Science in action : how to follow scientists and engineers through society/ □
Bruno Latour; Cambridge: Harvard University Press, 1987 (1-62 p.)

INTRODUCTION

Opening Pandora's Black Box

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[1], 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 Lawrence 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[2] 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 proudly 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 chips 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, what 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 up 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 plaguing 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?

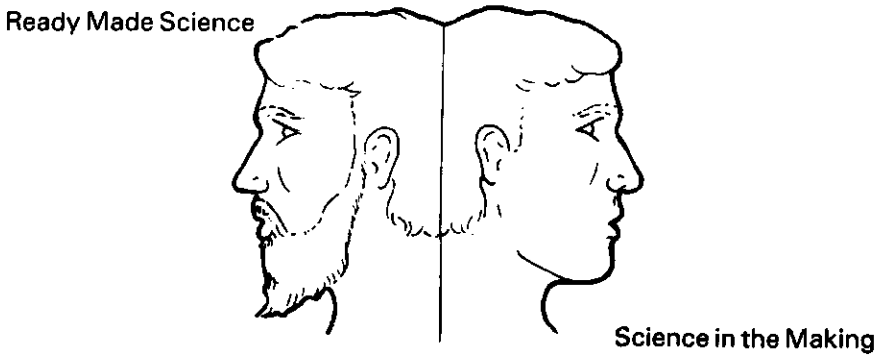


Figure I.1

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 controverial 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 will 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 his blunder. If a student had made a similar mistake, he would be thought unfit 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 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:

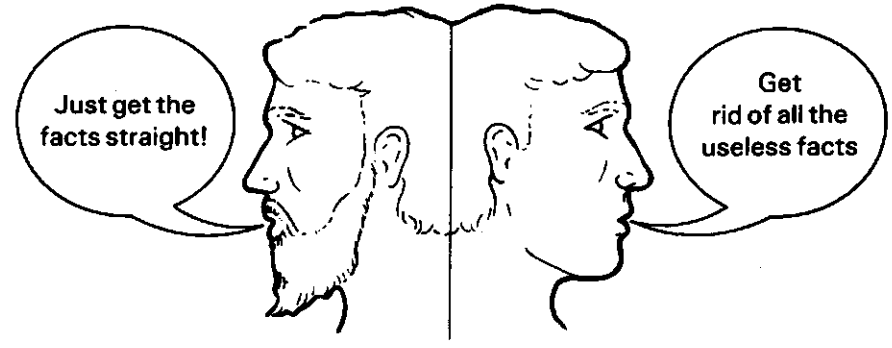


Figure I.2

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 blowing 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 pumping 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:

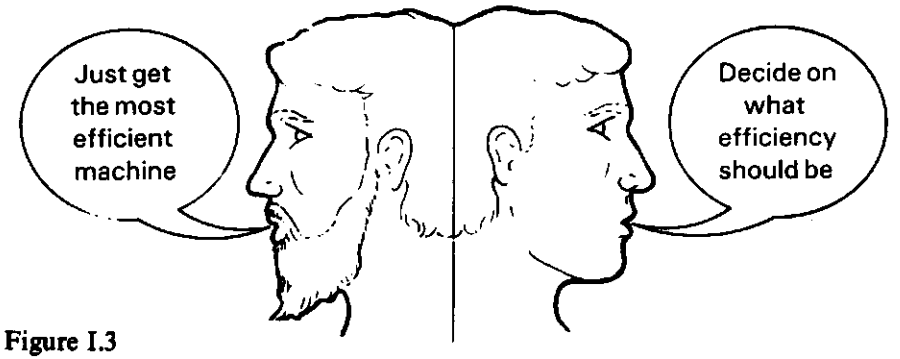


Figure I.3

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

> 'I want to build a simulator, Tom.'
>
> 'It'll take too long, Alsing. The machine'll 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 betchya 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.

Janus' third dictum:

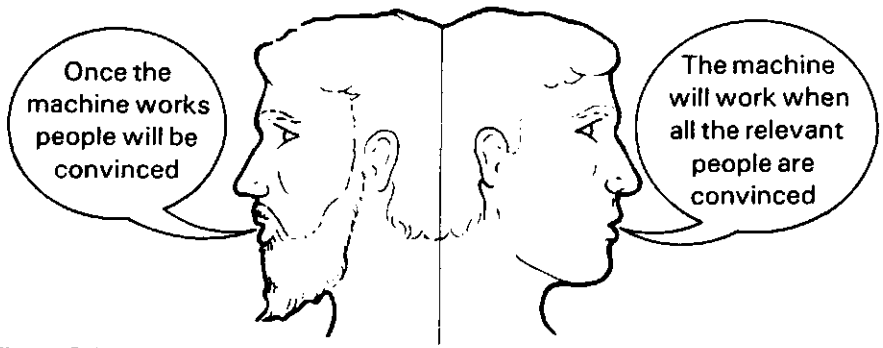


Figure I.4

Scene 8: West has insulated his team for two years from the rest of the company. 'Some of the kids,' he says, 'don't have a notion that there's a company behind all of this. It could be the CIA funding this. It could be a psychological test' (Kidder: 1982, p. 200). During this time, however, West has constantly lobbied the company on behalf of *Eagle*. Acting as a middle-man he has filtered the constraints imposed on the future machine by de Castro (the Big Boss), the marketing department, the other research group in North Carolina, the other machines presented in computer fairs, and so on. He was also the one who kept negotiating the deadlines that were never met. But there comes a point when all the other departments he has lobbied so intensely want to see something, and call his bluff. The situation becomes especially tricky when it is clear at last that the North Carolina group will not deliver a machine, that DEC is selling *VAX* like hot cakes and that all the customers want a supermini 32-bit fully compatible machine from Data General. At this point West has to break the protective shell he has built around his team. To be sure, he designed the machine so as to fit it in with the other departments' interests, but he is still uncertain of their reaction and of that of his team suddenly bereft of the machine.

> 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)

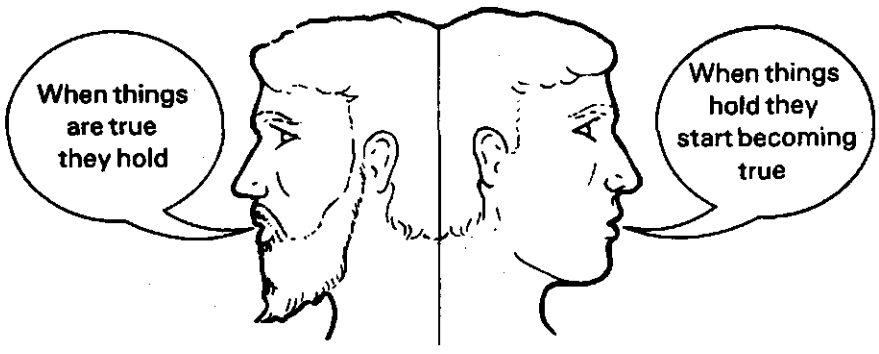
Not only the Software people have to be kept happy, but also the manufacturing people, those from marketing, those who write the technical documentation, the designers who have to place the whole machine in a nice looking box (not a black one this time!), not mentioning the stockholders and the customers.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

Janus's fourth dictum:



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 burn 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/colleagues 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'[3] model that did everything the double helix does–plus 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 is 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.**[4] 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

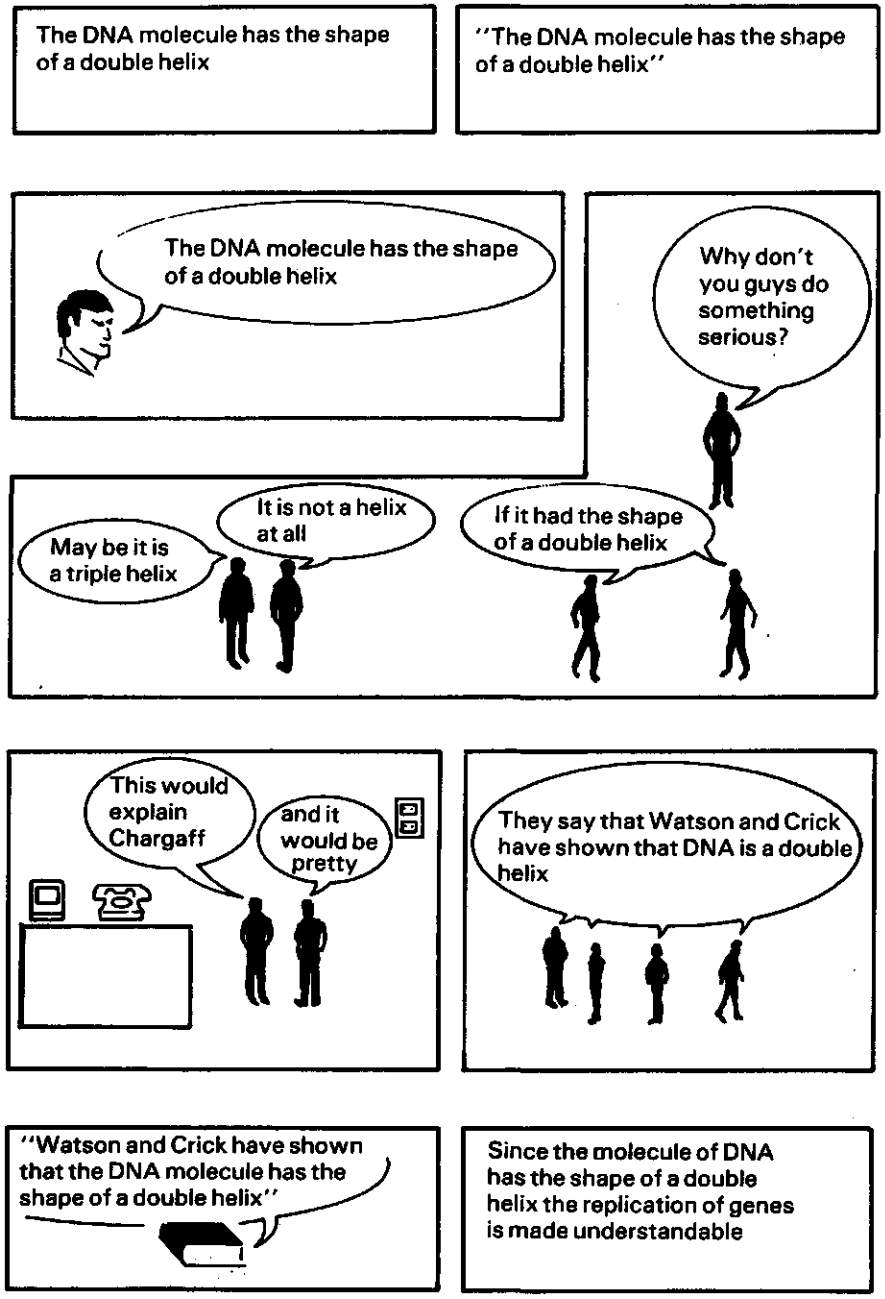


Figure I.6

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 solidity, its foundation, its development or its dangers; unfortunately, almost none of them are 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 *Fatum* 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

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. I 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* experiments 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 math 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 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 method' 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.

