SCIENCE IN ACTION

Bruno Latour

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How to follow scientists and engineers through society

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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.

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:

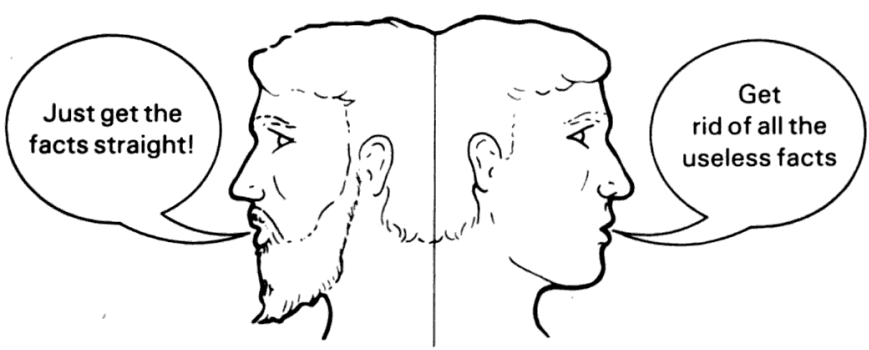

Figure I.2

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

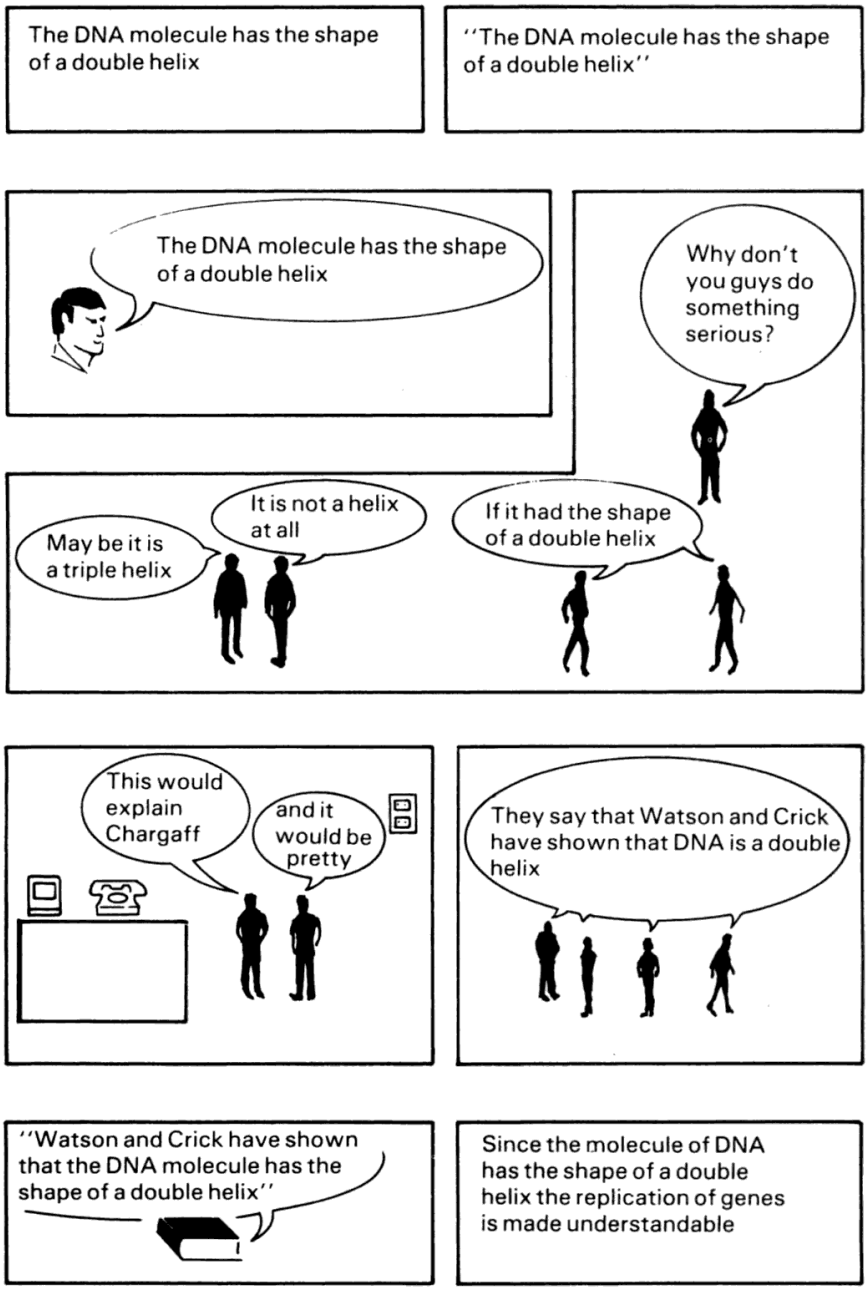

Figure I.6

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.

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* 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

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.


image
not
available

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]:

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

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

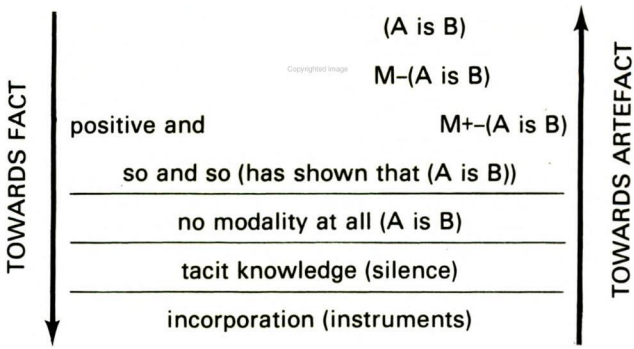

Figure 1.6

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.

Bruno Latour provides a lively and challenging analysis of science, demonstrating how social context and technical content are both essential to a proper understanding of scientific activity. Emphasizing that science can only be understood through its practice, the author examines science and technology *in action:* the role of scientific literature, the activities of laboratories, the institutional context of science in the modern world, and the means by which inventions and discoveries become accepted.

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