-year moratorium. If those who believe in miracles were so sure of their position, they would accept the challenge.

**Part C**

**Metrologies**

Translating the world towards the centres is one thing (Part A); gaining an unexpected supplement of strength by working inside these centres on $n$th degree inscriptions is another (Part B). There is still one remaining snag, because the final inscriptions are not the world: they are only representing it in its absence. New infinite spaces and times, gigantic black holes, minuscule electrons, enormous economies, mind-boggling billions of years, intricate scale models, complex equations, all occupy no more than a few square metres that a few per cent of the population (see Chapter 4) dominate. To be sure, many clever traps and tricks have been discovered to reverse the balance of forces and make the centres bigger and wiser than the things that dominated them until then. However, nothing is irreversibly gained at this point if there is no way to translate back the relation of strength that has been made favourable to the scientists’ camp. More additional work has yet to be done. This movement from the centre to the periphery is to be studied as well, if we want to follow scientists up to the end. Although this last leg of the journey is as important as the other two, it is usually forgotten by the observers of science because of this queer notion that ‘science and technology’ are ‘universal’; according to this notion, once theories and forms have been discovered, they spread ‘everywhere’ without added cost. This application of abstract theories everywhere and at every time appears to be another miracle. As usual, following scientists and engineers at work gives a more mundane but more interesting answer.

**(1) Extending the networks still further**

When, on 5 May 1961, Alan Shepard got his turn on the first American Mercury space flight, was it the first time? In a way, yes, since no American had really been out there. In another sense, no, it was simply the ($n + 1$)th time. He had done every possible gesture hundreds of times before on the simulator, a scale model of another sort. What was his main impression when he finally got outside the simulator and inside the rocket? It was either 'just the way it sounded in the centrifuge' or 'it was different from the simulator, it was easier' or 'Man, that
wasn’t like the centrifuge, it was more sudden’. During his short flight he kept comparing the similarities and slight differences between the *nth* rehearsal on the flight simulator, and the *(n + 1)th* actual flight. The attendants in the control tower were surprised how cool Shepard was. This guy obviously had the ‘right stuff’ since he was not afraid of going out there in the unknown. But the point is that he was not really going into the unknown, as Magellan did crossing the strait that bears his name. He had been *there* already hundreds of times, and monkeys before him hundreds of other times. What is admirable is not how one can get into space, but how the complete space flight can be simulated in advance, and then slowly extended to unmanned flights, then to monkeys, then to one man, then to many, by incorporating *inside* the Space Centre more and more *outside* features brought back to the centre by each trial. The slow and progressive extension of a network from Cape Canaveral to the orbit of the earth is more of an achievement than the ‘application’ of calculations done inside the Space Centre to the outside world.

‘Still, is not the application of science outside of the laboratories the best proof of its efficacy, of the quasi-supernatural power of scientists? Science works outside and its predictions are fulfilled.’ Like all the other claims we have encountered in this chapter they are based on no independent and detailed study. No one has ever observed a fact, a theory or a machine that could survive outside of the networks that gave birth to them. Still more fragile than termites, facts and machines can travel along extended galleries, but they cannot survive one minute in this famous and mythical ‘out-thereness’ so vaunted by philosophers of science.

When the architects, urbanists and energeticians in charge of the Frangocastello solar village project in Crete had finished their calculations in early 1980 they had in their office, in Athens, a complete paper scale model of the village. They knew everything available about Crete: solar energy, weather patterns, local demography, water resources, economic trends, concrete structures and agriculture in greenhouses. They had rehearsed and discussed every possible configuration with the best engineers in the world and had triggered the enthusiasm of many European, American and Greek development banks by settling on an optimal and original prototype. Like Cape Canaveral engineers they had *simply* to go ‘out there’ and apply their calculations, proving once again the quasi-supernatural power of scientists. When they sent their engineers from Athens to Frangocastello to start expropriating property and smoothing out the little details, they met with a totally unexpected ‘outside’. Not only were the inhabitants not ready to abandon their lands in exchange for houses in the new village, but they were ready to fight with their rifles against what they took as a new American atomic military base camouflaged under a solar energy village. The application of the theory became harder every day as the mobilisation of opposition grew in strength, enrolling the pope and the Socialist Party. It soon became obvious that, since the army could not be sent to force Cretans to occupy willingly the future prototype, a negotiation had to start between the inside and the outside. But how could they strike a compromise between a brand new solar village and a few shepherds who simply wanted three kilometres of asphalted road and a gas station? The compromise was to abandon the solar village altogether. All the planning of the energeticians was routed back *inside* the network and limited to a paper scale model, another one of the many projects engineers have in their drawers. The ‘out-thereness’ had given a fatal blow to this example of science.

