SCIENCE IN ACTION

How to follow scientists and engineers through society

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INTRODUCTION

Opening Pandora's Black Box

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

Scene 2: In 1951 in the Cavendish laboratory at Cambridge, England, the X-ray pictures of crystallised deoxyribonucleic acid were not 'nice pictures' on a computer screen. The two young researchers, Jim Watson and Francis Crick[1], had a hard time obtaining them from Maurice Wilkins and Rosalind Franklin in London. It was impossible yet to decide if the form of the acid was a triple or a double helix, if the phosphate bonds were at the inside or at the outside of the molecule, or indeed if it was an helix at all. It did not matter much to their boss, Sir Francis Bragg, since the two were not supposed to be working on DNA anyway, but it mattered a lot to them, especially since Linus Pauling, the famous chemist, was said to be about to uncover the structure of DNA in a few months.

Scene 3: In 1980 in a Data General building on Route 495 in Westborough, Massachusetts, Tom West[2] and his team were still trying to debug a makeshift prototype of a new machine nicknamed *Eagle* that the company had not planned to build at first, but that was beginning to rouse the marketing department's interest. However, the debugging program was a year behind schedule. Besides, the choice West had made of using the new PAL chips kept delaying the machine – renamed *Eclipse MV/8000*, since no one was sure at the time if the company manufacturing the chips could deliver them on demand. In the meantime, their main competitor, DEC, was selling many copies of its *VAX 11/780*, increasing the gap between the two companies.

(1) Looking for a way in

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

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

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

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

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

In the Pasteur Institute John Whittaker is taking no big risk in believing the three-dimensional shape of the double helix or in running his program on the *Eclipse*. These are now routine choices. The risks he and his boss take lie elsewhere, in this gigantic program of comparing all the base pairs generated by molecular biologists all over the world. But if we go back to Cambridge, thirty years ago, who should we believe? Rosalind Franklin who says it might be a three-strand helix? Bragg who orders Watson and Crick to give up this hopeless work entirely and get back to serious business? Pauling, the best chemist in the world, who unveils a structure that breaks all the known laws of chemistry? The same uncertainty arises in the Westborough of a few years ago. Should West obey his boss, de Castro, when he is explicitly asked *not* to do a new research project there, since all the company research has now moved to North Carolina? How long should West pretend he is not working on a new computer? Should he believe the marketing experts when they say that all their customers want a fully compatible machine (on which they can reuse their old software) instead of doing as his competitor DEC does a 'culturally compatible' one (on which they cannot reuse their software but only the most basic commands)? What confidence should he have in his old team burned out by the failure of the *EGO* project? Should he risk using the new PAL chips instead of the older but safer ones?

Figure I.1

Uncertainty, people at work, decisions, competition, controversies are what one gets when making a flashback from certain, cold, unproblematic black boxes to their recent past. If you take two pictures, one of the black boxes and the other of the open controversies, they are utterly different. They are as different as the two sides, one lively, the other severe, of a two-faced Janus. 'Science in the making' on the right side, 'all made science' or 'ready made science' on the other; such is Janus *bifrons*, the first character that greets us at the beginning of our journey.

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

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

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

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

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

> Looking into the VAX, West had imagined he saw a diagram of DEC's corporate organization. He felt that VAX was too complicated. He did not like, for instance, the system by which various parts of the machine communicated with each other, for his taste, there was too much protocol involved. He decided that VAX embodied flaws in DEC's corporate organization. The machine expressed that phenomenally successful company's cautious, bureaucratic style. Was this true? West said it did not matter, it was a useful theory. Then he rephrased his opinions. 'With VAX, DEC was trying to minimize the risk', he said, as he swerved around another car. Grinning, he went on: 'We're trying to maximize the win, and make Eagle go as fast as a raped ape.'
>
> (Kidder: 1981, p. 36)

