SCIENCE IN ACTION

How to follow scientists and engineers through society

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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 disagregate 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 Minutemer 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 Motor shares. Who is right? Whom should you believe? The answer to this question is not in any one of the statements, but in what everyone is going to do with them later on. If you wish to buy a car, will you be stopped by the high price of petrol? Will you shift to electric cars, more sluggish but cheaper? If you do so, then sentence (15) is wrong, and (8), (9) or (10) was right, since they all wanted electric cars. If the consumer buys an internal combustion engine car without any hesitation and doubts, then claim (15) is right and all the others were wrong to invest millions in useless technologies without a future.

This retrospective transformation of the truth value of earlier sentences does not happen only when the average consumer at the end of the line gets into the picture, but also when the Board of Directors decides on a research strategy. Suppose that you 'bought the argument' presented in statement (10). You go for electric cars, you believe in fuel cells, and in quantum physics as the only way to

get at them. All the other statements are *made more wrong* by this decision. The linkages between the future of the automobile, the electric engine, the fuel cells, and electrophysics are all conflated in one single black box which no one in the company is going to dispute. Everyone in the company will start from there: 'Since sentence (10) is right then let's invest so many millions.' As we will see in Chapter 3, this does not mean that your company will win. It means that, as far as you could, you shaped the other machines and facts of the past so as to win: the internal combustion engine is weakened by your decision and made more of an obsolete technology; by the same token electrophysics is strengthened, while the metallurgy section of the company is gently excluded from the picture. Fuel cells now have one more powerful ally: the Board of Directors.

Again I interrupt the controversy abruptly for practical reasons; the company may go broke, become the IBM of the twenty-first century or linger for years in limbo. The point of the three examples is that *the fate of what we say and 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 millenia, 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!

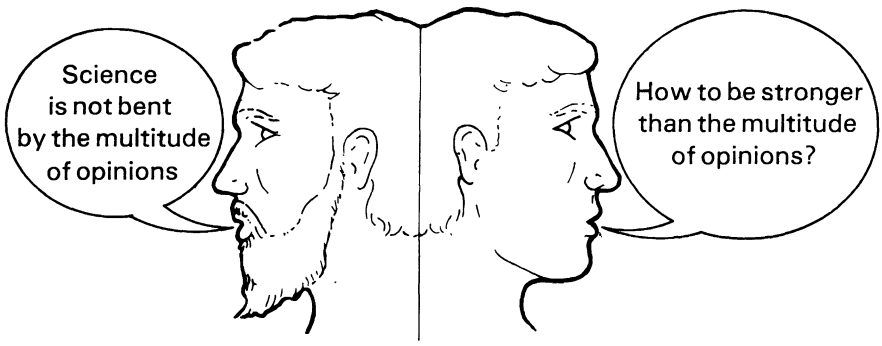


Figure 1.1

'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.

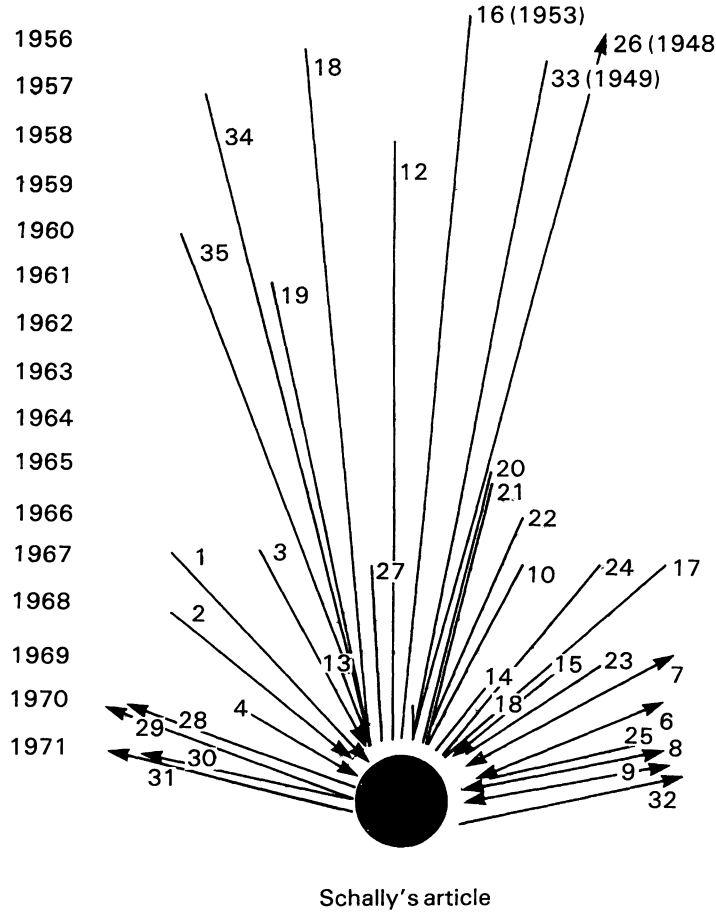
(2) Referring to former texts

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



—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.

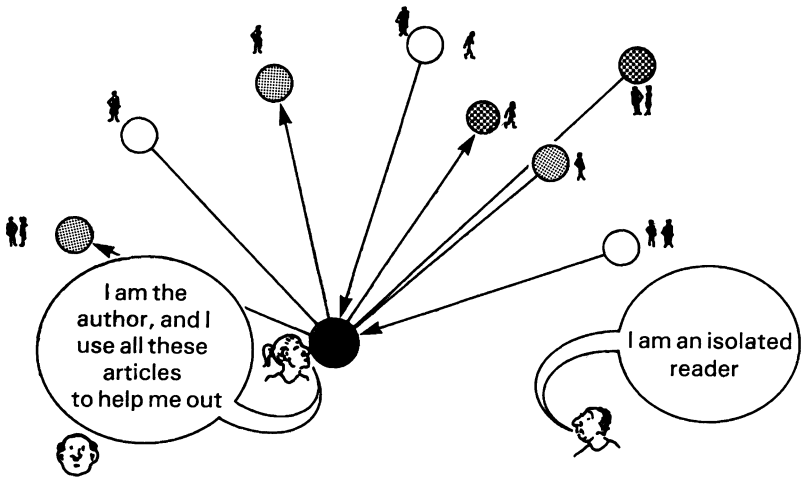


Figure 1.3

(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.)

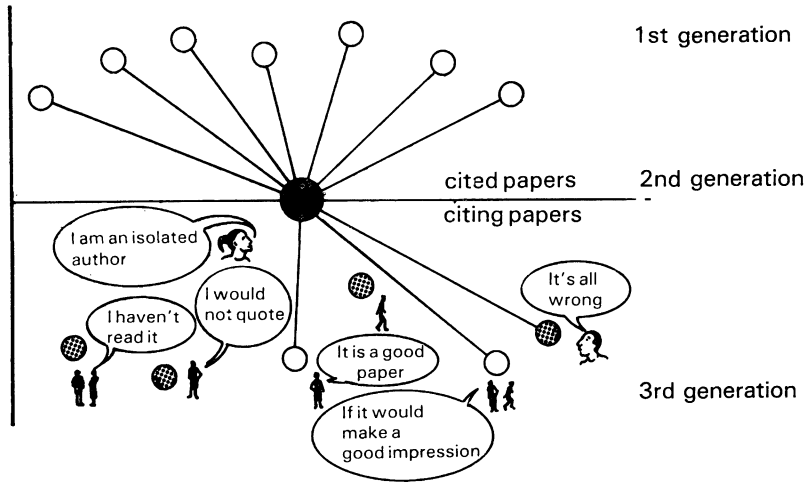


Figure 1.4

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 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

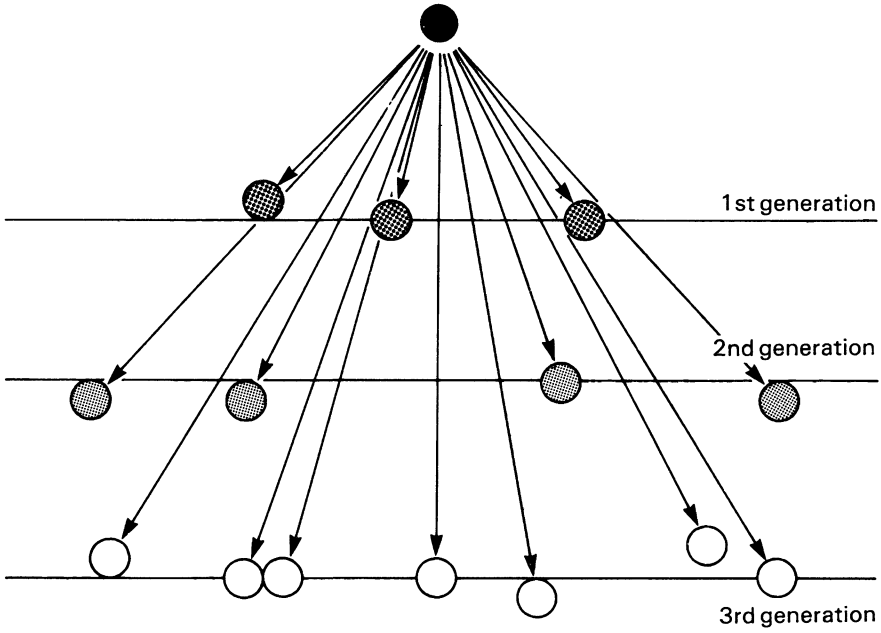


Figure 1.5

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!