So how is it that in some cases science’s predictions are fulfilled and in some other cases pitifully fail? The rule of method to apply here is rather straightforward: every time you hear about a successful application of a science, look for the progressive extension of a network. Every time you hear about a failure of science, look for what part of which network has been punctured. I bet you will always find it.

There was nothing more dramatic at the time than the prediction solemnly made a month in advance by Pasteur that on 2 June 1881 all the non-vaccinated sheep of a farm in the little village of Pouilly-le-Fort would have died of the terrible anthrax disease and that all the vaccinated ones would be in perfect health. Is this not a miracle, as if Pasteur had travelled in time, and in the vast world outside, anticipating a month in advance what will happen in a tiny farm in Beauce? If, instead of gaping at this miracle, we look at how a network is extended, sure enough we find a fascinating negotiation between Pasteur and the farmers’ representatives on how to *transform the farm into a laboratory*. Pasteur and his collaborators had already done this trial several times inside their lab, reversing the balance of forces between man and diseases, creating artificial epizootics in their lab (see Chapter 3). Still, they had never done it in full-scale farm conditions. But they are not fools, they know that in a dirty farm thronged by hundreds of onlookers they will be unable to repeat exactly the situation that had been so favourable to them (and will meet the same sort of failure as the energeticians bringing their village to the Cretans). On the other hand, if they ask people to come to *their* lab no one will be convinced (any more than telling Kennedy that Shepard has flown on the centrifuge one more time will convince the American people that they had taken their revenge over the Russians for being first in space). They have to strike a compromise with the organisers of a field test, to transform enough features of the farm into laboratory-like conditions – so that the same balance of forces can be maintained – but taking enough risk – so that the test is realistic enough to count as a trial done outside. In the end the prediction is fulfilled but it was in effect a *retrodicticon*, exactly like the foresight of Professor Bijker on the future of Rotterdam harbour (see Part A) was in effect *hindsight*. To say this is not to diminish the courage of Shepard in his rocket, of the energeticians mobbed by the farmers, or of Pasteur taking the risk of a terrible mistake, any more than knowing in advance that Hamlet will die at the end of the play diminishes the talent of the actor. No amount of rehearsals frees the talented player from stage fright.

The predictable character of technoscience is entirely dependent on its ability
to spread networks further. As soon as the outside is really encountered, complete chaos ensues. Of all the features of technoscience, I find this ability to extend networks and to travel along inside them the most interesting to follow; it is the most ingenious and the most overlooked of all (because of the inertia model depicted at the end of Chapter 3). Facts and machines are like trains, electricity, packages of computer bytes or frozen vegetables: they can go everywhere as long as the track along which they travel is not interrupted in the slightest. This dependence and fragility is not felt by the observer of science because 'universality' offers them the possibility of applying laws of physics, of biology, or of mathematics everywhere in principle. It is quite different in practice. You could say that it is possible in principle to land a Boeing 747 anywhere; but try in practice to land one on 5th Avenue in New York. You could say that telephone gives you a universal reach in principle. Try to call from San Diego someone in the middle of Kenya who does not, in practice, have a telephone. You can very well claim that Ohm's law (Resistance = Voltage/Current – see page 238) is universally applicable in principle; try in practice to demonstrate it without a voltmeter, a wattmeter and an ammeter. You may very well claim that in principle a navy helicopter can fly anywhere; but try to fix it in the Iranian desert when it is stalled by a sandstorm, hundreds of miles from the aircraft carrier. In all these mental experiments you will feel the vast difference between principle and practice, and how everything works according to plan it means that you do not move an inch out of well-kept and carefully sealed networks. 