Part I

From Weaker to Stronger Rhetoric

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 Minutemen silos are accurate to 100 metres.[1]
>
> (2) Since [new Soviet missiles are accurate within 100 metres] this means that Minutemen 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 sifting 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 Hormone[2] (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[3] 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 is 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 is the accuracy of Soviet missiles; the cure for dwarfism is not yet as well settled in sentence (6) as is 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 of fiction. They break the ground for our analysis. We, laymen, outsiders 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 so 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 Guillemin . . . 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 Guillemin'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 Guillemin'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 Guillemin'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, Guillemin 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 Guillemin 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 Guillemin'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 Guillemin'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

attribution 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 architypical 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 Motors 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 make 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.

Part B
When controversies flare up the literature becomes technical

When we approach the places where facts and machines are made, we get into the midst of controversies. The closer we are, the more controversial they become. When we go from 'daily life' to scientific activity, from the man in the street to the men in the laboratory, from politics to expert opinion, we do not go from noise to quiet, from passion to reason, from heat to cold. We go from controversies to fiercer controversies. It is like reading a law book and then going to court to watch a jury wavering under the impact of contradictory evidence. Still better, it is like moving from a law book to Parliament when the law is still a bill. More noise, indeed, not less.

In the previous section I stopped the controversies before they could proliferate. In real life you cannot stop them or let them go as you wish. You have to decide whether to build the MX or not; you have to know if GHRH is worth investing in; you have to make up your mind as to the future of fuel cells. There are many ways to win over a jury, to end a controversy, to cross-examine a witness or a brain extract. **Rhetoric** is the name of the discipline that has, for millennia, studied how people are made to believe and behave and taught people how to persuade others. Rhetoric is a fascinating albeit despised discipline, but it becomes still more important when debates are so exacerbated that they become scientific and technical. Although this statement is slightly counter-intuitive, it follows from what I said above. You noticed in the three examples that the more I let the controversies go on, the more we were led into what are called 'technicalities'. This is understandable since people in disagreement open more and more black boxes and are led further and further upstream, so to speak, into the conditions that produced the statements. There is always a point in a discussion when the local resources of those involved are not enough to open or close a black box. It is necessary to fetch further resources coming from other places and times. People start using texts, files, documents, articles to force others to transform what was at first an opinion into a fact. If the discussion continues then the contenders in an *oral* dispute become the *readers* of technical texts or reports. The more they dissent, the more the literature that is read will become scientific and technical. For instance, if, after reading sentence (12), which puts the accusations against the CIA into doubt, the MX is still disputed, the dissenter will now be confronted with boxes of reports, hearings, transcripts and studies. The same thing happens if you are obstinate enough not to believe in Schally's discovery. Thousands of neuroendocrinology articles are now waiting for you. Either you give up or you read them. As for fuel cells, they have their own research library whose index lists over 30,000 items, not counting the patents. This is what you have to go through in order to disagree. Scientific or technical texts – I will use the terms interchangeably – are not written differently by different breeds of writers. When you reach them, this does not mean that you quit

rhetoric for the quieter realm of pure reason. It means that rhetoric has become heated enough or is still so active that many more resources have to be brought in to keep the debates going. Let me explain this by considering the anatomy of the most important and the least studied of all rhetorical vehicles: the scientific article.

(1) Bringing friends in

When an oral dispute becomes too heated, hard-pressed dissenters will very quickly allude to what others wrote or said. Let us hear one such conversation as an example:

> (16) Mr Anybody (as if resuming an old dispute): 'Since there is a new cure for dwarfism, how can you say this?'
> Mr Somebody: 'A new cure? How do you know? You just made it up.'
> –I read it in a magazine.
> –Come on! I suppose it was in a colour supplement . . .
> –No, it was in *The Times* and the man who wrote it was not a journalist but someone with a doctorate.
> –What does that mean? He was probably some unemployed physicist who does not know the difference between RNA and DNA.
> –But he was referring to a paper published in *Nature* by the Nobel Prize winner Andrew Schally and six of his colleagues, a big study, financed by all sorts of big institutions, the National Institute of Health, the National Science Foundation, which told what the sequence of a hormone was that releases growth hormone. Doesn't that mean something?
> –Oh! You should have said so first . . . that's quite different. Yes, I guess it does.

Mr Anybody's opinion can be easily brushed aside. This is why he enlists the support of a written article published in a newspaper. That does not cut much ice with Mr Somebody. The newspaper is too general and the author, even if he calls himself 'doctor', must be some unemployed scientist to end up writing in *The Times*. The situation is suddenly reversed when Mr Anybody supports his claim with a new set of allies: a journal, *Nature*; a Nobel Prize author; six co-authors; the granting agencies. As the reader can easily image, Mr Somebody's tone of voice has been transformed. Mr Anybody is to be taken seriously since he is not alone any more: a group, so to speak, accompanies him. Mr Anybody has become Mr Manybodies!

This appeal to higher and more numerous allies is often called the **argument from authority**. It is derided by philosophers and by scientists alike because it creates a majority to impress the dissenter even though the dissenter 'might be right'. Science is seen as the opposite of the argument from authority. A few win over the many because truth is on their side. The classical form of this derision is provided by Galileo when he offers a contrast between rhetoric and real science. After having mocked the florid rhetoric of the past, Galileo opposed it to what happens in physics[4]:

> But in the physical sciences when conclusions are sure and necessary and have nothing to do with human preference, one must take care not to place oneself in the defence of error; for here, a thousand Demosthenes and a thousand Aristotles would be left in the lurch by any average man who happened to hit on the truth for himself.

This argument appears so obvious at first that it seems there is nothing to add. However, a careful look at the sentence reveals two completely different arguments mixed together. Here again the two faces of Janus we have encountered in the introduction should not be confused even when they speak at once. One mouth says: 'science is truth that authority shall not overcome'; the other asks: 'how can you be stronger than one thousand politicians and one thousand philosophers?' On the left side rhetoric is opposed to science just as authority is opposed to reason; but on the right, science is a rhetoric powerful enough, if we make the count, to allow one man to win over 2000 prestigious authorities!

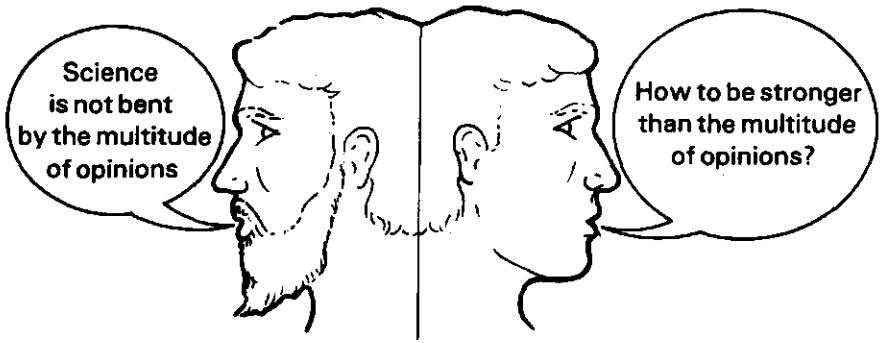


Figure 1.1

'Authority', 'prestige', 'status' are too vague to account for why Schally's article in *Nature* is stronger than Dr Nobody's piece in *The Times*. In practice, what makes Mr Somebody change his mind is exactly the opposite of Galileo's argument. To doubt that there is a cure for dwarfism, he at first has to resist his friend's opinion plus a fake doctor's opinion plus a newspaper. It is easy. But at the end, how many people does he have to oppose? Let us count: Schally and his coworkers plus the board of the New Orleans university who gave Schally a professorship plus the Nobel Committee who rewarded his work with the highest prize plus the many people who secretly advised the Committee plus the editorial board of *Nature* and the referees who chose this article plus the scientific boards of the National Science Foundation and of the National Institutes of Health who awarded grants for the research plus the many technicians and helping hands thanked in the acknowledgements. That's a lot of people and all this is *before* reading the article, just by counting how many people are engaged in its

publication. For Mr Somebody, doubting Mr Anybody's opinion takes no more than a shrug of the shoulders. But how can you shrug off dozens of people whose honesty, good judgment and hard work you must weaken before disputing the claim?

The adjective 'scientific' is not attributed to *isolated* texts that are able to oppose the opinion of the multitude by virtue of some mysterious faculty. A document becomes scientific when its claims stop being isolated and when the number of people engaged in publishing it are many and explicitly indicated in the text. When reading it, it is on the contrary the reader who becomes *isolated.* The careful marking of the allies' presence is the first sign that the controversy is now heated enough to generate technical documents.

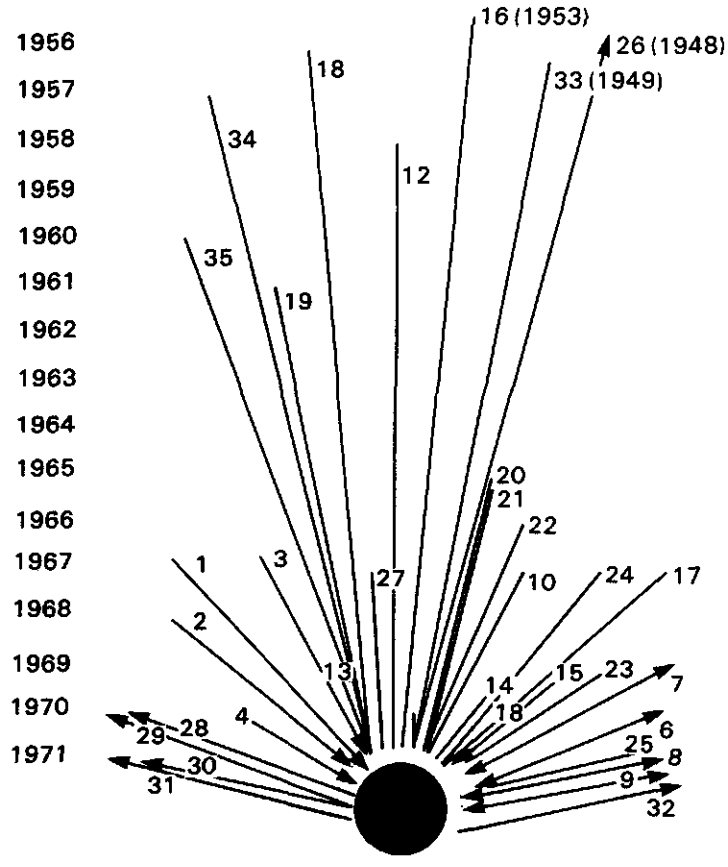
(2) Referring to former texts

There is a point in oral discussions when invoking other texts is not enough to make the opponent change his or her mind. The text itself should be brought in and read. The number of external friends the text comes with is a good indication of its strength, but there is a surer sign: references to other documents. The presence or the absence of references, quotations and footnotes is so much a sign that a document is serious or not that you can transform a fact into fiction or a fiction into fact just by adding or subtracting references.

The effect of references on persuasion is not limited to that of 'prestige' or 'bluff'. Again, it is a question of *numbers*. A paper that does not have references is like a child without an escort walking at night in a big city it does not know: isolated, lost, anything may happen to it. On the contrary, attacking a paper heavy with footnotes means that the dissenter has to weaken each of the other papers, or will at least be threatened with having to do so, whereas attacking a naked paper means that the reader and the author are of the same weight: face to face. The difference at this point between technical and non-technical literature is not that one is about fact and the other about fiction, but that the latter gathers only a few resources at hand, and the former a lot of resources, even from far away in time and space. Figure 1.2 drew the references reinforcing another paper by Schally.[5]

Whatever the text says we can see that it is already linked to the contents of no less than thirty-five papers, from sixteen journals and books from 1948 to 1971. If you wish to do anything to this text and if there is no other way of getting rid of the argument you know in advance that you might have to engage with all these papers and go back in time as many years as necessary.