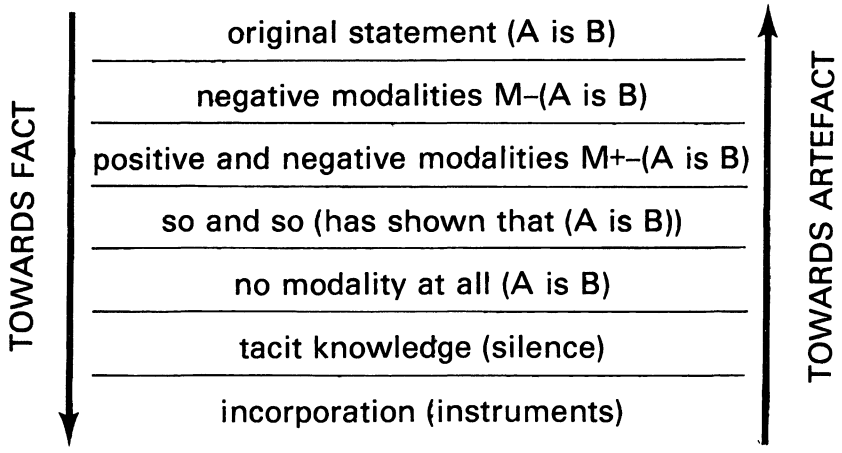


Figure 1.6

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?

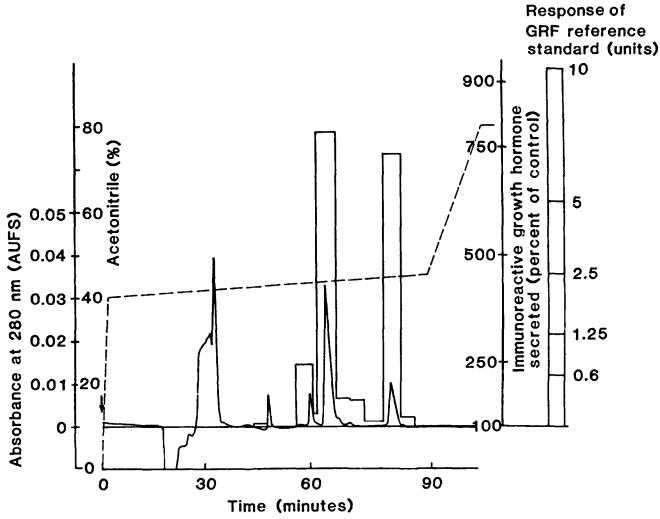
Reading the sentences of the paper without imagining the reader's objections is like watching only one player's strokes in the tennis final. They just appear as so many empty gestures. The accumulation of what appears as technical detail is not meaningless; it is just that it makes the opponent harder 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 hid 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

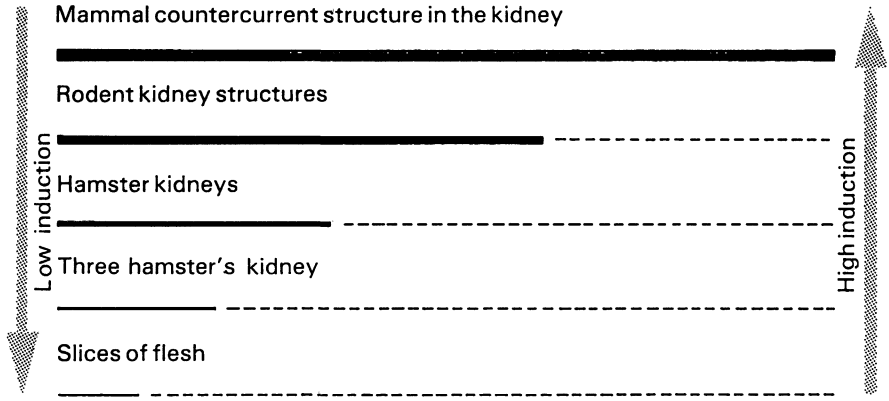


Figure 1.7

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

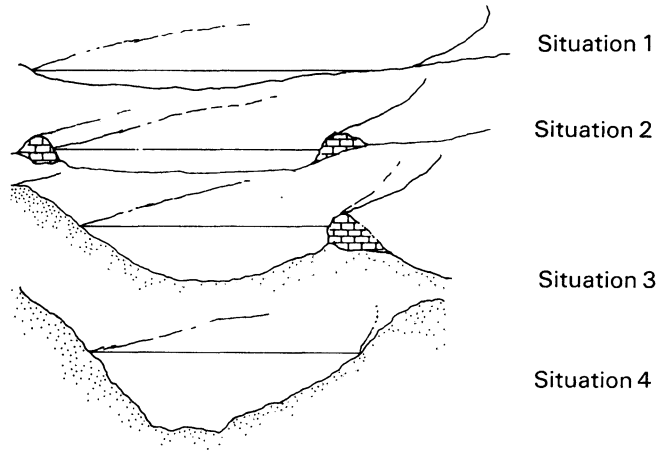


Figure 1.8

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:

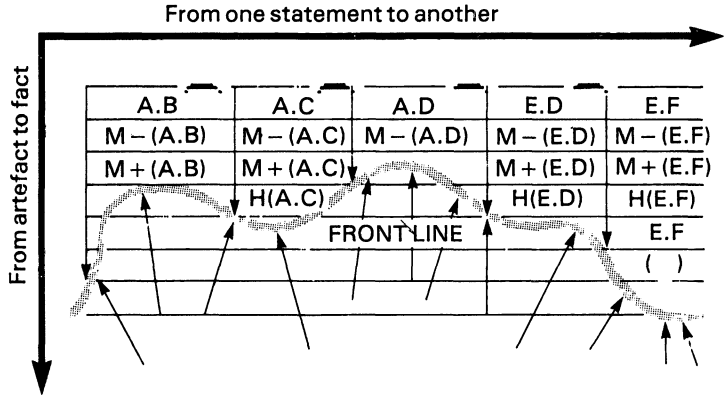


Figure 1.9

(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.

CHAPTER 2

Laboratories

We could stop our enquiry where we left it at the end of the previous chapter. For a layperson, studying science and technology would then mean analysing the discourse of scientists, or counting citations, or doing various bibliometric calculations, or performing semiotic studies[1] of scientific texts and of their iconography, that is, extending literary criticism to technical literature. No matter how interesting and necessary these studies are, they are not sufficient if we want to follow scientists and engineers at work; after all, they do not draft, read and write papers twenty-four hours a day. Scientists and engineers invariably argue that there is something behind the technical texts which is much more important than anything they write.

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

Part A
From texts to things: a showdown

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

(1) Inscriptions

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

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

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

(2)

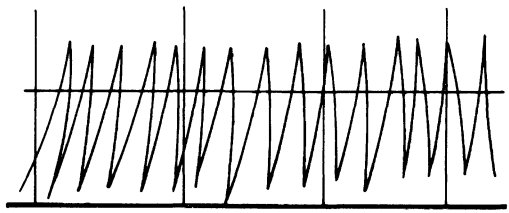


Figure 2.1

'OK. This is the base line; now, I am going to inject endorphin, what is going to happen? See?!' (Figure 2.2)

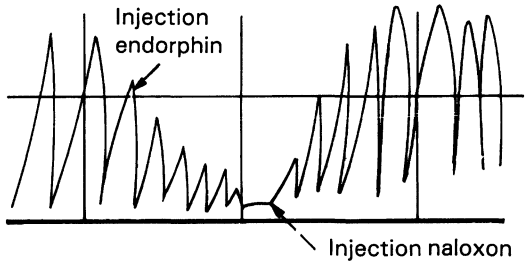


Figure 2.2

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

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

For a time we focus on the stylus pulsating regularly, inking the paper, scribbling cryptic notes. We remain fascinated by this fragile film that is in between text and laboratory. Soon, the Professor draws our attention beneath and beyond the traces on the paper, to the physiograph from which the image is slowly being emitted. Beyond the stylus a massive piece of electronic hardware records, calibrates, amplifies and regulates signals coming from another instrument, an array of glassware. The Professor points to a glass chamber in which bubbles are regularly flowing around a tiny piece of something that looks like elastic. It is indeed elastic, the Professor intones. It is a piece of gut, guinea pig gut ('myenteric plexus-longitudinal muscle of the guinea pig ileum', are his words). This gut has the property of contracting regularly if maintained alive. This regular pulsation is easily disturbed by many chemicals. If one hooks the gut up so that each contraction sends out an electric pulse, and if the pulse is made to move a stylus over graph paper, then the guinea pig gut will be induced to produce regular scribbles over a long period. If you then add a chemical to the chamber you *see* the peaks drawn by the inked stylus slow down or accelerate at the other end. This perturbation, invisible in the chamber, is visible on paper: the

chemical, no matter what it is, is given a *shape* on paper. This shape 'tells you something' about the chemical. With this set-up you may now ask new questions: if I double the dose of chemical will the peaks be doubly decreased? And if I triple it, what will happen? I can now measure the white surface left by the decreasing scribbles directly on the graph paper, thereby defining a quantitative relation between the dose and the response. What if, just after the first chemical is added, I add another one which is known to counteract it? Will the peaks go back to normal? How fast will they do so? What will be the pattern of this return to the base line level? If two chemicals, one known, the other unknown, trace the same slope on the paper, may I say, in this respect at least, that they are the same chemicals? These are some of the questions the Professor is tackling with endorphin (unknown), morphine (well known) and naloxone (known to be an antagonist of morphine).