Every time a fact is verified and a machine runs, it means that the lab or shop conditions have been extended in some way. A medical doctor's cabinet a century ago would have been furnished with an armchair, a desk and maybe an examination table. Today, your doctor's cabinet is filled with dozens of instruments and diagnostic kits. Each of them (like the thermometer, the blood pressure kit or the pregnancy test) has come from a laboratory to the cabinet through the instrument industry. If your doctor verifies the application of the laws of physiology, very well, but do not ask her to verify them in an empty cabin in the middle of the jungle, or she will say, 'Give me my instruments back first!' Forgetting the extension of the instruments when admiring the smooth running of facts and machines would be like admiring the road system, with all those fast trucks and cars, and overlooking civil engineering, the garages, the mechanics and the spare parts. Facts and machines have no inertia of their own (Chapter 3); like kings or armies they cannot travel without their retinues or impediments.

(2) Tied in by a few metrological chains

The dependency of facts and machines on networks to travel back from the centres to the periphery makes our job much easier. It would have been impossible for us to follow 'universal' laws of science that would have been applicable everywhere without warning. But the progressive extension of the domain of application of a laboratory is very simple to study: just follow the traces this application creates. As we saw in Part B, a calculation on paper can apply to the outside world only if this outside world is itself another piece of paper of the same format. At first, this requirement seems to mark the end of the road for the calculations. It is impossible to transform Sakhalin, Rotterdam, turbulences, people, microbes, electrical grids and all the phenomena out there into a paper world similar to the one in there. This would be without allowing for the ingenuity of the scientists in extending everywhere the instruments that produce this paper world. Metrology is the name of this gigantic enterprise to make of the outside a world inside which facts and machines can survive. Termites build their obscure galleries with a mixture of mud and their own droppings; scientists build their enlightened networks by giving the outside the same paper form as that of their instruments inside. In both cases the result is the same: they can travel very far without ever leaving home.

In the pure, abstract and universal world of science the extension of the new objects created in the labs costs nothing at all. In the real, concrete and local world of technoscience, however, it is frightfully expensive simply to maintain the simplest physical parameters stable. A simple example will be enough. If I ask, 'What time is it?' you will have to look at your watch. There is no way to settle this question without taking a reading at the window of this scientific instrument (the sun will do, but not when you need to catch a train). No matter how humble it is, the clock is of all scientific instruments the one with the longest and most influential history. Remember that Lapérouse carried with him no less than twelve ship chronometers and had several scientists on board simply to check and compare their movements. His whole trip would have been rendered useless if he could not have kept the time constant. Now, if our two watches disagree, we will be led to a third one which will act as our referee (a radio station, a church clock). If there is still a disagreement on the quality of the clock used as referee, we might very well call the 'speaking clock'. If one of us was as obstinate as the dissenter of Chapters 1 and 2, he or she will be led into an extraordinarily complex maze of atomic clocks, lasers, satellite communications: the International Bureau of Time coordinating throughout the earth what time it is. Time is not universal; every day it is made slightly more so by the extension of an international network that ties together, through visible and tangible linkages, each of all the reference clocks of the world and then organises secondary and tertiary chains of references all the way to this rather imprecise watch I have on my wrist. There is a continuous trail of readings, checklists, paper forms, telephone lines, that tie all the clocks together. As soon as you leave this trail, you start to be uncertain about what time it is, and the only way to regain certainty is to get in touch again with the metrological chains. Physicists use the nice word constant to designate these elementary parameters necessary for the simplest equation to be written in the laboratories. These constants, however, are so inconstant that the US, according to the National Bureau of Standards, spends 6 per cent of its Gross National Product, that is, three times what is spent on R & D (see Chapter 4), just to maintain them stable!
That much more effort has to be invested in extending science than in doing it may surprise those who think it is naturally universal. In the figures that I presented in Chapter 4 we could not make sense at first of this mass of scientists and engineers engaged in management of R & D, management, inspection, production, and so on (see page 16). It need no longer surprise us. We know that scientists are too few to account for the enormous effect they are supposed to generate and that their achievements circulate in frail, recent, costly and rare galleries. We know that 'science and technology' is only the abstracted tip of a much larger process, and has only a very vague resemblance to it. The paramount importance of metrology (like that of development and industrial research) gives us a measure, so to speak, of our ignorance.