However, stacking masses of reference is not enough to become strong if you are confronted with a bold opponent. On the contrary, it might be a source of weakness. If you explicitly point out the papers you attach yourself to, it is then possible for the reader – if there still are any readers – to trace each reference and to probe its degree of attachment to your claim. And if the reader is courageous enough, the result may be disastrous for the author. First, many references may



Schally's article

—those going to the text are constituting the imported paradigm;
—those going from the text are discussing the referred papers (only one, 32, is critical)
—those going both ways refer to previous work by the same group on the same question

Figure 1.2

be misquoted or wrong; second, many of the articles alluded to might have no bearing whatsoever on the claim and might be there just for display; third, other citations might be present but only because they are always present in the author's articles, whatever his claim, to mark affiliation and show with which group of scientists he identifies – these citations are called **perfunctory**.[6] All these little defects are much less threatening for the author's claim than the references to papers which explicitly say the contrary of the author's thesis. For instance, Figure 1.2 shows Schally referring to the following paper (reference number 32):

(17) 32. Veber, D.F., Bennett, C., Milkowski, J.D., Gal, G., Denkewalter, R.D., and Hirschman, R., in *Biochemistry and Biophysics Communication*, 45, 235 (1971).

This is a quite an impressive set of allies, *if* they support the claim. But the author should not let the unflinching reader go to reference 32 by himself. Why not? Because in this paper Veber *et al.* link the structure of Schally's GHRH with that of the beta-chain of haemoglobin, levelling exactly the criticisms that we have already seen in sentence (7). A dangerous link indeed in an opponent's hands. To ward it off, Schally cites it but qualifies the paper within his own text:

(18) [Note added in proof.] D.F. Veber et al. have pointed out the similarity between the structure of our decapeptide and the amino-terminal of the Beta-chain of porcine haemoglobin (ref. 32). The significance of this observation remains to be established.

The article is not only referred to; it is also qualified or, as we said earlier, modalised. In this case, the reader is warned not to take Veber's article as a fact; since its significance is not established, it cannot be used against Schally to destroy his GHRH (remember that if Veber's claims were turned into a fact, then Schally's own article would become just a fiction). What Schally does to sentence (17) is done by all articles to all their references. Instead of passively linking their fate to other papers, the article *actively* modifies the status of these papers. Depending on their interests, they turn them more into facts or more into fictions, thus replacing crowds of uncertain allies by well-arrayed sets of obedient supporters. What is called the **context of citation** shows us how one text acts on others to make them more in keeping with its claims.

In sentence (18) Schally added the other article referred to in excerpt (17) to *maintain it in a stage intermediate between fact and fiction. But he also needs* well-established facts so as to start his article with a black box which no one would dare to open. This solid foundation is offered, not surprisingly, at the beginning of the article:

(19) The hypothalamus controls the secretion of growth hormone from the anterior pituitary gland (ref. 1 to Pend Muller, E.E., *Neuroendocrinology*, 1, 537, 1967). This control is mediated by a hypothalamic substance designated growth hormone releasing hormone (ref. 2 to Schally, A.V., Arimura, A., Bowers, C.Y., Kastin, A.J., Sawano, S., and Redding, T.W., *Recent Progress in Hormone Research*, 24, 497, 1968).

The first reference is borrowed as it stands with no indication of doubt or *uncertainty. Besides, it is a five-year-old citation – a very long time for these* short-lived creatures. If you, the reader, doubt this control of the hypothalamus, then forget it, you are out of the game entirely. Inside neuroendocrinology, this is the most solid point, or, as it is often called, the **paradigm**.[7] The second reference is also borrowed as a matter of fact, although it is slightly weaker than the former. Dissent was impossible to reference 1, at least coming from a neuro-endocrinologist; with reference 2 it is possible for a colleague to nitpick: maybe the control is mediated by something other than a hormone; maybe, even if it is a hormone, it blocks growth hormone instead of triggering it; or, at the very least, the name Schally gave to this substance could be criticised (Guillemin, for

instance, calls it GRF). No matter what controversy could start here, Schally needs this reference in his article as a fact, since without it the whole paper would be purposeless: why look for a substance if the possibility of its existence is denied? Let us not forget that, according to our first principle, by borrowing references 1 and 2 as matters of fact he makes them more certain, strengthening their case as well as his own.

There are many other papers this article needs to borrow without question, especially the ones describing methods used in determining the sequence of peptides in general. This is visible in another excerpt from the same article:

> (20) The porcine peptide used in this work was an essentially homogeneous sample isolated as described previously (refs. 5, 9). (. . .) In some cases products of carboxypeptidase B. were analysed with the lithium buffer system of Benson, Gordon and Patterson (ref. 10). (. . .) The Edman degradation was performed as reported by Gottlieb et al. (ref. 14). The method of Gray and Smith (ref. 15) was also used.

None of these references, contrary to the others, are qualified either positively or negatively. They are simply there as so many signposts indicating to the readers, if need be, the technical resources that are under Schally's command. The reader who would doubt the hormone sequence is directed towards another set of people: Benson, Edman, Gottlieb, and even Gray and Smith. The work of these people is not present in the text, but it is indicated that they could be mobilised at once if need be. They are, so to speak, in reserve, ready to bring with them the many technical supports Schally needs to make his point firm.

Although it is convenient for a text to borrow references that could help in strengthening a case, it is also necessary for a text to attack those references that could explicitly oppose its claims. In sentence (18) we saw how the referred paper was maintained in a state between fact and fiction, but it would have been better to destroy it entirely so as to clear the way for the new paper. Such a destruction happens in many ways directly or obliquely depending on the field and the authors. Here is an instructive negative modality made by Guillemin about a set of papers, including the one written by Schally that we just studied:

> (21) The now well established concept of a neurohumoral control of adenohypohyseal secretions by the hypothalamus indicates the existence of a hypothalamic growth-hormone-releasing factor (GRF) (ref. 1) having somatostatin as its inhibitory counterpart (ref. 2). So far hypothalamic GRF has not been unequivocally characterized, despite earlier claims to the contrary (ref. 3).

This citation comes from a recent paper by Guillemin, presenting a new structure for the same GHRH, which he calls GRF. Reference 3 is to Schally's paper. The beginning of excerpt (21) is the same as that of (19) in Schally's text: the hypothalamic control is the blackest of all black boxes. Even if they are in dispute with one another Schally and Guillemin accept that no one can contest this control and call him or herself a neuroendocrinologist. But Schally's article in Guillemin's hands is not a black box at all. If Schally's sequence had been a

fact, then the 1982 article by Guillemin would be meaningless. It would also be meaningless if Schally's sequence had any relation with Guillemin's. The latter would just add to the former's work. With sentence (21) Guillemin's paper just pushes aside Schally's sequence. It was not an unequivocal fact, but a very equivocal 'claim'. It does not count; it was a blind alley. Real work starts from this 1982 paper, and real GRF (wrongly called by Schally GHRH) starts from this sequence.

Articles may go still further in transforming the former literature to their advantage. They might combine positive and negative modalities, strengthening for instance a paper X in order to weaken a paper Y that would otherwise oppose their claim. Here is an instance of such a tactic:

> (22) A structure has been proposed for GRF [reference to Schally's article]; it has been recently shown, however [reference to Veber *et al.*] that it was not GHRH but a minor contaminant, probably a piece of hemoglobin.

Veber's article, that Schally himself cited in excerpt (18), did not say exactly what it is *made to say* here; as for Schally's article it did not exactly claim to have found the GHRH structure. This does not matter for the author of sentence (22); he simply needs Veber as an established fact to make Schally's paper more of an empty claim which, after a rebound, gives more solidity to sentence (21) that proposes a new real substance 'despite earlier claims to the contrary'.

Another frequent tactic is to oppose two papers so that they disable one another. Two dangerous counter-claims are turned into impotent ones. Schally, in the paper under study, uses one test in order to assay his GHRH. Other writers who tried to replicate his claim had used another type of test, called the radioimmunoassay, and failed to replicate Schally's claim. That is a major problem for Schally, and in order to find a way out he retorts that:

> (23) This synthetic decapeptide material or the natural material were (sic) only weakly active in tests where the release of growth hormone was measured by a radioimmunoassay for rat growth hormone (two refs.). However, the adequacy of radioimmunoassays for measuring rat growth hormone in plasma has been questioned recently (ref. 8).

Could the absence of any effect of GHRH in the assay not shake Schally's claim? No, because another paper is used to cast doubt on the assay itself: the absence of GHRH proves nothing at all. Schally is relieved.

It would be possible to go much further in the Byzantine political schemes of the context of citations. Like a good billiard player, a clever author may calculate shots with three, four or five rebounds. Whatever the tactics, the general strategy is easy to grasp: do whatever you need to the former literature to render it as helpful as possible for the claims you are going to make. The rules are simple enough: weaken your enemies, paralyse those you cannot weaken (as was done in sentence (18)), help your allies if they are attacked, ensure safe communications with those who supply you with indisputable instruments (as in (20)), oblige your enemies to fight one another (23); if you are not sure of winning, be humble and

understated. These are simple rules indeed: the rules of the oldest politics. The result of this adaptation of the literature to the needs of the text is striking for the readers. They are not only impressed by the sheer quantity of references; in addition, all of these references are aimed at specific goals and arrayed for one purpose: lending support to the claim. Readers could have resisted a crowd of disorderly citations; it is much harder to resist a paper which has carefully modified the status of all the other articles it puts to use. This activity of the scientific paper is visible in Figure 1.3 in which the paper under study is a point related by arrows to the other papers, each type of arrow symbolising a type of action in the literature.

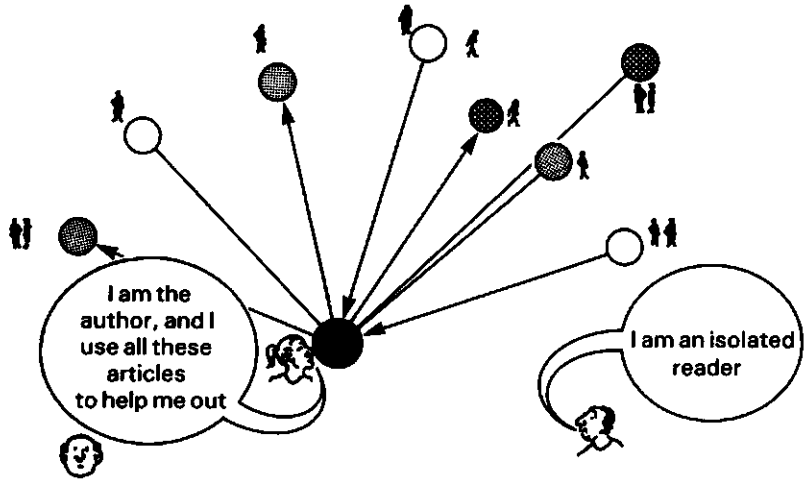


Figure 1.3

(3) Being referred to by later texts

The goal of convincing the reader is not automatically achieved, even if the writer has a high status, the references are well arrayed, and the contrary evidences are cleverly disqualified. All this work is not enough for one good reason: whatever a paper does to the former literature, the later literature will do to it. We saw earlier that a statement was fact or fiction not by itself but only by what the other sentences made of it later on. To survive or to be turned into fact, a statement needs the *next generation* of papers (I will call 'generation' the span of time necessary for another round of papers to be published that refers to the first ones, that is between two and five years). Metaphorically speaking, statements, according to the first principle, are much like genes that cannot survive if they do not manage to pass themselves on to later bodies. In the former section we saw how Schally's paper inserted other articles, distributing honour and shame,

disabling some, strengthening others, borrowing without qualification from still more papers, and so on. All of the cited papers survive in Schally's paper and are modified by its action. But no paper is strong enough to stop controversies. By definition, a fact cannot be so well established that no support is necessary any more. That would be like saying that a gene is so well adapted that it does not need new bodies to survive! Schally may adapt the literature to his end; but each of his assertions, *in turn*, needs other articles later on to make it more of a fact. Schally cannot avoid this any more than the papers he quoted could survive without his taking them up.