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

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

about the similarity of endorphin to morphine; it was not mobilisable into a text and would not help to convince us. Only a part of its gut, tied up in the glass chamber and hooked up to a physiograph, can be mobilised in the text and add to our conviction. Thus, the Professor's art of convincing his readers must extend beyond the paper to preparing the ileum, to calibrating the peaks, to tuning the physiograph.

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

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

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

This move through the looking glass of the paper allows me to define an **instrument**, a definition which will give us our bearings when entering any

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

It is important to note that the use of this definition of instrument is a relative one. It depends on time. Thermometers *were* instruments and very important ones in the eighteenth century, so were Geiger counters between the First and Second World Wars. These devices provided crucial resources in papers of the time. But now they are only parts of larger set-ups and are only used so that a new visual proof can be displayed at the end. Since the definition is relative to the use made of the 'window' in a technical paper, it is also relative to the intensity and nature of the associated controversy. For instance, in the guinea pig ileum assay there is a box of electronic hardware with many readings that I will call 'intermediate' because they do not constitute the visual display eventually put to use in the article. It is unlikely that anyone will quibble about this because the calibration of electronic signals is now made through a black box produced industrially and sold by the thousand. It is a different matter with the huge tank built in an old gold mine in South Dakota at a cost of $600,000 (1964 dollars!) by Raymond Davis[2] to detect solar neutrinos. In a sense the whole set-up may be considered as *one* instrument providing one final window in which astrophysicists can read the number of neutrinos emitted by the sun. In this case all the other readings are intermediate ones. If the controversy is fiercer, however, the set-up is broken down into *several* instruments, each providing a specific visual display which has to be independently evaluated. If the controversy heats up a bit we do not see neutrinos coming out of the sun. We see and hear a Geiger counter that clicks when $Argon^{37}$ decays. In this case the Geiger counter, which gave only an intermediate reading when there was no dispute, becomes an instrument in its own right when the dispute is raging.

The definition I use has another advantage. It does not make presuppositions about what the instrument is made of. It can be a piece of hardware like a telescope, but it can also be made of softer material. A statistical institution that

employs hundreds of pollsters, sociologists and computer scientists gather all sorts of data on the economy *is* an instrument if it yields inscriptions for papers written in economic journals with, for instance, a graph of the inflation rate by month and by branch of industry. No matter how many people were made to participate in the construction of the image, no matter how long it took, no matter how much it cost, the whole institution is used as *one* instrument (as long as there is no controversy that calls its intermediate readings into question).

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

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

There is a corollary to this change of relevance on the inscription devices depending on the strength of the controversy, a corollary that will become more important in the next chapter. If you consider only fully-fledged facts it seems that everyone could accept or contest them equally. It does not cost anything to contradict or accept them. If you dispute further and reach the frontier where facts are made, instruments become visible and with them the cost of continuing the discussion rises. It appears that *arguing is costly*. The equal world of citizens

having opinions about things becomes an unequal world in which dissent or consent is not possible without a huge accumulation of resources which permits the collection of relevant inscriptions. What makes the differences between author and reader is not only the ability to utilise all the rhetorical resources studied in the last chapter, but also to gather the many devices, people and animals necessary to produce a visual display usable in a text.

(2) Spokesmen and women

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

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

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

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

(3)

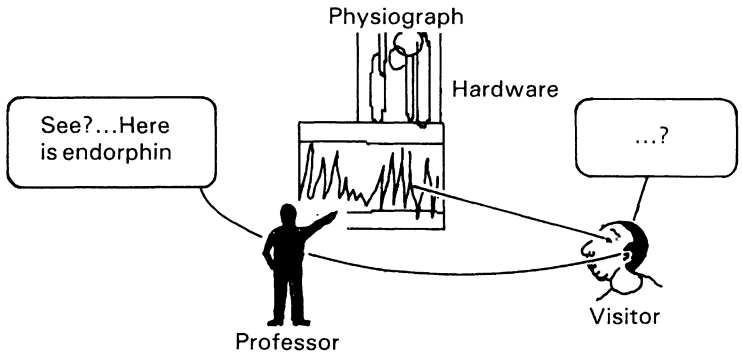


Figure 2.3

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

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

The spokesperson is someone who speaks for others who, or which, do not speak. For instance a shop steward is a spokesman. If the workers were gathered

together and they all spoke at the same time there would be a jarring cacophony. No more meaning could be retrieved from the tumult than if they had remained silent. This is why they designate (or are given) a delegate who speaks on their behalf, and in their name. The delegate – let us call him Bill – does not speak in *his* name and when confronted with the manager does not speak 'as Bill' but as the 'workers' voice'. So Bill's longing for a new Japanese car or his note to get a pizza for his old mother on his way home, are not the right topics for the meeting. The voice of the floor, articulated by Bill, wants a '3 per cent pay rise—and they are deadly serious about it, sir, they are ready to strike for it,' he tells the manager. The manager has his doubts: 'Is this really what they want? Are they really so adamant?' 'If you do not believe me,' replies Bill, 'I'll show you, but don't ask for a quick settlement. I told you they are ready to strike and you will see more than you want!' What does the manager see? He does not see what Bill said. Through the office window he simply sees an assembled crowd gathered in the aisles. Maybe it is because of Bill's interpretation that he reads anger and determination on their faces.

For everything that follows, it is very important not to limit this notion of spokesperson and not to impose any clear distinction between 'things' and 'people' in advance. Bill, for instance, represents people who could talk, but who, in fact, cannot all talk at once. Davis represents neutrinos that cannot talk, in principle, but which are made to write, scribble and sign thanks to the device set up by Davis. So in practice, there is not much difference between people and things: they both need someone to talk for them. From the spokesperson's point of view there is thus no distinction to be made between representing people and representing things. In each case the spokesperson literally does the talking for who or what cannot talk. The Professor in the laboratory speaks for endorphin like Davis for the neutrinos and Bill for the shopfloor. In our definition the crucial element is not the quality of the represented but only their number and the unity of the representative. The point is that confronting a spokesperson is not like confronting any average man or woman. You are confronted not with Bill or the Professor, but with Bill and the Professor *plus* the many things or people on behalf of whom they are talking. You do not address Mr Anybody or Mr Nobody but Mr or Messrs Manybodies. As we saw in the chapter on literature, it may be easy to doubt one person's word. Doubting a spokesperson's word requires a much more strenuous effort however because it is now one person – the dissenter – against a crowd – the author.

On the other hand, the strength of a spokesperson is not so great since he or she is by definition *one* man or woman whose word could be dismissed – one Bill, one Professor, one Davis. The strength comes from the representatives' word when they do not talk by and for themselves but *in the presence of* what they represent. Then, and only then, the dissenter is confronted simultaneously with the spokespersons and what they speak for: the Professor and the endorphin made visible in the guinea pig assay; Bill and the assembled workers; Davis and his solar neutrinos. The solidity of what the representative says is directly supported

by the silent but eloquent presence of the represented. The result of such a set-up is that it seems as though the mouthpiece does not 'really talk', but that he or she is just commenting on what you yourself directly see, 'simply' providing you with the words you would have used anyway.

This situation, however, is the source of a major weakness. Who is speaking? The things or the people *through* the representative's voice? What does she (or he, or they, or it) say? Only what the things they represent would say if they could talk directly. But the point is that they cannot. So what the dissenter sees is, in practice, rather different from what the speaker says. Bill, for instance, says his workers want to strike, but this might be Bill's own desire or a union decision relayed by him. The manager looking through the window may see a crowd of assembled workers who are just passing the time and can be dispersed at the smallest threat. At any rate do they really want 3 per cent and not 4 per cent or 2 per cent? And even so, is it not possible to offer Bill this Japanese car he so dearly wants? Is the 'voice of the worker' not going to change his/its mind if the manager offers a new car to Bill? Take endorphin as another instance. What we really saw was a tiny depression in the regular spikes forming the base line. Is this the same as the one triggered by morphine? Yes it is, but what does that prove? It may be that all sorts of chemicals give the same shape in this peculiar assay. Or maybe the Professor so dearly wishes his substance to be morphine-like that he unwittingly confused two syringes and injected the same morphine twice, thus producing two shapes that indeed look identical.

What is happening? The controversy flares even after the spokesperson has spoken and displayed to the dissenter what he or she was talking about. How can the debate be stopped from proliferating again in all directions? How can all the strength that a spokesman musters be retrieved? The answer is easy: by letting the things and persons represented *say for themselves the same thing that the representatives claimed they wanted to say.* Of course, this never happens since they are designated because, by definition, such direct communication is impossible. Such a situation however may be convincingly staged.

Bill is not believed by the manager, so he leaves the office, climbs onto a podium, seizes a loudspeaker and asks the crowd, 'Do you want the 3 per cent rise?' A roaring 'Yes, our 3 per cent! Our 3 per cent!' deafens the manager's ears even through the window pane of his office. 'Hear them?' asks Bill with a modest but triumphant tone when they are sitting down again at the negotiating table. Since the workers themselves said exactly what the 'workers' voice' had said, the manager cannot dissociate Bill from those he represents and is really confronted with a crowd acting as one single man.