These long metrological chains necessary for the very existence of the simplest laboratories concern only the official constants (time, weight, length, biological standards, etc.), but this is only a tiny part of all the measurements made. We are so used to the pervasive presence of all these meters, counters, paper forms and tallies which pave the way for centres of calculation that we forget to consider each of them as the sure trace of an earlier invasion by a scientific profession. Just think about the kind of answer you can provide to these questions: How much did I earn this month? Is my blood pressure above or below normal? Where was my grandfather born? Where is the tip of Sakhalin island? How many square metres is my flat? How much weight have you put on? How many good grades did my daughter get? What temperature is it today? Is this pack of beer on sale a good buy? Depending on who asks these questions you may provide either a softer answer or a harder one. In the latter case you will have to fall back on a paper form: the accounting slip sent to you by your bank; the reading taken out of the blood pressure kit in your doctor's office; the birth certificates kept at City Hall or a genealogical tree, the list of flashing lights printed in the Nautical Almanac; a geometrical drawing of your flat; a scale; a school report kept in your daughter's college administration; a thermometer; the dozen of metrological marks made on the pack of beer (content, alcoholic degree, amount of preservatives, etc.). What we call 'thinking with accuracy' in a situation of controversy is always bringing to the surface one of these forms. Without them we simply don't know.

If for one reason or another (crime, accident, controversy), the dispute is not settled at this point, you will be led along one of the many metrological chains that pile up paper forms to the nth order. Even the question 'who are you' cannot be solved, in some extreme situations, without superimposing passports to fingerprints to birth certificates to photographs, that is without constituting a file that brings together many different paper forms of various origins. You might very well know who you are and be satisfied with a very soft answer to this absurd query, but the policeman, who raises the question from the point of view of a centre, wants to have a harder answer than that, exactly as when Lapérouse kept asking the Chinese fishermen where they were in terms of longitude and latitude. We can understand now the misunderstanding studied in Chapter 5, Part C between the softer and the harder ways of solving the paradox of the fact-builder. The requirements put on knowledge are utterly different if one wants to use it to settle a local dispute or to participate in the extension of a network far away. All the intermediaries are enough in the first case (I know who I am, what time it is, if it is warm or cold, if my flat is big or small, if I earn enough, if my daughter works well, if Sakhalin is an island or not). They are all found wanting in the latter case. The misunderstanding is of the same nature and has the same concrete meaning as if an army engineer in charge of preparing the landing of 852 bombers on a Pacific island finds only a muddy landing strip a few hundred yards long. He will indeed be disappointed and will find the airstrip wanting.

The only way to prepare 'landing strips' everywhere for facts and machines is to transform as many points as possible of the outside world into instruments. The walls of the scientific galleries are literally papered over.

Machines, for instance, are drawn, written, argued and calculated, before being built. Going from 'science' to 'technology' is not going from a paper world to a messy, greasy, concrete world. It is going from paperwork to still more paperwork, from one centre of calculation to another which gathers and handles more calculations of still more heterogeneous origins. The more modern and complex they are, the more paper forms machines need so as to come into existence. There is a simple reason for this: in the very process of their construction they disappear from sight because each part hides the others as they become darker and darker black boxes (Chapter 3). The Eagle group, during the debugging, had to build a computer program just to keep track of the modifications each of them was doing to the prototype, just to remember what Eagle was about, to keep it synoptically under their eyes while it became more and more obscure (Introduction). Of all the parts of technoscience, the engineers' drawings and the organisation and management of the traces generated simultaneously by engineers, draughtsmen, physicists, economists, accountants, marketing agents and managers are the most revealing. They are the ones where the distinctions between science, technology, economics and society are the most absurd. The centres of calculations of major machine-building industries concentrate on the same desks paper forms of all origins, recombining them in such a way that some slips of paper bring together the shape of the part to be built (drawn in a codified geometrical space); the tolerance and calibration necessary for its construction (all the metrological chains inside and outside the forms); the physical equations of material resistance; the names of the workers in charge of the parts; the mean time necessary to effect the operations (result of decades of Taylorisation); the dozens of codes that make the keeping of the inventory possible; the economic calculations; and so on. Those who would try to replace the common history of these centres of calculations by clean distinct histories of science, of technology, and of management would have to butcher the subject.