Remember how in claim (18) Schally needed the harsh criticisms formulated in Veber's article cited in (17) to remain uncertain so as to protect his claim against a fatal blow. But to maintain (17) in such a state, Schally needs others to confirm his action. Although Schally is able to control most of what he writes in his papers, he has only weak control over what others do. Are they going to follow him?

One way to answer this is to examine the references in *other articles* subsequent to Schally's paper and to look at *their* context of citation. What did they do with what Schally did? It is possible to answer this question through a bibliometric instrument called the *Science Citation Index*.[8] For instance, statement (17) is not maintained by later articles in between fact and fiction. On the contrary, every later writer who cites it takes it as a well-established fact, and they all say that haemoglobin and GHRH have the same structure, using this fact to undermine Schally's claim to have 'discovered' GHRH (this is now placed in quotation marks). If, in the first generation, Schally was stronger than Veber – see (18) – and since there was no ally later on to maintain this strength, in the next generation it is Veber who is strong and Schally who made a blunder by taking a trivial contaminant for a long-sought-after hormone. This reversal is imposed by the other papers and the way *they in turn transform the earlier literature to suit their needs.* If we add to Figure 1.3 a third generation we obtain something like what is shown in Figure 1.4.

By adding the later papers we may map out how the actions of one paper are supported or not by other articles. The result is a cascade of transformations, each of them expecting to be confirmed later by others.

We now understand what it means when a controversy grows. If we wished to continue to study the dispute we will not have simply to read one paper alone and possibly the articles to which it refers; we will also be bound to read all the others that convert each of the operations made by the first paper towards the state of fact or that of fiction. The controversy swells. More and more papers are involved in the mêlée, each of them positioning all the others (fact, fiction, technical details), but no one being able to fix these positions *without the help of the others.* So more and more papers, enrolling more and more papers, are needed at each stage of the discussion – and the disorder increases in proportion.

There is something worse, however, than being criticised by other articles; it is being misquoted. If the context of citations is as I have described, then this misfortune must happen quite often! Since each article adapts the former

literature to suit its needs, all deformations are fair. A given paper may be cited by others for completely different reasons in a manner far from its own interests. It may be cited without being read, that is perfunctorily; or to support a claim which is exactly the opposite of what its author intended; or for technical details so minute that they escaped their author's attention; or because of intentions attributed to the authors but not explicitly stated in the text; or for many other reasons. We cannot say that these deformations are unfair and that each paper should be read honestly as it is; these deformations are simply a consequence of what I called the activity of the papers on the literature; they all manage to do the same carving out of the literature to put their claims into as favourable as possible a state. If any of these operations is taken up and accepted by the others as a fact, then that's it; it is a fact and not a deformation, however much the author may protest. (Any reader who has ever written a quotable article in any discipline will understand what I mean.)

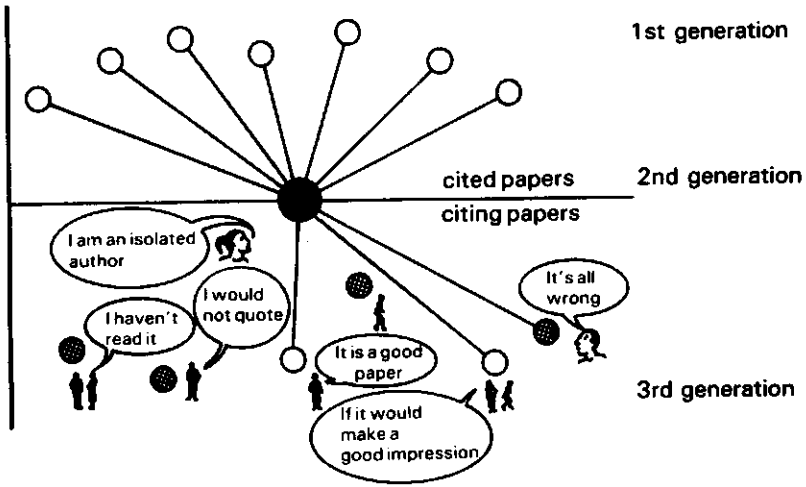


Figure 1.4

There is something still worse, however, than being either criticised or dismantled by careless readers: it is being *ignored*. Since the status of a claim depends on later users' insertions, what if there are *no* later users whatsoever? This is the point that people who never come close to the fabrication of science have the greatest difficulty in grasping. They imagine that all scientific articles are equal and arrayed in lines like soldiers, to be carefully inspected one by one. However, most papers are never read at all. No matter what a paper did to the former literature, if no one else does anything with it, then it is as if it never existed at all. You may have written a paper that settles a fierce controversy once and for all, but if readers ignore it, it cannot be turned into a fact; it simply *cannot*.

You may protest against the injustice; you may treasure the certitude of being right in your inner heart; but it will never go further than your inner heart; you will never go further in certitude without the help of others. Fact construction is so much a collective process that an isolated person builds only dreams, claims and feelings, not facts. As we will see later in Chapter 3, one of the main problems to solve is to interest someone enough to be read at all; compared to this problem, that of being believed is, so to speak, a minor task.

In the turmoil generated by more and more papers acting on more and more papers, it would be wrong to imagine that everything fluctuates. Locally, it happens that a few papers are always referred to by later articles with similar positive modalities, not only for one generation of articles but for several. This event – extremely rare by all standards – is visible every time a claim made by one article is borrowed without any qualification by many others. This means that anything it did to the former literature is turned into fact by whoever borrows it later on. The discussion, at least on this point, is ended. A black box has been produced. This is the case of the sentence 'fuel cells are the future of electric cars' inserted inside statements (8), (9) and (10). It is also the case for the control by the hypothalamus of growth hormone. Although Schally and Guillemin disagree on many things, this claim is borrowed by both without any qualification or misgivings – see sentences (19) and (20). In Figure 1.5 illustrating the context of

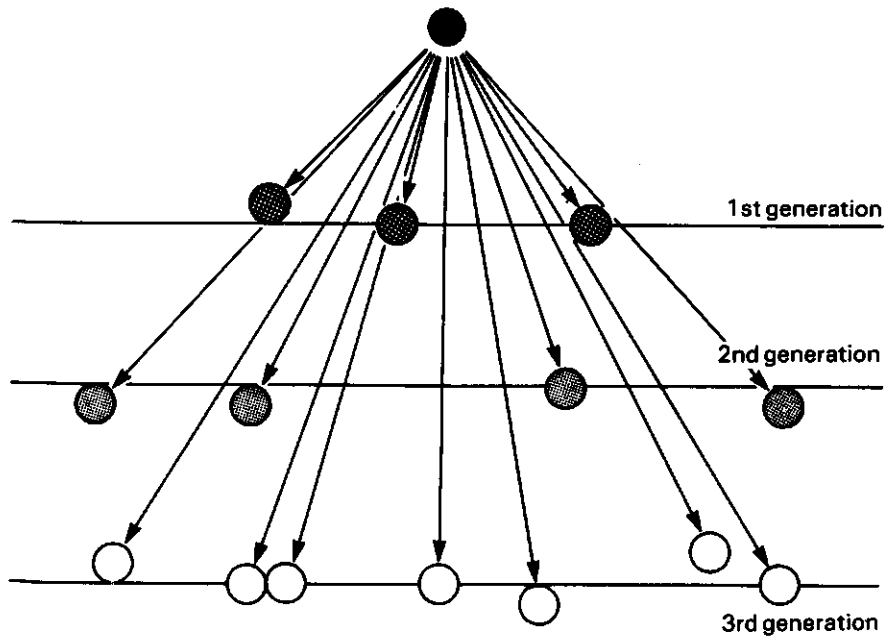


Figure 1.5

citations such an event will be seen as a regular flow of arrows all aligned in the same direction and leading to more and more papers. Every new paper getting into the fray pushes it one step further, adding its little force to the force of the already established fact, rather than reversing the trend.

This rare event is what people usually have in mind when they talk of a 'fact'. I hope it is clear by now that this event does not make it qualitatively different from fiction; a fact is what is collectively stabilised from the midst of controversies when the activity of later papers does not consist only of criticism or deformation but also of confirmation. The strength of the original statement does not lie in itself, but is derived from any of the papers that incorporate it. In principle, any of the papers could reject it. The control of growth hormone by the hypothalamus could be disputed, it has been, it will be disputed; but to do so the dissenter will be faced not with one claim in one paper, but with the same claims incorporated in hundreds of papers. It is not impossible in principle; it is just enormously difficult in practice. Each claim comes to the future author with its history, that is with itself plus all the papers that did something with it or to it.

This activity of each of the papers that makes up the strength of a given article is made visible not by any criticism – since in this case there is none – but by the erosion the original statement submits to. Even in the very rare cases where a statement is continuously believed by many later texts and borrowed as a matter of fact, it does not stay the same. The more people believe it and use it as a black box the more it undergoes transformations. The first of these transformations is an extreme *stylisation*. There is a mass of literature on the control of growth hormone, and Guillemin's article which I referred to is five pages long. Later papers, taking his article as a fact, turn it into one sentence:

> (24) Guillemin et al. (ref.) have determined the sequence of GRF: H Tyr Ala Asp Ala Ile Phe Thr Asn Ser Tyr Arg Lys Val Leu Gly Gln Leu Ser Ala Arg Lys Leu Leu Gln Asp Ile Met Ser Arg Gln Gln Gly Gly Ser Asn Gln Glu Arg Gly Ala Arg Ala Arg Leu NH2.

Later on this sentence itself is turned into a one-line long statement with only one simplified positive modality: 'X (the author) has shown that Y.' There is no longer any dispute.

If sentence (24) is to continue to be believed, as opposed to (5), each successive paper is going to add to this stylisation. The activity of all the later papers will result in the name of the author soon being dropped, and only the reference to Guillemin's paper will mark the origin of the sequence. This sequence in turn is still too long to write. If it becomes a fact, it will be included in so many other papers that soon it would not be necessary to write it at all or even to cite such a well-known paper. After a few dozen papers using statement (24) as an incontrovertible fact, it will be transformed into something like:

> (25) We injected sixty 20-day-old Swiss albino male mice with synthetic GRF . . . etc.