The same is true for the endorphin assay when the dissenter, losing his temper, accuses the Professor of fabricating facts. 'Do it yourself,' the Professor says, irritated but eager to play fair. 'Take the syringe and see for yourself what the assay reaction will be .' The visitor accepts the challenge, carefully checks the labels on the two vials and first injects morphine into the tiny glass chamber. Sure enough, a few seconds later the spikes start decreasing and after a minute or so

they return to the base line. With the vial labelled endorphin, the very same result is achieved with the same timing. A unanimous, incontrovertible answer is thus obtained by the dissenter himself. What the Professor said the endorphin assay will answer, if asked directly, is answered by the assay. The Professor cannot be dissociated from his claims. So the visitor has to go back to the 'negotiating table' confronted not with the Professor's own wishes but with a Professor simply transmitting what endorphin really is.

No matter how many resources the scientific paper might mobilise, they carry little weight compared with this rare demonstration of power: the author of the claim steps aside and the doubter sees, hears and touches the inscribed things or the assembled people that reveal to him or to her exactly the same claim as the author.

(3) Trials of strength

For us who are simply following scientists at work there is no exit from such a set-up, no back door through which to escape the incontrovertible evidence. We have already exhausted all sources of dissent; indeed we might have no energy left to maintain the mere idea that controversy might still be open. For us laymen, the file is now closed. Surely, the dissenter we have shadowed since the beginning of Chapter 1 will give up. If the things say the same as the scientist, who can deny the claim any longer? How can you go any further?

The dissenter goes on, however, with more tenacity than the laymen. The identical tenor of the representative's words and the answers provided by the represented were the result of a carefully staged situation. The instruments needed to be working and finely tuned, the questions to be asked at the right time and in the right format. What would happen, asks the dissenter, if we stayed longer than the show and went backstage; or were to alter any of the many elements which, everyone agrees, are necessary to make up the whole instrument? The unanimity between represented and constituency is like what an inspector sees of a hospital or of a prison camp when his inspection is announced in advance. What if he steps outside his itinerary and tests the solid ties that link the represented and their spokesmen?

The manager, for instance, heard the roaring applause that Bill received, but he later obtains the foremen's opinion: 'The men are not for the strike at all, they would settle for 2 per cent. It is a union order; they applauded Bill because that's the way to behave on the shopfloor, but distribute a few pay rises and lay off a few ringleaders and they will sing an altogether different song.' In place of the unanimous answer given by the assembled workers, the manager is now faced with an *aggregate* of possible answers. He is now aware that the answer he got earlier through Bill was extracted from a complex setting which was at first invisible. He also realises that there is room for action and that each worker may be made to behave differently if pressures other than Bill's are exerted on them.

The next time Bill screams 'You want the 3 per cent don't you?' only a few half-hearted calls of agreement will interrupt a deafening silence.

Let us take another example, this time from the history of science. At the turn of the century, Blondlot, a physicist from Nancy, in France, made a major discovery like that of X-rays.[3] Out of devotion to his city he called them 'N-rays'. For a few years, N-rays had all sorts of theoretical developments and many practical applications, curing diseases and putting Nancy on the map of international science. A dissenter from the United States, Robert W. Wood, did not believe Blondlot's papers even though they were published in reputable journals, and decided to visit the laboratory. For a time Wood was confronted with incontrovertible evidence in the laboratory at Nancy. Blondlot stepped aside and let the N-rays inscribe themselves straight onto a screen in front of Wood. This, however, was not enough to get rid of Wood who obstinately stayed in the lab asking for more experiments and himself manipulating the N-ray detector. At one point he even surreptitiously removed the aluminium prism which was generating the N-rays. To his surprise, Blondlot on the other side of the dimly lit room kept obtaining the same result on his screen even though what was deemed the most crucial element had been removed. The direct signatures made by the N-rays on the screen were thus made by something else. The unanimous support became a cacophony of dissent. By removing the prism, Wood severed the solid links that attached Blondlot to the N-rays. Wood's interpretation was that Blondlot so much wished to discover rays (at a time when almost every lab in Europe was christening new rays) that he unwittingly made up not only the N-rays, but also the instrument to inscribe them. Like the manager above, Wood realised that the coherent whole he was presented with was an aggregate of many elements that could be induced to go in many different directions. After Wood's action (and that of other dissenters) no one 'saw' N-rays any more but only smudges on photographic plates when Blondlot presented his N-rays. Instead of enquiring about the place of N-rays in physics, people started enquiring about the role of auto-suggestion in experimentation! The new fact had been turned into an artefact. Instead of going down the ladder of Figure 1.9, it went up the ladder and vanished from view.

The way out, for the dissenter, is not only to dissociate and disaggregate the many supporters the technical papers were able to muster. It is also to shake up the complicated set-up that provides graphs and traces in the author's laboratory in order to see how resistant the array is which has been mobilised in order to convince everyone. The work of disbelieving the literature has now been turned into the difficult job of manipulating the hardware. We have now reached another stage in the escalation between the author of a claim and the disbeliever, one that leads them further and further into the details of what makes up the inscriptions used in technical literature.

Let us continue the question-and-answer session staged above between the Professor and the dissenter. The visitor was asked to inject morphine and endorphin himself in order to check that there was no foul play. But the visitor is

now more devious and does not make any effort to be polite. He wants to check where the vial labelled endorphin comes from. The Professor, unruffled, shows him the protocol book with the same code number as on the vial, a code that corresponds to a purified sample of brain extract. But this is a text, another piece of literature, simply an account book that could have been either falsified or accidentally mislabelled.

By now, we have to imagine a dissenter boorish enough to behave like a police inspector suspecting everyone and believing no one and finally wanting to see the real endorphin with his own eyes. He then asks, 'Where do I go from this label in the book to where the contents of the vial comes from?' Exasperated, the author leads him towards another part of the laboratory and into a small room occupied by glass columns of various sizes, filled with a white substance, through which a liquid is slowly percolating. Underneath the columns, a small piece of apparatus moves a rack of tiny flasks in which the percolated liquid is collected every few minutes. The continous flow at the top of the columns is collected, at the bottom, into a discrete set of flasks, each of which contains the part of the liquid that took the same given amount of time to travel through the column.

(4)–Here it is, says the guide, here is your endorphin.

–Are you kidding, replies the dissenter, where is endorphin? I don't see a thing?

–Hypothalamic brain extract is deposited on the top of the Sephadex column. It is a soup. Depending on what we fill it with, the column disassociates the mixture, sieves it; it may be done by gravity, or electrical charge, anything. At the end you get racks that collect samples which have behaved similarly in the column. This is called a fraction collector. Each fraction is then checked for purity. *Your* vial of endorphin came from *this* rack two days ago, no. 23/16/456.

–And this is what you call pure? How do I know it is pure? Maybe there are hundreds of brain extracts that travel through the column at the same pace exactly and end up in the same fraction.

The pressure is mounting. Everyone in the lab is expecting an outburst of rage, but the Professor politely leads the visitor towards another part of the laboratory.

(5)–Here is our new High Pressure Liquid Chromatograph (HPLC). See these tiny columns? They are like the ones you just saw, but each fraction collected there is submitted to an enormous pressure here. The column delays the passage and at this pressure it strongly differentiates the molecules. The ones that arrive at the same time at the end are *the same* molecules, the same, my dear colleague. Each fraction is read through an optical device that measures its optical spectrum. Here is the chart that you get . . .. See? Now, when you get a single peak it means the material is pure, so pure that a substance with only one different amino-acid in a hundred will give you *another* peak. Is not that quite convincing?

–(silence from the dissenter)

–Oh, I know! Maybe you are uncertain that I did the experiment with *your* vial of endorphin? Look here in the HPLC book. Same code, same time. Maybe you claim that I asked this gentleman here to fake the books, and obtain this peak for me with another substance? Or maybe you doubt the measurement of optical spectra. Maybe

you think it is an obsolete piece of physics. No such luck, my dear colleague, Newton described this phenomenon quite accurately – but maybe he's not good enough for you.

The Professor's voice is quivering with hardly suppressed rage but he still behaves. Of course the dissenter could start doubting the HPLC or the fraction collector as he did with the guinea pig ileum assay, converting them from black boxes into a field of contention. He *could* in principle, but he *cannot* in practice since time is running out and he is sensitive to the exasperation in everyone's voice. And who is he anyway to mount a dispute against Water Associates, the company who devised this HPLC prototype? Is he ready to cast doubt on a result that has been accepted unquestioningly for the past 300 years, one that has been embedded in thousands of contemporary instruments? What he wants is to see endorphin. The rest, he must face it, cannot be disputed. He has to compromise and to admit that the Sephadex column, and the HPLC, are indisputable. In a conciliatory tone he says:

(6)–This is very impressive; however I must confess a slight disappointment. What I see here is a peak which, I admit, means that the brain extract is now pure. But how do I know that this pure substance is endorphin?

With a sigh, the visitor is led back to the assay room where the little guinea pig gut is still regularly contracting.

(7)–Each of the fractions deemed pure by the HPLC is tried out here, in this assay. Of all the pure fractions only two display any activity, I repeat only two. When the whole process is repeated in order to get purer material, this activity dramatically increases. The shape may be exactly superimposed onto that of commercially available morphine. Is that insignificant? We did it thirty-two times! Is that nothing? Each modification of the spikes has been tested for statistical significance. Only endorphin and morphine have any significant effect. Does all of that count for nothing? If you are so clever, can you give me an alternative explanation why morphine and this pure substance X would behave identically? Can you even imagine another explanation?