Each of these paper forms is necessary for one of the dozens of sciences involved in machine-building simply to have any relevance at all. Accountancy, for instance, is a crucial and pervasive science in our societies. Its extension,
however, is strictly limited by the few paper forms that make accurate book-keeping possible. How do you apply book-keeping to the confusing world of goods, consumers, industry? Answer: by transforming each of these complex activities, so that, at one point or another, they generate a paper form that is readily applicable to book-keeping. Once each hamburger sold in the United States, each coffee cup, each bus ticket is accompanied by a numbered stub, or one of these little white tallies spews out of every cash register, then indeed accountants, managers and economists are able to expand their skill at calculating. A restaurant, a supermarket, a shop, an assembly line are generating as many readings from as many instruments as a laboratory (think of the scales, the clocks, the registers, the order forms). It is only once the economy is made to generate enough of these paper forms so as to resemble economics that the economists become part of an expanding profession. There is no reason to limit the study of science to the writing of the Book of Nature, and to forget to study this ‘Great Book of Culture’ which has a much more pervasive influence on our daily life than the other – the mere information in banks, for example, is several orders of magnitude more important than scientific communication.

Even geography, that seems so readily applicable ‘outside’, once the map is made, cannot escape very far from the networks without becoming useless. When we use a map, we rarely compare what is written on the map with the landscape – to be capable of such a feat you would need to be yourselves a well-trained topographer, that is, to be closer to the profession of geographer. No, we most often compare the readings on the map with the road signs written in the same language. The outside world is fit for an application of the map only when all its relevant features have themselves been written and marked by beacons, landmarks, boards, arrows, street names and so on. The easiest proof of this is to try to navigate with a very good map along an unmarked coast, or in a country where all the road boards have been torn off (as happened to the Russians invading Czechoslovakia in 1968). The chance is that you will soon be wrecked and lost. When the out-thereness is really encountered, when things out there are seen for the first time, this is the end of science, since the essential cause of scientific superiority has vanished.

The history of technoscience is in a large part the history of all the little inventions made along the networks to accelerate the mobility of traces, or to enhance their faithfulness, combination and cohesion, so as to make action at a distance possible. This will be our sixth principle.

(3) About a few other paper-shufflers

If we extend the meaning of metrology to include not only the upkeep of the basic physical constants but also the transformation of as many features as possible of the outside in paper forms, we might end up studying the most despised of all the aspects of technoscience: the paper-shufflers, the red-tape worms, the bureaucrats. Ah! these bureaucrats, how hated they are – these people who only deal with pieces of paper, files and forms, who know nothing about the real world, but are only superimposing forms on other forms simply to check if they have been correctly filled in; this curious breed of lunatics that prefers to believe a piece of paper to any other source of information, even if it is against common sense, logic and even their own feelings. Sharing this scorn would be, however, a major mistake for us who wish to follow science in action up to the end. First, because what are seen as defects in the case of the paper-shufflers are considered noble qualities when considering these other paper-shufflers who are called scientists and engineers. Believing more the nth order paper form than common sense is a feature of astronomers, economists, bankers, of everyone who treats in the centres phenomena which are, by definition, absent.

It would be a mistake, second, because it is through bureaucracy and inside the files that the results of science travel the furthest. For instance, the loggings produced by Schlumberger engineers on oil platforms (Part A, section 2) become part of a file inside a bank at Wall Street that combines geology, economics, strategy and law. All these unrelated domains are woven together once they become sheets of this most despised of all objects, the record, the dusty record. Without it, though, the loggings would stay where they were, inside the Schlumberger cabin or truck, without any relevance to other issues. The microbiological tests of water made by bacteriologists would have no relevance either if they stayed inside the lab. Now that they are integrated, for instance, in another complex record at City Hall that juxtaposes architects’ drawings, city regulations, poll results, vote tallies and budget proposals, they profit from each of these other skills and crafts. Understanding the bearing of bacteriology on ‘society’ might be a difficult task; but following in how many legal, administrative and financial operations bacteriology has been enrolled is feasible: just follow the trail. As we saw in Chapter 4, the esoteric character of a science is inversely proportional to its exoteric character. What we realise now is that administration, bureaucracy, and management in general are the only big resources available to expand really far: the government supports the bacteriology laboratory which has become an obligatory passage point for every decision to be made. What appeared at the beginning of this book as vast and insulated pockets of science are probably best understood if they are seen to be scattered through centres of calculation, dispersed over files and records, seeded through all the networks and visible only because they accelerate the local mobilisation of some resources among many others that are necessary to administer many people on a large scale and at a distance.23