The accepted statement is, so to speak, eroded and polished by those who accept it. We are back to the single sentence statements with which I started this chapter – see (1), (5) and (8). Retrospectively, we realise that a lot of work went into this stylisation and that a one-phrase fact is never at the beginning of the process (as I had to imply in order to get our discussion going) but is already a semi-final product. Soon, however, the reference itself will become redundant. Who refers to Lavoisier's paper when writing the formula H_2O for water? If positive modalities continue acting on the same sentence (24), then it will become so well known that it will not be necessary even to talk about it. The original discovery will have become *tacit knowledge*. GRF will be one of the many vials of chemicals that any first year university student takes from the shelf at some point in his or her training. This erosion and stylisation happens only when all goes well; each successive paper takes the original sentence as a fact and encapsulates it, thereby pushing it, so to speak, one step further. The opposite happens, as we saw earlier, when negative modalities proliferate. Schally's sentence (5) about a new GHRH was not stylised and was still less incorporated into tacit practice. On the contrary, more and more elements he would have liked to maintain as tacit emerge and are talked about, like the purification procedures of statement (7) or his previous failures in (13). Thus, depending on whether the other articles push a given statement downstream or upstream, it will be incorporated into tacit knowledge with no mark of its having been produced by anyone, or it will be opened up and many specific conditions of production will be added. This double move with which we are now familiar is summarised in Figure 1.6 and allows us to take our bearings in any controversy depending on which stage the statement we chose as our point of departure happens to be and in which direction other scientists are pushing it.

Now we start to understand the kind of world into which the reader of scientific or technical literature is gradually led. Doubting the accuracy of Soviet missiles, (1), or Schally's discovery of GHRH, (5), or the best way to build fuel cells, (8), was at first an easy task. However, if the controversy lasts, more and more elements are brought in, and it is no longer a simple verbal challenge. We go from conversation between a few people to texts that soon fortify themselves, fending off opposition by enrolling many other allies. Each of these allies itself uses many different tactics on many other texts enlisted in the dispute. If no one takes up a paper, it is lost forever, no matter what it did and what it cost. If an article claims to finish the dispute once and for all it might be immediately dismembered, quoted for completely different reasons, *adding* one more empty claim to the turmoil. In the meantime, hundreds of abstracts, reports and posters get into the fray, adding to the confusion, while long review papers strive to put some order into the debates though often on the contrary simply adding more fuel to the fire. Sometimes a few stable statements are borrowed over and over again by many papers but even in these rare cases, the statement is slowly eroded, losing its original shape, encapsulated into more and more foreign statements, becoming so familiar and routinised that it becomes part of tacit practice and disappears from view!

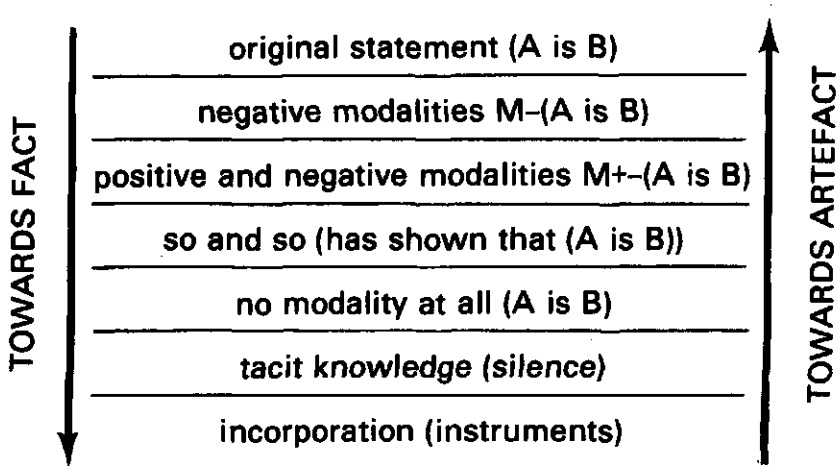


Figure 1.6

This is the world with which someone who wishes to dissent and make a contribution to the debates will be confronted. The paper he or she is reading has braced itself for survival in this world. What must it do in order to be read, to be believed, to avoid being misunderstood, destroyed, dismembered, ignored? How can it ensure that it is taken up by others, incorporated into later statements as a matter of fact, quoted, remembered and acknowledged? This is what has to be sought by the authors of a new technical paper. They have been led by the heated controversy into reading more and more articles. Now they have to *write* a new one in order to put to rest whichever issue they started from: the MX affair, the GHRH blunder, the fuel cell fiasco. Needless to say that, by now, *most dissenters will have given up.* Bringing friends in, launching many references, acting on all these quoted articles, visibly deploying this battlefield, is already enough to intimidate or to force most people out. For instance, if we wish to dispute the accuracy of Soviet missiles as in (1), the discovery of GHRH as in (5) or the right way to get at fuel cells as in (8), we will be very, very isolated. I do not say that because the literature *is too technical* it puts people off, but that, on the contrary, we feel it necessary to call technical or scientific a literature that is made to isolate the reader by bringing in many more resources. The 'average man who happens to hit the truth', naively postulated by Galileo, will have no chance to win over the thousands of articles, referees, supporters and granting bodies who oppose his claim. The power of rhetoric lies in making the dissenter feel lonely. This is indeed what happens to the 'average man' (or woman) reading the masses of reports on the controversies we so innocently started from.

Part C
Writing texts that withstand the assaults of a hostile environment

Although most people will have been driven away by the external allies invoked by the texts, Galileo is still right, because a few people may not be willing to give up. They may stick to their position and not be impressed by the title of the journal, the names of authors, or by the number of references. They will read the articles and still dispute them. The image of the scientific David fighting against the rhetorical Goliath reappears and gives some credence to Galileo's position. No matter how impressive the allies of a scientific text are, this is not enough to convince. Something else is needed. To find this something else, let us continue our anatomy of scientific papers.

(1) Articles fortify themselves

For a few obstinate readers, already published articles are not enough: more elements have to be brought in. The mobilisation of these new elements transforms deeply the manner in which texts are written: they become more technical and, to make a metaphor, **stratified**. In sentence (21), I quoted the beginning of a paper written by Guillemin. First, this sentence mobilised a two-decade-old fact, the control by the hypothalamus of the release of growth hormone, and then a decade-old fact, the existence of a substance, somatostatin, that inhibits the release of growth hormone. In addition, Schally's claim about this new substance was dismissed. But this is not enough to make us believe that Guillemin has done better than Schally and that his claim should be taken more seriously than that of Schally. If the beginning of his paper was playing on the existing literature in the manner I analysed above, it soon becomes very different. The text announces, for instance, more material from which to extract these elusive substances. The authors found a patient with enormous tumours formed in the course of a rare disease, acromegaly, these tumours producing large quantities of the sought-for substance.[9]

> *(26) At surgery, two separate tumors were found in the pancreas (ref. 6);* the tumor tissues were diced and collected in liquid nitrogen within 2 or 5 minutes of resection with the intent to extract them for GRF. (. . .) The extract of both tumors contained growth hormone releasing activity with the same elution volume as that of hypothalamic GRF (Kav=0,43, where Kav is the elution on constant (ref. 8). The amounts of GRF activity (ref. 9) were minute in one of the tumors (0.06 GRF unit per milligram (net weight), but extremely high in the other (1500 GRF units per milligram (net weight), 5000 times more than we had found in rat hypothalamus (ref. 8).

Now, we are in business! Sentence (26) appears to be the most difficult sentence

we have had to analyse so far. Where does the difficulty come from? From the number of objections the authors have to prevent. Reading it after the other sentences, we have not suddenly moved from opinions and disputes to facts and technical details; we have reached a state where the discussion is so tense that each word fences off a possible fatal blow. Going from the other disputes to this one is like going from the first elimination rounds to the final match at Wimbledon. Each word is a move that requires a long commentary, not because it is 'technical', but because it is the final match *after* so many contests. To understand this, we simply have to add the reader's objection to the sentence that answers it. This addition transforms sentence (26) into the following dialogue:

(27)– How could you do better than Schally with such minute amounts of your substance in the hypothamali?
– We find tumours producing masses of substance making isolation much easier than anything Schally could do.
– Are you kidding? These are pancreas tumours, and you are looking for a hypothalamic substance that is supposed to come from the brain!
– Many references indicate that often substances from the hypothalamus are found in the pancreas too, but anyway they have the same elution volume; this is not decisive but it is quite a good proof – enough, at any rate, to accept the tumour as it is, with an activity 5000 times greater than hypothalamic. No one can deny that it is a godsend.
– Hold on! How can you be so sure of this 5000; you cannot just conjure up figures? Is it dry weight or wet weight? Where does the standard come from?
– Okay. First, it is dry weight. Second, one GRF unit is the amount of a purified GRF preparation of rat hypothalamic origin that produces a half-maximal stimulation of growth hormone in the pituitary cell monolayer bioassay. Are you satisfied?
– Maybe, but how can we be sure that these tumours have not deteriorated after the surgery?
– We told you, they were diced and put in liquid nitrogen after 2 to 5 minutes. Where could you find better protection?

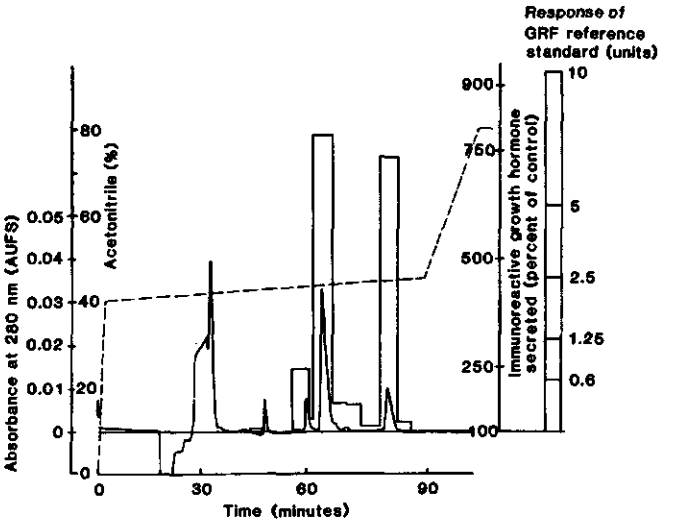
Reading the sentences of the paper without imagining the reader's objections is like watching only one player's strokes in the tennis final. They just appear as so many empty gestures. The accumulation of what appears as technical detail is not meaningless; it is just that it makes the opponent harder to beat. The author protects his or her text against the reader's strength. A scientific article becomes more difficult to read, just as a fortress is shielded and buttressed; not for fun, but to avoid being sacked.

Another deep transformation occurs in the texts that want to be strong enough to resist dissent. So far, the sentences we studied linked themselves to *absent* articles or events. Every time the opponent started to doubt, he or she was sent back to other texts, the link being established either by the references or sometimes by quotations. There is, however, a much more powerful ploy, and it is to *present* the very thing you want the readers to believe in the text. For instance:

(28) Final purification of this material by analytical reverse-phase HPLC yielded three highly purified peptides with GRF activity (Fig. 1)

The authors are not asking you to believe them. They do not send you back outside the texts to libraries to do your homework by reading stacks of references, but to figure 1 within the article:

(29)



Science, vol. 218, pp. 586 (by permission of *Science Magazine* and of the author)

This figure *shows* what the text *says*, but is not quite transparent for all readers, even for the few who are left in the controversy. Then another text, the legend, explains how to read the figure, as the name 'legend' indicates:

(30) Final purification of hpGRF by reverse-phase HPLC. The column (Ultrasphere C18), 25 by 0,4cm, 5-(pu)m particle size, was eluted with a gradient of acetonitrile (—) in 0.5 percent (by volume) heptafluorobutyric acid at a flow-rate of 0.6ml/min. Fractions (2.4 ml) were collected as indicated on the abscissa and portions were used for bioassays (ref. 7). The vertical bars represent the amount of growth hormone secreted in the assay of each fraction of the effluent, expressed as percentage of the amount of growth hormone secreted by the pituitary cells receiving no treatment. AUFS, absorbance units full scale.