–No, I must admit, whispers the believer, I am very impressed. This really looks like genuine endorphin. Thank you so much for the visit. Don't trouble yourselves, I will find my own way out . . .. (exit the dissenter)

This exit is not the same as that of the semiotic character of Chapter 1, p.53. This time it is for good. The dissenter tried to disassociate the Professor from his endorphin, and he failed. Why did he fail? Because the endorphin constructed in the Professor's lab *resisted* all his efforts at modification. Every time the visitor followed a lead he reached a point where he had either to quit or start a new controversy about a still older and more generally accepted fact. The Professor's claim was tied to the brain, to the HPLC, to the guinea pig ileum assay. There is something in his claim that is connected to classic claims in physiology, pharmacology, peptide chemistry, optics, etc. This means that when the doubter tries out the connections, all these other facts, sciences and black boxes come to

the Professor's rescue. The dissenter, if he doubts endorphin, has also to doubt Sephadex columns, HPLC technics, gut physiology, the Professor's honesty, that of his whole lab, etc. Although 'enough is never enough' – see the introduction – there is a point where no matter how pig-headed the dissenter could be, enough is enough. The dissenter would need so much more time, so many more allies and resources to continue to dissent that he has to quit, accepting the Professor's claim as an established fact.

Wood, who did not believe in N-rays, also tried to shake the connection between Blondlot and his rays. Unlike the former dissenter he succeeded. To dislocate the black boxes assembled by Blondlot, Wood did not have to confront the whole of physics, only the whole of one laboratory. The manager who suspected the workers' determination tried out the connections between them and their union boss. These connections did not resist a few classic clever tricks for long. In the three cases the dissenters imposed a showdown running from the claim to what supports the claim. When imposing such a **trial of strength** they are faced with spokespersons and what (or whom) these persons speak for. In some cases the dissenters isolate the representative from his or her 'constituency', so to speak; in other cases such a separation is impossible to obtain. It cannot be obtained without a trial of strength, any more than a boxer can claim to be a world champion without convincingly defeating the previous world champion. When the dissenter succeeds, the spokesperson is transformed from someone who speaks for others into someone who speaks for him or herself, who represents only him or herself, his or her wishes and fancies. When the dissenter fails, the spokesperson is seen not really as an individual but as the mouthpiece of many other mute phenomena. Depending on the trials of strength, spokespersons are turned into **subjective** individuals or into **objective** representatives. Being objective means that no matter how great the efforts of the disbelievers to sever the links between you and what you speak for, the links resist. Being subjective means that when you talk *in the name* of people or things, the listeners understand that you represent only yourself. From Mr Manybodies you are back to being Mr. Anybody.

It is crucial to grasp that these two adjectives ('objective', 'subjective') are *relative* to trials of strength in specific settings. They cannot be used to qualify a spokesperson or the things he or she is talking about once and for all. As we saw in Chapter 1, each dissenter tries to transform a statement from objective to subjective status, to transform, for instance, an interest in N-rays inside physics into an interest in self-suggestion in provincial laboratories. In the endorphin example, the dissenter seemed to be trying very hard to convert the Professor's claim into a subjective flight of fancy. In the end it was the lonely dissenter who saw his naive questioning turned into a trivial flight of fancy, if not an obsessive drive to seek fraud and find fault everywhere. In the trial of strength the Professor's endorphin was made *more objective* – going down the ladder – and the dissenter's counter-claim was made *more subjective* – pushed up the ladder. 'Objectivity' and 'subjectivity' are relative to trials of strength and they can shift

gradually, moving from one to the other, much like the balance of power between two armies. A dissenter accused by the author of being subjective must now wage another struggle if he or she wishes to go on dissenting without being isolated, ridiculed and abandoned.

Part B
Building up counter-laboratories

Let me summarise our trip from the discussion at the beginning of Chapter 1 up to this point. What is behind the claims? Texts. And behind the texts? More texts, becoming more and more technical because they bring in more and more papers. Behind these articles? Graphs, inscriptions, labels, tables, maps, arrayed in tiers. Behind these inscriptions? Instruments, whatever their shape, age and cost that end up scribbling, registering and jotting down various traces. Behind the instruments? Mouthpieces of all sorts and manners commenting on the graphs and 'simply' saying what they mean. Behind them? Arrays of instruments. Behind those? Trials of strength to evaluate the resistance of the ties that link the representatives to what they speak for. It is not only words that are now lined up to confront the dissenter, not only graphs to support the words and references to support the whole assembly of allies, not only instruments to generate endless numbers of newer and clearer inscriptions, but, behind the instruments, new objects are lined up which are defined by their resistance to trials. Dissenters have now done all they can do to disbelieve, disaggregate and disassociate what is mustered behind the claim. They have come a long way since barging into the first discussion at the beginning of Chapter 1. They became readers of technical literature, then visitors to the few laboratories from which the papers were coming, then impolite inspectors manipulating the instruments to check how faithful they were to the author.

At this point they have to take another step – either give up, or find other resources to overcome the author's claim. In the second part of this book we will see that there exist many ways to reject the laboratory results (Chapter 4); but for this chapter we will concentrate on the rarest outcome, when, all else being equal, there is no other way open to the dissenters than to *building another laboratory.* The price of dissent increases dramatically and the number of people able to continue decreases accordingly. This price is entirely determined by the authors whose claims one wishes to dispute. The dissenters cannot do less than the authors. They have to gather more forces in order to untie what attaches the spokesmen and their claims. This is why all laboratorries are *counter-laboratories* just as all technical articles are counter-articles. So the dissenters do not simply have to get a laboratorry; they have to get a *better* laboratory. This makes the price still higher and the conditions to be met still more unusual.

(1) Borrowing more black boxes

How is it possible to obtain a better laboratory, that is a laboratory producing less disputable claims and allowing the dissenter – now head of a lab – to disagree and be believed? Remember what happened to the visitor to the Professor's laboratory. Every time a new flaw appeared which the disbeliever tried to exploit, the Professor presented him with a new and seemingly incontrovertible black box: a Sephadex column, an HPLC machine, basic physics, or classic physiology, etc. It might have been possible to dispute each of these, but it was not practical because the same energy would have been needed to reopen each of these black boxes. Indeed, *more* energy would have been applied because each of these facts in turn would have led to more tightly sealed black boxes: the microprocessors treating the data from the HPLC, the fabrication of the gel in the columns, the raising of guinea pigs in the animal quarters, the production of morphine at an Ely-Lily factory, etc. Each fact could be made the departure point of a new controversy that would have led to many more accepted facts, and so on *ad infinitum*.

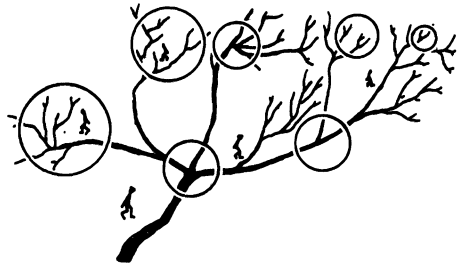


Figure 2.4

The dissenter was thus confronted by an exponential curve, a slope similar to the one drawn in Figure 1.8. Now that he has become the head of a brand new laboratory, one of the ways to make it a better counter-lab is to discover ways either of levelling the slope or of confronting his opponents with an even steeper one.

For instance Schally, in order to back up his ill-fated GHRH, see Chapter 1, statement (5) – used a bioassay called the rat tibia cartilage assay. Guillemin, who disagreed with GHRH, started to try out the tibia assay in exactly the way our dissenter tried out his guinea pig ileum assay.[4] In the face of this challenge, Schally's tibia assay was made to say quite different things by Guillemin. The growth of tibia cartilage in the rat might be caused by a growth hormone substance but might just *as well* have been caused by a variety of other chemicals, or

indeed not have occurred at all. In several harsh papers, Guillemin said the 'results were so erratic that Schally's claims should be taken with the most extreme precaution'. Thus Schally was cut off from his supply line. He claimed the existence of GHRH, but nothing followed. Isolated, his claim was made more subjective by the dissenter's action.

Why should anyone believe Guillemin's counter-claim rather than Schally's claim? One obvious way to strengthen this belief is to modify the bioassay to make it impossible for anyone to make it say different things from Guillemin. Guillemin discarded the rat tibia assay and shifted to a rat pituitary cell culture. Instead of seeing the growth of cartilage with the naked eye, what is 'seen' is the amount of hormone released by the few pituitary cells maintained in a culture; this amount is measured by an instrument – in the sense I gave this term earlier – called radio-immunoassay. The new assay is much *more* complicated than Schally's older ones – in itself the radio-immunoassay requires several technicians and takes up to a week to complete – but it gives inscriptions at the end that may be said to be more clear-cut, that is they literally cut shapes out of the background. In other words, even without understanding a word of the issue, the perceptive judgment to be made on one is easier than on the other.

The answers are less equivocal than the 'erratic' ones given by the tibia assay – that is, they leave less room for the dissenter to quibble – and the whole instrument is *less* easily disputable. Although it is complicated, the cell culture assay can be taken as *a single black box* which provides a single window from which to read the amount of GHRH. Naturally, it can be disputed in principle. It is just that it's harder to do so in practice. A physiologist with a little training may nitpick at the cartilage assay, may quibble about the length from growth in the tibia. He or she needs much more than a little training to dispute Guillemin's new figures. The assay is now tied to basic advances in molecular biology, immunology and the physics of radioactivity. Nitpicking at the inscriptions is possible but less reasonable, the heckler needing more resources and becoming more isolated. The gain in conviction is clear: from Schally's first words a fierce dispute ensues about the assay which is supposed to reveal the very existence of GHRH. In Guillemin's counter-paper this part of the discussion *at least* has been sealed off since his detection system is made indisputable, and the range of possible disputes has *shifted to other* aspects of the same claims.