The third and final reason why we should not despise bureaucrats, managers, paper-shufflers or, in brief, this tertiary sector that completely dwarfs the size of technoscience is that it constitutes a mixture of other disciplines which have to be studied with the same method I have presented in this book even though they are not considered as pertaining to ‘science and technology’. When people claim they want to explain ‘socially’ the development of ‘science and technology’ they use
entities like national policy, multinational firms’ strategies, classes, world economic trends, national cultures, professional status, stratification, political decisions, and so on and so forth. At no point in this book have I used any of these entities; on the contrary, I have explained several times that we should be as agnostic about society as about nature, and that providing a social explanation does not mean anything ‘social’ but only something about the relative solidity of associations. I also promised, however, at the end of Chapter 3, that we will meet at some point a stable state of society. Well, here we are: a stable state of society is produced by the multifarious administrative sciences exactly like a stable interpretation of black holes is provided by astronomy, of microbes by bacteriology, or of proven oil reserves by geology. No more, no less. Let us end with a few more examples.

The state of the economy, for instance, cannot be used unproblematically to explain science, because it is itself a very controversial outcome of another soft science: economics. As we saw earlier, it is extracted out of hundreds of statistical institutions, questionnaires, polls and surveys, and treated in centres of calculation. Something like the Gross National Product is an nth order visual display which, to be sure, may be combined to other paper forms, but which is no more outside the frail and tiny networks built by economists than stars, electrons or plate tectonics. The same is true for many aspects of politics. How do we know that Party A is stronger than Party B? Each of us may have an opinion about the relative strength of these parties; indeed, it is because each of us has one opinion about it that we may have to build a huge scientific experiment to settle the question. Scientific? Sure. What is a national election, if not the transformation through a very costly and cumbersome instrument of all the opinions into marks on ballot papers, marks which are then counted, summed, compared (with great care and with much controversy) to eventually end up in one nth order visual display: Party A: 51%, Party B: 45%, Null: 4%? To distinguish between or oppose science, politics and economics would be meaningless from our point of view, because in terms of size, relevance and cost, the few figures that decide the Gross National Product or the political balance of forces are much more important, trigger much more interest, much more scrutiny, much more passion, much more scientific method than a new particle or a new radio source. All of them depend on the same basic mechanism: calibrating inscription devices, focusing the controversies on the final visual display, obtaining the resources necessary for the upkeep of the instruments, building nth order theories on the archived records. No, our method would gain nothing in explaining ‘natural’ sciences by invoking ‘social’ sciences. There is not the slightest difference between the two, and they are both to be studied in the same way. Neither of them should be believed more nor endowed with the mysterious power of jumping out of the networks it builds.

What is clear for economics, politics and management is all the clearer for sociology itself. How could someone who decided to follow scientists in action forget to study sociologists striving to define what society is all about, what keeps us all glued together, how many classes there are, what is the aim of living in society, what are the major trends of its evolution? How could one believe these people who say what society is about more than the others? How could one transform astronomers into spokespersons for the sky and still accept that the sociologists tell us what society is. The very definition of a ‘society’ is the final outcome, in Sociology Departments, in Statistical Institutions, in journals, of other scientists busy at work gathering surveys, questionnaires, archives, records of all sorts, arguing together, publishing papers, organising other meetings. Any agreed definition marks the happy end of controversies like all the settlements we have studied in this book. No more, no less. The results on what society is made of do not spread more or faster than those of economics, topology or particle physics. These results too would die if they went outside of the tiny networks so necessary for their survival. A sociologist’s interpretation of society will not be substituted for what every one of us thinks of society without additional struggle, without textbooks, chairs in universities, positions in the government, integration in the military, and so on, exactly as for geology, meteorology or statistics.