The reader was sent from statement (28) to excerpt (29) and from there to the legend (30). The text said that 'three purified peptides had GRF activity'; what is seen in figure 1 is the superimposition of peaks and vertical bars. 'Peaks' and 'bars' are said in the legend to be the visual equivalents of 'purity' and 'activity'. Belief in the author's *word* is replaced by the inspection of 'figures'. If there is any doubt about where the picture comes from, then sentence (30), the legend, will offer a new line of support. Peaks are not a visual display chosen by chance; they

are what is drawn by an instrument (called a High Pressure Liquid Chromatograph); if the reader knows anything about the instrument and how different pictures can be obtained from it, then details are provided to hold the image steady: the size of particles, the timing, the conventions for drawing the lines, and so on.

What is gained in persuasion, by arraying excerpts (28), (29) and (30) in tiers? The dissenter is now faced not only with the author's opinion, not only with older articles' positions, but also with what the text is about. Often, when we talk, we designate absent things, which we call the **referent** of our speech. 'Six peach trees blooming' is a phrase about trees which I am not showing you. The situation is completely different when sentence (28) claims that three active and pure substances exist. The referent of this sentence is immediately added to the commentary; it is the figure shown in (29), and so is the referent of this referent, the legend (30). This transformation of the usual literature is a sure indicator that we are now faced with a technical or a scientific text. In this kind of literature you may, so to speak, have your cake and eat it too. The effects on conviction are enormous. The assertion 'we discovered GRF' does not stand *by itself*. It is supported first by many other texts and second by the author's assertions. This is good, but not enough. It is much more powerful if the supporters are arrayed in the text itself. How can you deny statement (28)? Look for yourself at the peaks in (29)! You are doubtful about the meaning of the figure? Well, read the legend. You only have to believe the evidence of your own eyes; this is not a question any more of belief; this is *seeing*. Even doubting Thomas would abandon his doubts (even though you cannot touch GRF – but wait until the next chapter . . .).

We are certain now that the texts we have been led to by the intensity of the controversies are scientific. So far, journalists, diplomats, reporters and lawyers could have written texts with references and with careful labelling of the authors' roles, titles and sources of support. Here, we enter another game entirely. Not because the prose is suddenly written by extraterrestrial minds, but because it tries to pack inside the text as many supporters as possible. This is why what is often called 'technical details' proliferate. The difference between a regular text in prose and a technical document is the stratification of the latter. The text is arranged in layers. Each claim is interrupted by references outside the texts or inside the texts to other parts, to figures, to columns, tables, legends, graphs. Each of these in turn may send you back to other parts of the same texts or to more outside references. In such a stratified text, the reader, once interested in reading it, is as free as a rat in a maze.

The transformation of linear prose into, so to speak, a folded array of successive defence lines is the surest sign that a text has become scientific. I said that a text without references was naked and vulnerable, but even with them it is weak as long as it is not stratified. The simplest way to demonstrate this change in solidity is to look at two articles in the same field taken at a twenty-year interval. Compare for instance the first primatology articles written by the pioneers of this field twenty years ago with one recent application of sociobiology to the study of primates written by Packer.[10] Visually, and even without reading the article, the

difference is striking. In both cases, it is about baboons, but the prose of the first article flows with no interruption except sparse references and a few pictures of baboons (like the ones you could find in a journalist's travel account); Packer's article, on the contrary, is stratified into many layers. Each observation of baboons is coded, sifted for its statistical significance; curves and diagrams summarise columns; no part of the paper stands by itself but each is linked by many references to other layers (Methods, Results, Discussion). Comparing Hall's and Packer's texts is like comparing a musket with a machine-gun. Just by looking at the differences in prose you can imagine the sort of worlds they had to write in: Hall was alone, one of the first baboon watchers; Packer is in a pack of scientists who watch closely not only baboons but also one another! His prose folds itself into many defensive layers to withstand their objections.

Notice that neither in Packer's nor in Guillemin's and Schally's articles do you see the actual furry creatures called 'baboons' or the 'GHRH'. Nevertheless, through their stratification, these articles give the reader an impression of *depth of vision*; so many layers supporting each other create a thicket, something you cannot breach without strenuous efforts. This impression is present even when the text is later turned into an artefact by colleagues. No one getting into the GRF business or into baboon study can now write in plain naked prose, no matter what he or she sees and wants. It would be like fighting tanks with swords. Even people who wish to defraud have to pay an enormous price in order to create this depth that resembles reality. Spector, a young biologist convicted of having fudged his data, had to hide his fraud in a four-page long section on Materials and Methods.[11] Inside the array of hundreds of methodological precautions only one sentence is fabricated. It is, so to speak, a homage rendered by vice to virtue, since such a fraud is not within the reach of just any crook!

At the beginning of this section, we said that we needed 'something other' than just references and authorities to win over the dissident. We understand now that going from the outer layers of the articles to the inner parts is not going from the argument of authority to Nature as it is going from authorities to more authorities, from numbers of allies and resources to still *greater numbers*. Someone who disbelieves Guillemin's discovery will now be faced not only with big names and thick references, but also with 'GRF units', 'elution volume', 'peaks and bars', 'reverse-phase HPLC'. Disbelieving will not only mean courageously fighting masses of references, but also unravelling endless new links that tie instruments, figures and texts together. Even worse, the dissenter will be unable to oppose the text to the real world out there, since the text claims to bring within it the real world 'in there'. The dissenter will indeed be isolated and lonely since the referent itself has passed into the author's camp. Could it hope to break the alliances between all these new resources inside the article? No, because of the folded, convoluted and stratified form the text has taken defensively, tying all its parts together. If one doubts figure 1 in excerpt (29), then one has to doubt reverse phase HPLC. Who wishes to do so? Of course, any link can be untied, any instrument doubted, any black box reopened, any figure dismissed, but the accumulation of allies in the author's camp is quite

formidable. Dissenters are human too; there is a point where they cannot cope against such high odds.

In my anatomy of scientific rhetoric I keep shifting from the isolated reader confronted by a technical document to the isolated author launching his document amidst a swarm of dissenting or indifferent readers. This is because the situation is symmetrical: if isolated, the author should find new resources to convince readers; if he or she succeeds then each reader is totally isolated by a scientific article that links itself to masses of new resources. In practice, there is only one reversible situation, which is just the opposite of that described by Galileo: how to be 2000 against one.

(2) Positioning tactics

The more we go into this strange literature generated by controversies, the more it becomes difficult to read. This difficulty comes from the number of elements simultaneously gathered at one point – the difficulty is heightened by the acronyms, symbols and shorthand used in order to stack in the text the maximum number of resources as quickly as possible. But are numbers sufficient to convince the five or six readers left? No, of course head counts are no more sufficient in scientific texts than in war. Something more is needed: numbers must be arrayed and drilled. What I will call their **positioning** is necessary. Strangely, this is easier to understand than what we have just described since it is much closer to what is commonly called rhetoric.

(a) STACKING

Bringing pictures, figures, numbers and names into the text and then folding them is a source of strength, but it may also turn out to be a major weakness. Like references (see above Part B, section 2), they show the reader what a statement is tied to, which also means the reader knows where to pull if he or she wishes to unravel the statement. Each layer should then be carefully stacked on the former to avoid gaps. What makes this operation especially difficult is that there are indeed many gaps. The figure in excerpt (29) does not show GRF; it shows two superimposed pictures from one protocol in one laboratory in 1982; these pictures are said to be related to two tumours from one French patient in a Lyon hospital. So what is shown? GRF or meaningless scribbles on the printout of an instrument hooked up to a patient? Neither the first, nor the second. It depends on what happens to the text later on. What is shown is a stack of layers, each one ***adding something*** to the former. In Figure 1.7 I picture this stacking using another example. The lowest layer is made of three hamster kidneys, the highest, that is the title, claims to show 'the mammal countercurrent structure in kidney'. In dark lines I have symbolised the gain from one layer to the next. A text is like a bank; it lends more money than it has in its vault! The metaphor is a good one

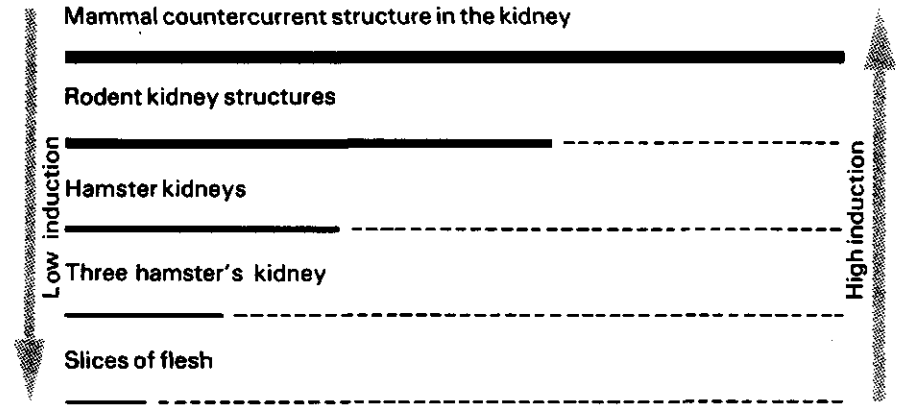


Figure 1.7

since texts, like banks, may go bankrupt if all their depositors simultaneously withdraw their confidence.

If all goes well, then the article sketched in Figure 1.7 has shown mammal kidney structure; if all goes badly, it shrinks to three hamsters in one laboratory in 1984. If only a few readers withdraw their confidence, the text lingers in any of the intermediate stages: it might show hamster kidney structure, or rodent kidney structure, or lower mammal kidney structure. We recognise here the two directions in fact-building or fact-breaking that we discussed earlier.

This extreme variation between the lower and the upper layers of a paper is what philosophers often call **induction**. Are you allowed to go from a few snippets of evidence to the largest and wildest claims? From three hamsters to the mammals? From one tumour to GRF? These questions have no answer in principle since it all depends on the intensity of the controversies with other writers. If you read Schally's article now, you do not see GHRH, but a few meaningless bars and spots; his claim 'this is the GHRH structure' which was the content of sentence (5), is now seen as an empty bluff, like a cheque that bounced. On the contrary, reading Guillemin's article, you *see* GRF in the text because you believe his claim expressed in sentence (24). In both cases the belief and the disbelief are making the claim more real or less real later on. Depending on the field, on the intensity of the competition, on the difficulty of the topic, on the author's scruples, the stacking is going to be different. No matter how different the cases we could look at, the name of the game is simple enough. First rule: never stack two layers exactly one on top of the other; if you do so there is no gain, no increment, and the text keeps repeating itself. Second rule: never go straight from the first to the last layer (unless there is no one else in the field to call your bluff). Third rule (and the most important): prove as much as you can with as little as you can considering the circumstances. If you are too timid, your paper will be lost, as it will if you are too audacious. The stacking of a paper is similar to

the building of a stone hut; each stone must go further than the one before. If it goes too far, the whole vault falls down; if not far enough, there will be no vault at all! The practical answers to the problem of induction are much more mundane than philosophers would wish. On these answers rests much of the strength that a paper is able to oppose to its readers' hostility. Without them, the many resources we analysed above remain useless.