Another example is provided by the controversy about the detection of gravitational waves.[5] One physicist, Weber, built a massive antenna made of a large aluminium alloy bar weighing several tons that vibrated at a certain frequency. To detect a gravitational wave the antenna must be insulated from all other influences – ideally it should be in a vacuum, free from seismic vibrations and radio interference, at a temperature at or near absolute zero, etc. Taken as an instrument, the whole set-up provides a window which allows one to read the presence of gravitational waves. The problem is that the peaks above the noise threshold are so tiny that any passing physicist could dispute Weber's claim. Indeed, any passing physicist could set the instrument off! Weber argues that

they represent gravitation but every dissenter may claim that they represent many other things *as well.* This little expression 'as well' is what kills most solid claims. As long as it is possible to say 'as well', there is no established line from the gravitation waves to Weber via the antenna. The figure offered by Weber may represent either 'gravitational waves' or meaningless scribbles registering terrestrial noise. To be sure, there are many ways out of the controversy so as to shrug off Weber's claim as a mere opinion. But the way out of the controversy that interests us here is to build *another* antenna, one, for instance, that is a thousand million times more sensitive than Weber's so that this part of the detection at least is not disputed. The aim of this new antenna is to confront the sceptic with an incontrovertible black box *earlier* in the process. After this, sceptics may still discuss the amount of gravitation, and what it does to the relativity theory or to astrophysics, but they will not argue that there are peaks that cannot be explained by terrestrial interferences. With the first antenna alone, Weber might be the freak and the dissenters the sensible professionals. With the new antenna, those who deny the presence of the peaks are the isolated sceptics and it is Weber who is the sensible professional. All other things being equal the balance of power would have been tipped. (In this case, however, it did not make the slightest difference because many other avenues for dissent were opened.)

Borrowing more black boxes and situating them earlier in the process is the first obvious strategy for building a better counter-laboratory. The discussion is diffracted and shunted away. Any one laboratory gets an edge on all the others if it finds a way to delay the possible discussions until later. In the early days of microbe cultures, for example, the microbes were grown in a liquid like urine. They were visible in the flasks but you needed keen and trained eyesight to detect them. Dissent could ensue because the construction of the fact was interrupted from the start by a preliminary discussion on whether or not microbes were present in the flask. When Koch invented the solid milieu culture, acute eyesight was no longer needed to see the little microbes: they made nice little coloured patches which contrasted clearly with the white background. The visibility was dramatically enhanced when specific dyes coloured certain microbes or their parts. The laboratory endowed with these techniques made dissent more difficult: a slope was deepened, a trench was dug. Although many other aspects were still open to dispute, the presence of the microbes was made indisputable.

At this point, it is easy to imagine the growing differences between good and bad (counter-) laboratories. Imagine a lab that starts making claims based on the cartilage tibia assay, Weber's first antenna and the liquid microbe culture. If the head of this laboratory wanted to be believed he would have an endless task. Every time he opened his mouth, any number of his dear colleagues would start shaking their heads, and suggesting many alternatives just as plausible as the first. To do so, they would only need a bit of imagination. Like Achilles in Zeno's paradox, the challenger will never reach the end of his argument since each point will be the start of an indefinite regression. In contrast, claims produced by the good laboratory cannot be opposed simply with a bit of imagination. The cost of

disputing the claims increases proportionally with the number of black boxes assembled by the author. Faced with the pituitary culture assay, the new antenna which is one thousand million times more sensitive and the solid milieu culture, the dissenters are forced to assent or, at least, *to redirect* their dissent toward some other aspect of the claims. They can still mount a controversy but the magnitude of the mobilisation needed to do so has increased. They need an even better equipped laboratory with more and more black boxes, thus delaying the dispute still further. The vicious (or virtuous) circle of lab construction is now launched and there is no way to stop it – apart from giving up the production of credible arguments altogether, or recruiting more powerful allies elsewhere.

(2) Making actors betray their representatives

The competition between scientists – whom I will treat in this section as alternately authors and dissenters – to turn one another's claims into subjective opinion leads to expensive laboratories equipped with more and more black boxes introduced as early as possible into the discussion. This game, however, would soon stop if only existing black boxes were mobilised. After a time dissenters and authors – all things remaining equal – would have access to the *same* equipment, would tie their claims to the same harder, colder and older facts and none would be able to get an edge on the other: their claims would be thus left in limbo, in intermediary stages between fact and artefact, objectivity and subjectivity. The only way to break this stalemate is to find either new and unexpected resources (see the next section) or, more simply, to force the opponent's allies to *change camp.*

This would happen, for instance, if the manager of our little vignette above could organise a secret ballot to decide about the continuation of the strike. Remember that Bill, the shop steward, claimed that 'all the workers want a 3 per cent pay rise'. This claim was confirmed at meetings during which the represented said the same things as their mouthpiece. Even if the manager suspects that the workers are not so unanimous, each public meeting loudly confirms Bill's claim. However, in organising a secret ballot, the manager tests the same actors in a different way, by exerting a new set of pressures on them: isolation, secrecy, recounting of the ballots, surveillance. Submitted to these new trials, only 9 per cent of the same workers voted for the continuation of the strike, and 80 per cent were ready to settle for 2 per cent. The represented have changed camp. They now say what the manager said they would say. They have a new spokesperson. This, naturally, does not stop the controversy, but the dispute will now bear on the election process itself. Bill and his union accuse the manager of intimidation, unfair pressure, of having stuffed the ballot boxes and so on. This shows that even the most faithful supporters of a spokesman may be made to *betray.*

As I showed above, both people able to talk and things unable to talk have

spokesmen (Part A, section 2). I propose to call whoever and whatever is represented **actant.** What the manager did to Bill, a dissenter may do for the ally of his opponent's laboratory. Pouchet, engaged in a bitter struggle against Louis Pasteur's claim that there is no spontaneous generation, built a nice counter-experiment.[6] Pasteur argued that it is always germs introduced from the outside that generate micro-organisms. Long swan-necked open glass flasks containing sterilised infusion were contaminated at low altitude but stayed sterile in the High Alps. This impressive series of demonstrations established an incontrovertible link between a new actor, the micro-organisms, and what Pasteur said they could do: microbes could not come from *within* the infusion but only from *outside.* Pouchet, who rejected Pasteur's conclusion, tried out the connection and forced the micro-organisms to emerge from within. Repeating Pasteur's experiment Pouchet showed that glass flasks containing a sterile hay infusion were very soon swarming with micro-organisms even in the 'germ-free' air of the Pyrenees Mountains. The micro-organisms on which Pasteur depended were made to betray him: they appeared spontaneously thus supporting Pouchet's position. In this case, the actants change camps and two spokesmen are supported at once. This change of camp does not stop the controversy, because it is possible to accuse Pouchet of having unknowingly introduced micro-organisms from outside even though he sterilised everything. The meaning of 'sterile' becomes ambiguous and has to be renegotiated. Pasteur, now in the role of dissenter, showed that the mercury used by Pouchet was contaminated. As a result Pouchet was cut off from his supply lines, betrayed by his spontaneous micro-organisms, and Pasteur becomes the triumphant spokesman, aligning 'his' micro-organisms which act on command. Pouchet failed in his dissent and ended up isolated, his 'spontaneous generation' reduced by Pasteur to a *subjective* idea, to be explained not by the behaviour of microbes but by the influence of 'ideology' and 'religion'.[7]

The same luring of allies away from their spokesperson occurred among the Samoans. As mobilised in the 1930s by Margaret Mead to act on North American ideals of education and sexual behaviour, Samoan girls were more liberated than Western ones and free from the crises of adolescence.[8] This well-established fact was attributed not to Mead – acting as the anthropologist mouthpiece of the Samoans – but to the Samoans. Recently another anthropologist, Derek Freeman, attacked Mead, severing all links between the Samoan girls and Margaret Mead. She was turned into an isolated liberal American lady without any serious contact with Samoa and writing a 'noble savage' fiction off the top of her head. Freeman, the new spokesman of the Samoans, said the girls there were sexually repressed, assaulted and often raped and that they went through a terrible adolescence. Naturally, this 'kidnapping', so to speak, of Samoan teenagers by a new representative does not bring the controversy to an end any more than in our other examples. The question is now to decide if Freeman is a boorish and insensitive male influenced by sociobiology, and if he has more Samoan allies on his side than Margaret Mead, a highly thought of female

anthropologist, sensitive to all the subtle cues of her Samoan informants. The point for us is that the most sudden reversal in the trials of strength between authors and dissenters may be obtained simply by cutting the links tying them to their supporters.