No, we should not overlook the administrative networks that produce, inside rooms in Wall Street, in the Pentagon, in university departments, fleeting or stable representations of what is the state of the forces, the nature of our society, the military balance, the health of the economy, the time for a Russian ballistic missile to hit the Nevada desert. To rely on social sciences more than on natural ones would put our whole journey in jeopardy, because we would have to accept that the space-time elaborated inside a network by one science has spread outside and included all the others. We are no more included in the space of society (built by sociologists through so many disputes), than in the time of geology (slowly elaborated in Natural History Museums), or in the domain of neurosciences (carefully extended by neuroscientists). More exactly, this inclusion is not naturally provided without additional work; it is obtained locally if the networks of sociologists, geologists and neuroscientists are extended, if we have to pass through their laboratories, or through their metrological chains, if they have been able to render themselves indispensable to our own trips and travels. The situation is exactly the same for the sciences as for gas, electricity, cable TV, water supplies or telephones. In all cases you need to be hooked up to costly networks that have to be maintained and extended. This book has been written to provide a breathing space to those who want to study independently the extensions of all these networks. To do such a study it is absolutely necessary never to grant to any fact, to any machine, the magical ability of leaving the narrow networks in which they are produced and along which they circulate. This tiny breathing space would become immediately vitiated if the same fair and symmetric treatment was not applied to the social and administrative sciences as well.
APPENDIX 1

Rules of Method

Rule 1 We study science in action and not ready made science or technology; to do so, we either arrive before the facts and machines are blackboxed or we follow the controversies that reopen them. (Introduction)

Rule 2 To determine the objectivity or subjectivity of a claim, the efficiency or perfection of a mechanism, we do not look for their intrinsic qualities but at all the transformations they undergo later in the hands of others. (Chapter 1)

Rule 3 Since the settlement of a controversy is the cause of Nature's representation, not its consequence, we can never use this consequence, Nature, to explain how and why a controversy has been settled. (Chapter 2)

Rule 4 Since the settlement of a controversy is the cause of Society's stability, we cannot use Society to explain how and why a controversy has been settled. We should consider symmetically the efforts to enrol human and non-human resources. (Chapter 3)

Rule 5 We have to be as undecided as the various actors we follow as to what technoscience is made of; every time an inside/outside divide is built, we should study the two sides simultaneously and make the list, no matter how long and heterogeneous, of those who do the work. (Chapter 4)

Rule 6 Confronted with the accusation of irrationality, we look neither at what rule of logic has been broken, nor at what structure of society could explain the distortion, but to the angle and direction of the observer's displacement, and to the length of the network thus being built. (Chapter 5)

Rule 7 Before attributing any special quality to the mind or to the method of people, let us examine first the many ways through which inscriptions are gathered, combined, tied together and sent back. Only if there is something unexplained once the networks have been studied shall we start to speak of cognitive factors. (Chapter 6)

APPENDIX 2

Principles

First principle The fate of facts and machines is in later users' hands; their qualities are thus a consequence, not a cause, of a collective action. (Chapter 1)

Second principle Scientists and engineers speak in the name of new allies that they have shaped and enrolled; representatives among other representatives, they add these unexpected resources to tip the balance of force in their favour. (Chapter 2)

Third principle We are never confronted with science, technology and society, but with a gamut of weaker and stronger associations; thus understanding what facts and machines are is the same task as understanding who the people are. (Chapter 3)

Fourth principle The more science and technology have an esoteric content the further they extend outside; thus, 'science and technology' is only a subset of technoscience. (Chapter 4)

Fifth principle Irrationality is always an accusation made by someone building a network over someone else who stands in the way; thus, there is no Great Divide between minds, but only shorter and longer networks; harder facts are not the rule but the exception, since they are needed only in a very few cases to displace others on a large scale out of their usual ways. (Chapter 5)

Sixth principle History of technoscience is in a large part the history of the resources scattered along networks to accelerate the mobility, faithfulness, combination and cohesion of traces that make action at a distance possible. (Chapter 6)