(b) STAGING AND FRAMING

No matter how numerous and how well stacked its resources, an article has not got a chance if it is read just by *any* passing reader. Naturally, most of the readership has already been defined by the medium, the title, the references, the figures and the technical details. Still, even with the remainder it is still at the mercy of malevolent readers. In order to defend itself the text has to explain how and by whom it should be read. It comes, so to speak, with its own user's notice, or legend.

The image of the ideal reader built into the text is easy to retrieve. Depending on the author's use of language, you immediately imagine to whom he or she is talking (at least you realise that in most cases he or she is not talking to you!). Sentence (24), that defined the amino acid structure of GRF, is not aimed at the same reader as the following:

> (31) There exists a substance that regulates body growth; this substance is itself regulated by another one, called GRF; it is made of a string of 44 amino acids (amino acids are the building block of all proteins); this string has recently been discovered by the Nobel Prize winner Roger Guillemin.

Such a sentence is addressed to a completely different audience. More people are able to read it than sentence (24) or (26). *More* people but equipped with *fewer* resources. Notice that popularisation follows the same route as controversy but in the opposite direction; it was because of the intensity of the debates that we were slowly led from non-technical sentences, from large numbers of ill-equipped verbal contestants to small numbers of well-equipped contestants who write articles. If one wishes to increase the number of readers again, one has to decrease the intensity of the controversy, and reduce the resources. This remark is useful because the difficulty of writing 'popular' articles about science is a good measure of the accumulation of resources in the hands of few scientists. It is hard to popularise science because it is designed to force out most people in the first place. No wonder teachers, journalists and popularisers encounter difficulty when we wish to bring the excluded readership back in.

The kind of words authors use is not the only way of determining the ideal reader at whom they are aiming. Another method is to anticipate readers' objections in advance. This is a trick common to all rhetoric, scientific or not. 'I knew you would object to this, but I have already thought of it and this is my answer.' The reader is not only chosen in advance, but what it is going to say is

taken out of its own mouth, as I showed for instance in excerpt (27) (I use 'it' instead of 'he or she' because this reader is not a person in the flesh but a person on paper, a **semiotic character**).[12] Thanks to this procedure, the text is carefully aimed; it exhausts all potential objections in advance and may very well leave the reader speechless since it can do nothing else but take the statement up as a matter of fact.

What sort of objections should be taken into account by the author? Again, this is a question that philosophers try to answer in principle although it only has practical answers, depending on the battlefield. The only rule is to ask the (imaginary) reader what sort of **trials** it will require before believing the author. The text builds a little story in which something incredible (the hero) becomes gradually more credible because it withstands more and more terrible trials. The implicit dialogue between authors and readers then takes something of this form:

> (34)–If my substance triggers growth hormone in three different assays, will you believe it to be GRF?
>
> –No, this is not enough, I also want you to show me that your stuff from a pancreas tumour is the same as the genuine GRF from the hypothalamus.
>
> –What do you mean 'the same'; what trials should my stuff, as you say, undergo to be called 'genuine GRF'?
>
> –The curves of your stuff from the pancreas and GRF from the hypothalamus should be superimposed; this is the trial I want to see with my own eyes before I believe you. I won't go along with you without it.
>
> –This is what you want? And after that you give up? You swear? Here it is: see figure 2, perfect superimposition!
>
> –Hold on! Not so fast! This is not fair; what did you do with the curves to get them to fit?
>
> –Everything that could be done given the present knowledge of statistics and today's computers. The lines are theoretical, computer-calculated and drawn, from the four-parameter logistic equations for each set of data! Do you give up now?
>
> –Yes, yes, certainly, I believe you!

'It' gives up, the imaginary reader whose objections and requirements have been anticipated by the master author!

Scientific texts look boring and drab from the most superficial point of view. If the reader recomposes the challenge they take up, they are as thrilling as story telling. 'What is going to happen to the hero? Is it going to resist this new ordeal? No, it is too much even for the best. Yes, it did win? How incredible. Is the reader convinced? Not yet. Ah hah, here is a new test; impossible to meet these requirements, too tough. Unfair, this is unfair.' Imagine the cheering crowds and the boos. No character on stage is watched with such passion and asked to train and rehearse as is, for instance, this GRF stuff.

The more we get into the niceties of the scientific literature, the more extraordinary it becomes. It is now a real opera. Crowds of people are mobilised by the references; from offstage hundreds of accessories are brought in. Imaginary readers are conjured up which are not asked only to believe the author

but to spell out what sort of tortures, ordeals and trials the heroes should undergo before being recognised as such. Then the text unfolds the dramatic story of these trials. Indeed, the heroes triumph over all the powers of darkness, like the Prince in *The Magic Flute*. The author adds more and more impossible trials just, it seems, for the pleasure of watching the hero overcoming them. The authors challenge the audience and their heroes sending a new bad guy, a storm, a devil, a curse, a dragon, and the heroes fight them. At the end, the readers, ashamed of their former doubts, have to accept the author's claim. These operas unfold thousands of times in the pages of *Nature* or the *Physical Review* (for the benefit, I admit, of very, very few spectators indeed).

The authors of scientific texts do not merely build readers, heroes and trials into the paper. They also make clear who they are. The authors in the flesh become the authors on paper, adding to the article more semiotic characters, more 'its'. The six authors of what I called Guillemin's paper did not, of course, write it. No one could remember how many drafts the paper passed through. The attribution of these six names, the order in which they enter, all that is carefully staged, and since this is one part of the writing of the plot, it does not tell us *who* writes the plot.

This obvious staging is not the only sign of the authors' presence. Although technical literature is said to be impersonal, this is far from being so. The authors are everywhere, built into the text. This can be shown even when the passive voice is used – this trait being often invoked to define scientific style. When you write: 'a portion of tissue from each tumour was extracted, a picture of the author is drawn as much as if you write 'Dr Schally extracted' ' or 'my young colleague Jimmy extracted'. It is just another picture; a grey backdrop on a stage is as much a backdrop as a coloured one. It all depends on the effects one wishes to have on the audience.

The portrayal of the author is important because it provides the imaginary counterpart of the reader; it is able to control how the reader should read, react and believe. For instance, it often positions itself in a genealogy which already presages the discussion:

> (33) Our conception of the hamster kidney structure has recently been dramatically altered by Wirz's observations (reference). We wish to report a new additional observation.

The author of this sentence does not portray itself as a revolutionary, but as a follower; not as a theoretician, but as a humble observer. If a reader wishes to attack the claim or the theory, it is redirected to the 'dramatic' transformations Wirz made and to the 'conceptions' he had. To show how such a sentence makes up a certain image of the author, let us rewrite it:

> (34) Wirz (reference) recently observed a puzzling phenomenon he could not interpret within the classical framework of kidney structure. We wish to propose a new interpretation of his data.

The article has immediately changed tack. It is now a revolutionary article and a theoretical one. Wirz's position has been altered. He was the master; he is now a precursor who did not know for sure what he was doing. The reader's expectations will be modified depending on which version the author chooses. The same changes will occur if we fiddle with sentence (21), which was the introduction to the paper written by Guillemin to announce the discovery of GRF. Remember that Schally's earlier endeavours were dismissed with the sentence: 'so far, hypothalamic GRF has not been unequivocally characterised, despite earlier claims to the contrary'. What does the reader feel if we now transform sentence (21) into this one:

> (35) Schally (reference) earlier proposed a characterisation of hypothalamic GRF; the present work proposes a different sequence which might solve some of the difficulties of the former characterisation.

The reader of sentence (21) is expecting truth at last after many senseless attempts at finding GRF, whereas the reader of (35) is prepared to read a new tentative proposition that situates itself in the same lineage as the former. Schally is a nonentity in the first case, an honourable colleague in the second. Any change in the author's position in the text may modify the readers' potential reactions.

Especially important is the staging by the author of what should be discussed, what is really interesting (what is especially important!) and what is, admittedly, disputable. This hidden agenda, built into the text, paves the way for the discussion. For instance, Schally, at the end of the article that I have used all along as an example, is suddenly not sure of anything any more. He writes:

> (36) Whether this molecule represents the hormone which is responsible for the stimulation of growth hormone released under physiological conditions can only be proven by further studies.

This is like taking out an insurance policy against the unexpected transformation of facts into artefacts. Schally did not say that he found 'the' GHRH, but only 'a' molecule that looked like GHRH. Later on, when he was so violently criticised for his blunder, he was then able to say that he never claimed that GHRH was the molecule cited in claim (5).

This caution is often seen as the sign of scientific style. Understatement would then be the rule and the difference between technical literature and literature in general would be the multiplication of negative modalities in the former. We now know this to be as absurd as saying that one walks only with one's left leg. Positive modalities are as necessary as negative ones. Each author allocates what shall not be discussed and what ought to be discussed (see again (21)). When it is necessary not to dispute a black box there is no understatement whatsoever. When the author is on dangerous ground, understatement proliferates. Like all the effects we have seen in this section, it all depends on circumstances. It is impossible to say that technical literature always errs on the side of caution; it also errs on the side of audacity; or rather it does not err, it zigzags through obstacles, and evaluates the risks as best it can. Guillemin, for instance, at the end of his paper runs hot and cold at the same time:

(39) What can certainly be said is that the molecule we have now characterized has all the attributes expected from the long-sought hypothalamic releasing factor for growth hormone.

Schally's caution is gone. The risk is taken; certainty is on their side: the new substance does everything that GRF does. The author simply stops short of saying 'this *is* GRF'. (Note that the author happily uses 'we' and the active voice when summarising its victory.) But the next paragraph adopts entirely different tactics:

(38) In keeping with other past experience, probably the most interesting role, effect, or use of GRF is currently totally unsuspected.

This is indeed an insurance policy against the unknown. No one will be able to criticise the author for its lack of vision, since the unexpected is expected. By using such a formula, the author protects itself against what happened in the past with another substance, somatostatin.[13] Originally isolated in the hypothalamus to inhibit the release of growth hormone, it turned out to be in the pancreas and to play a role in diabetes. But Guillemin's group missed this discovery that others made with their own substance. So, is the author cautious or not? Neither. It carefully writes to protect its claims as best as it can and to fence off the reader's objections.

Once a paper is written, it is very difficult to retrieve the careful tactics through which it was crafted, although a look at the drafts of scientific articles will be enough to show that the real authors are quite self-conscious about all of this. They know that without rewriting and positioning, the strength of their paper will be spoiled, because the authors and the readers built into the text do not match. Everything is at the mercy of a few ill-chosen words. The claim may become wild, the paper controversial, or, on the contrary, so timid and over-cautious, so polite and tame that it lets others reap the major discoveries.