A subtler strategy than Freeman's to cut these links was employed by Karl Pearson in his dispute with George Yule's statistics.[9] Yule had devised a coefficient to measure the strength of an association between two *discrete* variables. This crude but robust coefficient allowed him to decide whether or not there was an association between, for instance, vaccination and the death rate. Yule was not interested in defining links more precisely. All he wanted to be able to determine was whether vaccination decreased the death rate. Pearson, on the other hand, objected to Yule's coefficient because when you wanted to decide *how close* the links were, it offered a wide range of possible solutions. With Yule's coefficient you would never know, in Pearson's opinion, if you had your data all safely arrayed behind your claims. Yule did not bother because he was treating only discrete entities. Pearson, however, had a much more ambitious project and wanted to be able to mobilise a large number of *continuous* variables such as height, colour of skin, intelligence . . . With Yule's coefficient he would have been able to define only weak associations between genetic variables. This meant that any dissenter could easily have severed him from his data and turned one of the most impressive arrays on genetic determinism ever compiled into a mixed and disorderly crowd of unclear relations. Pearson devised a correlation coefficient which made any discrete variable the outcome of a continuous distribution. Yule was left with only weak associations and Pearson, tying his data together with his 'tetrachoric coefficient of correlation', could transform any continuous variable into a strongly associated whole of discrete variables and so *solidly* attach intelligence to heredity. This of course did not mark the end of the controversy. Yule tried out the Pearson coefficient showing that it arbitrarily transformed continuous variables into discrete ones. If successful, Yule would have deprived Pearson of the support of his data. Although this controversy has been continuing for nearly a hundred years, the lesson for us is that, with the same equipment and data, the stalemate between dissenting authors may be broken by a simple modification of what it is that ties the data together (we shall see more of this phenomenon in Chapter 6).

In each of the examples above I showed how allies were enticed away from their representative in order to tip the balance, but I also indicated that this need not settle the debate. Often it modifies the field of contention enough to buy time – not enough to win. This strategy must in general be combined with that of section 1 in order to succeed – borrowing more black boxes and positioning them earlier in the process – and with that of the third section, which is the most daring and the most difficult to grasp for the visiting layperson.

(3) Shaping up new allies

The dissenter, now the head of a (counter-) laboratory, has imported as many black-boxed instruments as possible and has tried to entice his opponent's supporters away. Even combining these two strategies he or she will not fare very well since all scientists are playing with a *limited set* of instruments and actants. After a few moves the controversy will reach a new stalemate with the supporters continually changing camp: for and against the manager, for and against Pasteur, for and against Margaret Mead, for and against Pearson, with no end in sight. No credible fact will be produced in such confusion since no third party will be able to borrow any statement as a black box to put it to use elsewhere. In order to break the stalemate, other allies which are *not yet* defined have to be brought in.

Let me go back to the example of GHRH discovered by Schally using his rat tibia cartilage assay. We saw how Guillemin, rejecting this 'discovery' – now in quotation marks – devised a new, less controvertible assay, the pituitary cell culture (Chapter 1, section 2). With it, he induced the GHRH supporting Schally's claim to shift alliances. Remember that when Schally thought he had found a new important hormone, Guillemin intervened and showed that this 'new important hormone' was a contaminant, a piece of haemoglobin. By following the two strategies we have just defined, Guillemin won but only *negatively*. Although he overcame his competitor, his own claims about GHRH – which he calls GRF – are not made more credible. For a third party the whole topic is simply a mess from which no credible fact emerges. In the search for the final *coup de grâce,* the dissenter needs something more, a supplement, a little 'je ne sais quoi' that, everything being equal, will ensure victory and convince the third party that the controversy has indeed been settled.

In the (counter-) laboratory the purified extracts of GRF are injected into the cell culture. The result is appalling: nothing happens. Worse than nothing, because the results are negative: instead of being triggered by GRF the growth hormone is decreased. Guillemin gives his collaborator, Paul Brazeau, who has done the experiment, a good dressing down.[10] The whole instrument, supposed to be a perfect black box, is called into doubt, and the whole career of Brazeau, supposed to be a skilled and honest worker, is jeopardised. The dissenter/author struggle has now shifted inside the laboratory and they are both trying out the assay, the purification scheme and the radio-immunoassay exactly as the visitor did above for endorphin (In Part A, section 3). At the third trial Brazeau still obtained the same result. That is, no matter how much effort he was making, the same negative results were produced. No matter how strongly Guillemin attacked him, he was led every time to the same sort of quandary with which I finished Part A: either to quit the game or to start discussing so many basic, old and accepted black boxes that the whole lab would have to be dismantled. Since the negative results resisted all trials of strength, since the cell culture assay was left indisputable, and since Brazeau's honesty and skill were withstanding the shock, some other weak point had to give way. The hormone they were looking for

released growth hormone; in their hands it *decreased* growth hormone. Since they could no longer doubt that their 'hands' were good, they had to doubt the first definition or quit the game altogether: they had got their hands on a hormone that *decreased* the production of growth hormone. They had, in other words, tried out a *new* hormone, a new, unexpected and still undefined ally to support another claim. Within a few months they had obtained a decisive advantage over Schally. Not only had he confused GHRH with a piece of haemoglobin, but he had sought the wrong substance all along.

We have reached a point which is one of the most delicate of this book, because, by following dissenting scientists, we have access to their most decisive arguments, to their ultimate source of strength. Behind the texts, they have mobilised inscriptions, and sometimes huge and costly instruments to obtain these inscriptions. But something else resists the trials of strength behind the instruments, something that I will call provisionally a **new object**. To understand what this is, we should stick more carefully than ever to our method of following only scientists' practice, deaf to every other opinion, to tradition, to philosophers, and even to what scientists say about what they do (see why in the last part of this chapter).

What is a new object in the hands of a scientist? Consider the GRF that Guillemin and Brazeau were expecting to find: it was defined by its effect on tibia cartilage assay and in cell cultures. The effect was uncertain in the first assay, certain and negative in the second. The definition had to change. The new object, at the time of its inception, is still undefined. More exactly, it is defined by what it does in the laboratory trials, *nothing more, nothing less*: its tendency to decrease the release of growth hormone in the pituitary cells culture. The etymology of 'definition' will help us here since defining something means providing it with limits or edges (*finis*), giving it a shape. GRF had a shape; this shape was formed by the answers it gave to a series of trials inscribed on the window of an instrument. When the answers changed and could not be ignored a new shape was provided, a new thing emerged, a something, still unnamed, that did exactly the opposite of GRF. Observe that in the laboratory, the new object is *named after what it does*: 'something that inhibits the release of growth hormone'. Guillemin then invents a new word that summarises the actions defining the thing. He calls it 'somatostatin' – that which blocks the body (implying body growth).

Now that somatostatin is named and accepted, its properties have changed and are not of interest to us at this point. What counts for us is to understand the new object just at the moment of its emergence. Inside the laboratory the new object is *a list of written answers to trials*. Everyone today talks for instance of 'enzymes' which are well-known objects. When the strange things later called 'enzymes' were emerging among competing laboratories, scientists spoke of them in very different terms:[11]

> (8) From the liquid produced by macerating malt, Payen and Persoz are learning to extract, through the action of alcohol, a solid, white, amorphous, neutral, more or

> less tasteless substance that is insoluble in alcohol, soluble in water and weak alcohol, and which cannot be precipitated by sub-lead acetate. Warmed from 65° to 75° with starch in the presence of water, it separates off a soluble substance, which is dextrin.

At the time of its emergence, you cannot do better than explain what the new object is by repeating the list of its constitutive actions: 'with A it does this, with C it does that.' It has *no other shape than this list.* The proof is that if you add an item to the list you *redefine the object*, that is, you give it a new shape. 'Somatostatin' for instance was defined by the now well-established fact that, coming from the hypothalamus, it inhibited the release of growth hormone. The discovery I summarised above was described in this way for a few months after its construction. When another laboratory added that somatostatin was also found in the pancreas and inhibited not only growth hormone but also glucagon and insulin production, the definition of somatostatin had to be changed, in the same way as the definition of GRF had to be altered when Brazeau failed to get positive results in his assay. The new object is completeley defined by the list of answers in laboratory trials. To repeat this essential point in a lighter way, the new object is always called after a name of actions summarising the trials it withstood like the old Red Indian appellations 'Bear Killer' or 'Dread Nothing' or 'Stronger than a Bison'!

In the strategies we have analysed so far, the spokesperson and the actants he or she represented were already present, arrayed and well drilled. In this new strategy the representatives are looking for actants they do not know and the only thing they can say is to list the answers the actants make under trials.

Pierre and Marie Curie originally had no name for the 'substance x' they tried out. In the laboratory of the Ecole de Chimie the only way to shape this new object is to multiply the trials it undergoes, to attack it by all sorts of terrible ordeals (acids, heat, cold, presure).[12] Will something resist all these trials and tribulations? If so, then here it is, the new object. At the end of their long list of 'sufferings' undergone by the new substance (and also by the unfortunate Curies attacked by the deadly rays so carelessly handled) the authors propose a new name – 'polonium'. Today polonium is one of the radioactive elements; at the time of its inception it was the long list of trials successfully withstood in the Curies' laboratory:

> (9) Pierre and Marie Curie: –Here is the new substance emerging from this mixture, pitchblende, see? It makes the air become conductive. You can even measure its activity with the instrument that Pierre devised, a quartz electrometer, right here. This is how we follow our hero's fate through all his ordeals and tribulations.
>
> Scientific Objector: This is far from new, uranium and thorium are also active.
>
> –Yes, but when you attack the mixture with acids, you get a liquor. Then, when you treat this liquor with sulphurated hydrogen, uranium and thorium stay with the liquor, while our young hero is precipitated as a sulphuride.
>
> – What does that prove? Lead, bismuth copper, arsenic and antimony all pass this

trial as well, they too are precipitated!

–But if you try to make all of them soluble in ammonium sulphate, the active something resists . . .