(c) CAPTATION

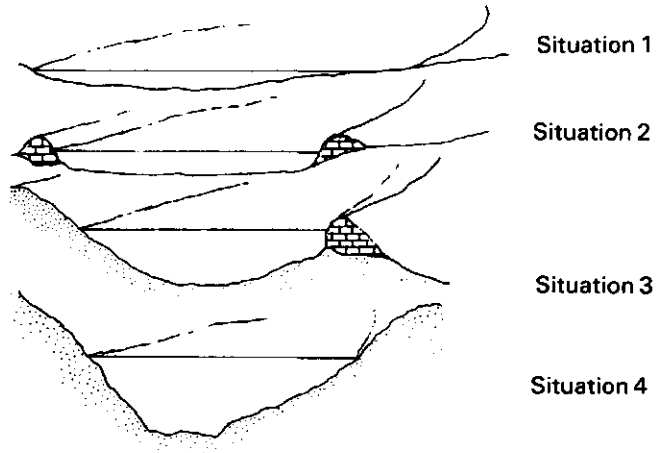
It may be discouraging for those of us who want to write powerful texts able to influence controversies, but even the enormous amount of work shown above is not enough! Something is still missing. No matter how many references the author has been able to muster; no matter how many resources, instruments and pictures it has been capable of mobilising in one place; no matter how well arrayed and drilled its troops are; no matter how clever its anticipation of what the readers will do and how subtle the presentation of itself; no matter how ingenious the choice of which ground should be held and which may be abandoned; regardless of all these strategies, the real reader, the reader in the flesh, the 'he' or 'she' may still *reach different conclusions.* Readers are devious people, obstinate and unpredictable – even the five or six left to read the paper from beginning to end. Isolated, surrounded, besieged by all your allies, they can still escape and conclude that Soviet missiles are accurate to within 100 metres,

that you have not proven the existence of GHRH or GRF, or that your paper on fuel cells is a mess. The paper-reader, the 'it' of, for instance, statement (32), may have stopped discussing and admitted the writer's credibility; but what about the real reader? He or she might have skipped a passage entirely, focused on a detail marginal to the author. The author told them in claim (21) that hypothalamus control of growth hormone is indisputable: are they going to follow him? It told them in (36) what was to be discussed; are they going to accept this agenda? The writer draws so many pathways going from one place to another and asks the reader to follow them; the readers may cross these paths and then escape. To come back to Galileo's sentence, 2000 Demosthenes and Aristotles are still weak if one average reader is allowed to break away and flee. All the numbers amassed by the technical literature are not enough if the reader is allowed to stroll and wander. All the objectors' moves should then be controlled so that they encounter massive numbers and are defeated. I call **captation** (or captatio in the old rhetoric) this subtle control of the objectors' moves.[14]

Remember that the authors need the readers' willingness to have their own claims turned into facts (see Part A, section 2). If the readers are put off, they are not going to take up the claim; but if they are left free to discuss the claim, it will be deeply altered. The writer of a scientific text is then in a quandary: how to leave someone completely free and have them at the same time completely obedient. What is the best way to solve this paradox? To lay out the text so that wherever the reader is there is only *one way* to go.

But how can this result be achieved, since by definition the real reader may *dispute everything and go in any direction? By making it more difficult for the* reader to go in all the other directions. How can this be achieved? By carefully stacking more black boxes, less easily disputable arguments. The nature of the game is exactly like that of building a dam. It would be foolish for a dam engineer to suppose that the water will obey his wishes, abstaining from overflowing or politely running from bottom to top. On the contrary, any engineer should start with the principle that if water can leak away it will. Similarly with readers, if you leave the smallest outlet open to them they will rush out; if you try to force them to go upstream they will not. So what you have to do is to make sure the reader always flows freely but *in a deep enough valley*! Since the beginning of this chapter we have observed this digging, trenching and damming many times over. All the examples moved from a better-known statement to a lesser-known one; all were using a less easily disputable claim to start or to stop discussion on a statement easier to dispute. Each controversy aimed at reversing the flow by shifting negative and positive modalities. Captation is a generalisation of the same phenomenon inducing readers to move far away from what they were ready to accept at first. If the digging and damming is well set up, the reader, although taken in, will feel entirely free (see Figure 1.8).

The hydraulic metaphor is an apt one since the scale of public work to be undertaken depends on how far you wish to force the water to go, on the intensity of the flow, on the slope and on what kind of landscape you have to buttress the dams and the ducts. It is the same thing with persuasion. It is an easy job if you



From unconvincing to incontrovertible evidence...

Figure 1.8

want to convince a few people of something that is almost obvious; it is much harder if you wish to convince a large number of people of something very remote from or even contrary to their current beliefs (see Chapter 5, Part C). This metaphor shows that the relation between the amount of work and persuasion depends on the circumstances. Convincing is not just a matter of throwing words about. It is a race between the authors and the readers to control each other's moves. It would be enormously difficult for one 'average man' to force off their paths '2000 Demosthenes and Aristotles' in a matter where, at first sight, every direction is equally possible; the only way to decrease the difficulty is to dam up all the alternative channels. No matter where the reader is in the text, he or she is confronted with instruments harder to discuss, figures more difficult to doubt, references that are harder to dispute, arrays of stacked black boxes. He or she flows from the introduction to the conclusion like a river flowing between artificial banks.

When such a result is attained – it is very rare – a text is said to be **logical**. Like the words 'scientific' or 'technical', it seems that 'logical' often means a different literature from the illogical type that would be written by people with different kinds of minds following different methods or more stringent standards. But there is no absolute break between logical and illogical texts; there is a whole gamut of nuances that depend as much on the reader as on the author. Logic refers not to a new subject matter but to simple practical schemes: Can the reader get out? Can he easily skip this part? Is she able, once there, to take another path? Is the conclusion escapable? Is the figure waterproof? Is the proof tight enough? The writer arrays whatever is at hand in tiers so that these questions find practical answers. This is where style starts to count; a good scientific writer may succeed in being 'more logical' than a bad one.

The most striking aspect of this race between the reader and the writer is when the limits are reached. In principle, of course, there is no limit since the fate of the statement is, as I said, in later users' hands (see Chapter 2, Part C). It is always possible to discuss an article, an instrument, a figure; it is always possible for a reader-in-the-flesh to move off the path expected of the reader-in-the-text. In practice, however, limits are reached. The author obtains this result by stacking so many tiers of black boxes that at one point the reader, obstinate enough to dissent, will be confronted with facts so old and so unanimously accepted that in order to go on doubting he or she will be *left alone*. Like a clever engineer who decides to build her dam on solid bedrock, the writer will manage to link the fate of the article to that of harder and harder facts. The practical limit is reached when the average dissenter is no longer faced with the author's opinion but with what thousands and thousands of people have thought and asserted. Controversies have an end after all. The end is not a natural one, but a carefully crafted one like those of plays or movies. If you still doubt that the MX should be built (see (1)), or that GHRH has been discovered by Schally (see (5)), or that fuel cells are the future of the electric engine (see (8)), then you will be all by yourself, without support and ally, alone in your profession, or, even worse, isolated from the community, or maybe, still more awful, sent to an asylum! It is a powerful rhetoric that which is able to drive the dissenter mad.

(3) The second rule of method

In this chapter we have learned a **second rule of method** in addition to the first one that required us to study science and technology in action. This second rule asks us not to look for the intrinsic qualities of any given statement but to look instead for all the transformations it undergoes later in other hands. This rule is the consequence of what I called our first principle: the fate of facts and machines is in the hands of later users.

These two rules of method taken together allow us to continue our trip through technoscience without being intimidated by the technical literature. No matter what controversy we start from, we will always be able to take our bearings:

(a) by looking at the stage the claim we chose as our departure point is at;
(b) by finding the people who are striving to make this claim more of a fact and those who are trying to make it less of a fact;
(c) by checking in which direction the claim is pushed by the opposite actions of these two groups of people; is it up the ladder drawn in Figure 1.5 or down?

This initial enquiry will give us our first bearing (our latitude so to speak). Then, if the statement we follow is quickly destroyed, we will have to see how it is transformed and what happens to its new version: is it more easily accepted or less? The new enquiry will offer us:

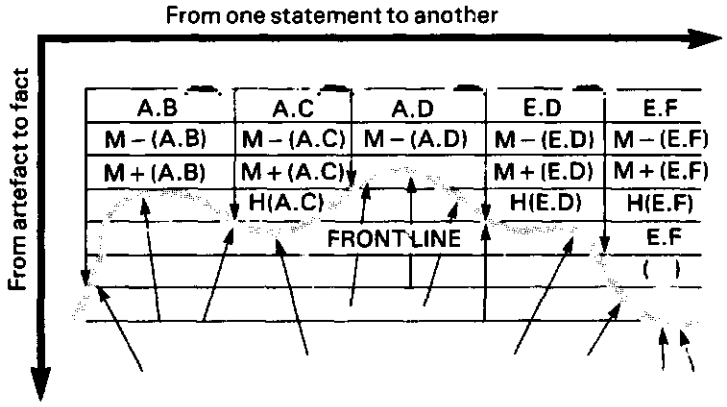


Figure 1.9

(d) 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:

(e) 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 untying 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 dissident driven into isolation because of the number of elements the authors of scientific articles mustered on their side. Although it sounds counter-intuitive 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.

Notes

Introduction

1 I am following here James Watson's account (1968).
2 I am following here Tracy Kidder's book (1981). This book, like Watson's, is compulsory reading for all of those interested in science in the making.
3 On this episode see T.D. Stokes (1982).
4 This notion of under-determination is also called the Duhem-Quine principle. It asserts that no one single factor is enough to explain the closure of a controversy or the certainty acquired by scientists. This principle forms the philosophical basis of most social history of sociology of science.

Chapter 1

1 This debate about the MX weapon system has been the object of a long public controversy in the USA.
2 This example is taken from Nicholas Wade (1981). The rest of the controversy is inspired from the book, although it is in part fictional.
3 This example is taken from Michel Callon (1981).
4 Cited in S. Drake (1970, p. 71).
5 I am using here the following article: A. V. Schally, V. Baba, R. M. G. Nair, C. D. Bennett (1971), 'The amino-acid sequence of a peptide with growth hormone-releasing isolated from porcine hypothalamus', *Journal of Biological Chemistry*, vol. 216, no. 21, pp. 6647–50.
6 The field of citation studies has become an independent sub-discipline. For a review see E. Garfield (1979) or the review *Scientometrics* for more recent and more specialised examples. For the context of citation, see M. H. MacRoberts and B. R. MacRoberts (1986).
7 This expression has become traditional since the work of Thomas Kuhn (1962).
8 The Science Citation Index is produced by the Institute for Scientific Information in Philadelphia and has become the basis of much work in science policy.
9 I am using here the following article: R. Guillemin, P. Brazeau, P. Böhlen, F. Esch, N. Ling, W. B. Wehrenberg (1982), 'Growth-hormone releasing factor from a human pancreatic tumor that caused acromegaly', *Science*, vol. 218, pp. 585–7.
10 The article commented on here is by C. Packer, 'Reciprocal altruism in papio P.', *Nature* 1977 Vol. 265, no. 5593, pp. 441–443. Although this transformation of the literature is a sure telltale of the differences between harder and softer fields, I know of no systematic study of this aspect. For a different approach and on the articles in physics see C. Bazerman (1984).
11 See M. Spector, S. O'Neal, E. Racker (1980), 'Regulation of phosporylation of the ß-subunit of the Ehrlich Ascites tumor Na→K→-ATPase by a protein kinase cascade'. *Journal of Biological Chemistry*, vol. 256, no. 9 pp. 4219–27. On this and many other borderline cases, see W. Broad and N. Wade (1982).
12 For a general presentation see M. Callon, J. Law and A. Rip (eds) (1986).
13 On the somatostatin episode see Wade (1981, chapter 13).
14 For a good introduction or rhetoric in settings other than the scientific ones see C. Perelman (1982).