– Okay, I admit it is not arsenic, nor antimony, but it might be one of the well-known heroes of the past, lead, copper or bismuth.

–Impossible, dear, since lead is precipitated by sulphuric acid while the substance stays in solution; as for copper, ammoniac precipitates it.

–So what? This means that your so-called 'active substance' is simply bismuth. It adds a property to good old bismuth, that of activity. It does not define a new substance.

–It does not? Well, tell us what will make you accept that there is a substance?

– Simply show me one trial in which bismuth reacts differently from your 'hero'.

– Try heating it in a Boheme tube, under vacuum, at 700° centigrade. And what happens? Bismuth stays in the hottest area of the tube, while a strange black soot gathers in the cooler areas. This is more active than the material with which we started. And you know what? If you do this several times, the 'something' that you confuse with bismuth ends up being four hundred times more active than uranium!

–. . ..

–Ah, you remain silent . . .. We therefore believe that the substance we have extracted from pitchblende is a hitherto unknown metal. If the existence of this new metal is confirmed we propose to name it polonium after Marie's native country.

What are these famous things which are said to be behind the texts made of? They are made of a list of victories: it defeated uranium and thorium at the sulphurated hydrogen game; it defeated antimony and arsenic at the ammonium sulphur game; and then it forced lead and copper to throw in the sponge, only bismuth went all the way to the semi-final, but it too got beaten down during the final game of heat and cold! At the beginning of its definition the 'thing' is a *score list* for a series of trials. Some of these trials are imposed on it either by the scientific objector and tradition – for instance to define what is a metal – or tailored by the authors – like the trial by heat. The 'things' behind the scientific texts are thus similar to the heroes of the stories we saw at the end of Chapter 1: they are all defined by their **performances**. Some in fairy tales defeat the ugliest seven-headed dragons or against all odds they save the king's daughter; other inside laboratories resist precipitation or they triumph over bismuth . . .. At first, there is no other way to know the essence of the hero. This does not last long however, because each performance presupposes a **competence**[13] which retrospectively explains why the hero withstood all the ordeals. The hero is no longer a score list of actions; he, she or it is an essence slowly unveiled through each of his, her or its manifestations.

It is clear by now to the reader why I introduced the word 'actant' earlier to describe what the spokesperson represents. Behind the texts, behind the instruments, inside the laboratory, we do not have Nature – not yet, the reader will have to wait for the next part. What we have is an array allowing new extreme constraints to be imposed on 'something'. This 'something' is progressively shaped by its re-actions to these conditions. This is what is behind all the

arguments we have analysed so far. What was the endorphin tried out by the dissenter in Part A, section 3? The superimposition of the traces obtained by: a sacrificed guinea pig whose gut was then hooked up to electric wires and regularly stimulated; a hypothalamus soup extracted after many trials from slaughtered sheep and then forced through HPLC columns under a very high pressure.

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

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

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

(4) Laboratories against laboratories

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

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

What makes a laboratory difficult to understand is not what is presently going on in it, but what *has been* going on in it and in other labs. Especially difficult to grasp is the way in which new objects are immediately transformed into something else. As long as somatostatin, polonium, transfinite numbers, or anaerobic microbes are shaped by the list of trials I summarised above, it is easy to relate to them: tell me what you go through and I will tell you what you are. This situation, however, does not last. New objects become **things**: 'somatostatin', 'polonium', 'anaerobic microbes', 'transfinite numbers', 'double helix' or '*Eagle* computers', things isolated from the laboratory conditions that shaped them, things with a name that now seem independent from the trials in which they proved their mettle. This process of transformation is a very common one and occurs constantly both for laypeople and for the scientist. All biologists now take 'protein' for an object; they do not remember the time, in the 1920s, when protein was a whitish stuff that was separated by a new ultracentrifuge in Svedberg's laboratory.[15] At the time protein was nothing but the action of differentiating cell contents by a centrifuge. Routine use however transforms the naming of an actant after what it does into a common name. This process is not mysterious or special to science. It is the same with the can opener we routinely use in our kitchen. We consider the opener and the skill to handle it as one black box which means that it is unproblematic and does not require planning and

attention. We forget the many trials we had to go through (blood, scars, spilled beans and ravioli, shouting parent) before we handled it properly, anticipating the weight of the can, the reactions of the opener, the resistance of the tin. It is only when watching our own kids still learning it the hard way that we might remember how it was when the can opener was a 'new object' for us, defined by a list of trials so long that it could delay dinner for ever.

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

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

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

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

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

write the word 'true', a new, formidable ally suddenly appears in the winner's camp, an ally invisible until then, but behaving now as if it had been there all along: Nature.

Part C
Appealing (to) Nature

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

(1) 'Natur mit uns'

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

Is this not a refreshing objection? It means that Galileo was right after all. The dreadnoughts I studied in Chapters 1 and 2 may be easily defeated in spite of the many associations they knit, weave and knot. Any dissenter has got a chance. When faced with so much scientific literature and such huge laboratories, he or she has just to look at Nature in order to win. It means that there is a *supplement*, something more which is nowhere in the scientific papers and nowhere in the labs which is able to settle all matters of dispute. This objection is all the more

refreshing since it is made by the scientists themselves, although it is clear that this rehabilitation of the average woman or man, of Ms or Mr Anybody, is also an indictment of these crowds of allies mustered by the same scientists.

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

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

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

Another violent dispute divides those who believe dinosaurs to have been cold-blooded (lazy, heavy, stupid and sprawling creatures) and those who think that dinosaurs were warm-blooded (swift, light, cunning and running animals).[16] If we support the objection, there would be no need for the 'average man' to read the piles of specialised articles that make up this debate. It is enough to wait for Nature to sort them out. Nature would be like God, who in medieval times judged between two disputants by letting the innocent win.

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

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

says. Hold it! The challengers are supposed to be refereed by Nature, and not to start another dispute about what Nature's voice really said.

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

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

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

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

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

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

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

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

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

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

This sudden inversion of what counts as referee and what counts as being refereed, although counter-intuitive at first, is as easy to grasp as the rapid

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

So we are confronted with two almost simultaneous suppositions:

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

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

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

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

The two faces of Janus talking together make, we must admit, a startling spectacle. On the left side Nature is cause, on the right side consequence of the end of controversy. On the left side scientists are *realists*, that is they believe that representations are sorted out by what really is outside, by the only independent referee there is, Nature. On the right side, the same scientists are *relativists*, that is, they believe representations to be sorted out among themselves and the actants they represent, without independent and impartial referees lending their weight to any one of them. We know why they talk two languages at once: the left mouth speaks about settled parts of science, whereas the right mouth talks about

unsettled parts. On the left side polonium was discovered long ago by the Curies; on the right side there is a long list of actions effected by an unknown actant in Paris at the Ecole de Chimie which the Curies propose to call 'polonium'. On the left side all scientists agree, and we hear only Nature's voice, plain and clear; on the right side scientists disagree and no voice can be heard over theirs.

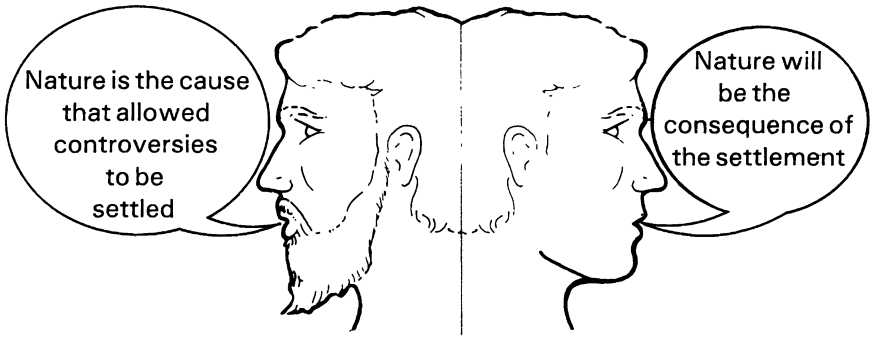


Figure 2.5

(3) The third rule of method

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

This principle is easy to apply as long as the dispute lasts, but is difficult to bear in mind once it has ended, since the other face of Janus takes over and does the talking. This is what makes the study of the past of technoscience so difficult and unrewarding. You have to hang onto the words of the right face of Janus – now barely audible – and ignore the clamours of the left side. It turned out for instance that the N-rays were slowly transformed into artefacts much like Schally's GHRH. How are we going to study this innocent expression 'it turned out'?

Using the physics of the present day there is unanimity that Blondlot was badly mistaken. It would be easy enough for historians to say that Blondlot failed because there was 'nothing really behind his N-rays' to support his claims. This way of analysing the past is called Whig history, that is, a history that crowns the winners, calling them the best and the brightest and which says the losers like Blondlot lost simply *because* they were wrong. We recognise here the left side of Janus' way of talking where Nature herself discriminates between the bad guys and the good guys. But, is it possible to use this as the reason why in Paris, in London, in the United States, people slowly turned N-rays into an artefact? Of course not, since at that time today's physics obviously could not be used as the touchstone, or more exactly since today's state is, in part, the *consequence* of settling many controversies such as the N-rays!

Whig historians had an easy life. They came after the battle and needed only one reason to explain Blondlot's demise. He was wrong all along. This reason is precisely what does not make the slightest difference while you are searching for truth in the midst of a polemic. We need, not one, but *many* reasons to explain how a dispute stopped and a black box was closed.[17]

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

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