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

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

> Peter's face betrayed something important as he entered the door, and my stomach sank in apprehension at learning that all was lost. Seeing that neither Francis nor I could bear any further suspense, he quickly told us that the model was a three-chain helix with the sugar phosphate backbone in the center. This sounded so suspiciously like our aborted effort of last year that immediately I wondered whether we might already have had the credit and glory of a great discovery if Bragg had not held us back.
>
> (Watson: 1968, p. 102)

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

> Yet somehow Linus, unquestionably the world's most astute chemist, had come to the opposite conclusion. When Francis was amazed equally by Pauling's unorthodox chemistry, I began to breathe slower. By then I knew we were still in the game. Neither of us, however, had the slightest clue to the steps that had led Linus to his blunder. If a student had made a similar mistake, he would be thought unfit to benefit from Cal Tech's chemistry faculty. Thus, we could not but initially worry whether Linus's model followed from a revolutionary reevaluation of the acid-based properties of very large molecules. The tone of the manuscript, however, argued against any such advance in chemical theory.
>
> (idem: p. 103)

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

> When his mistake became known, Linus would not stop until he had captured the right structure. Now our immediate hope was that his chemical colleagues would be more than ever awed by his intellect and not probe the details of his model. But since the manuscript had already been dispatched to the *Proceedings of the National Academy*, by mid-March at the latest Linus's paper would be spread around the world. Then it would be only a matter of days before the error would be discovered. We had anywhere up to six weeks before Linus again was in full-time pursuit of DNA.
>
> (idem: p. 104)

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

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

ABANDON HOPE ALL YE WHO ENTER HERE.

At the onset of this voyage should be written:

ABANDON KNOWLEDGE ABOUT KNOWLEDGE
ALL YE WHO ENTER HERE.

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

(2) When enough is never enough

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

Janus' first dictum:

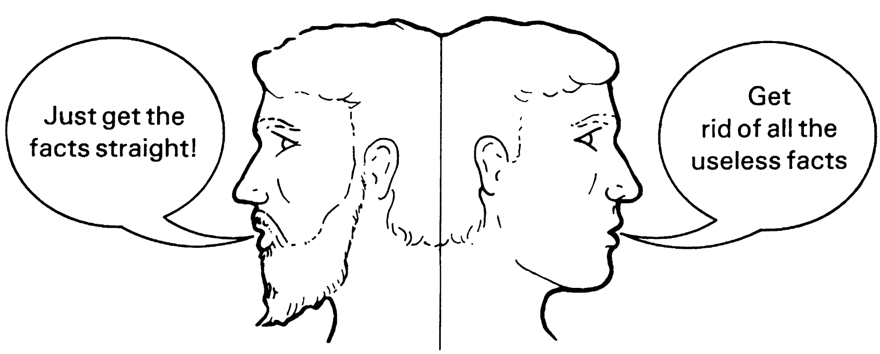


Figure I.2

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

> I no sooner got to the office and began explaining my scheme than the American crystallographer Jerry Donohue protested that the idea would not work. The tautomeric forms I had copied out of Davidson's book were, in Jerry's opinion, incorrectly assigned. My immediate retort that several other texts also pictured guanine and thymine in the enol form cut no ice with Jerry. Happily he let out that for years organic chemists had been arbitrarily favoring particular tautomeric forms over their alternatives on only the flimsiest of grounds. (. . .) Though my immediate reaction was to hope that Jerry was blowing hot air, I did not dismiss his criticism. Next to Linus himself, Jerry knew more about hydrogen bonds than anyone in the world. Since for many years he had worked at Cal Tech on the crystal structures of small organic molecules, I couldn't kid myself that he did not grasp our problem. During the six months that he occupied a desk in our office, I had never heard him shooting off his mouth on subjects about which he knew nothing. Thoroughly worried, I went back to my desk hoping that some gimmick might emerge to salvage the like-with-like idea.
>
> (Watson: 1968, pp. 121–2)

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

> The unforeseen dividend of having Jerry share an office with Francis, Peter, and me, though obvious to all, was not spoken about. If he had not been with us in Cambridge, I might still have been pumping out for a like-with-like structure. Maurice, in a lab devoid of structural chemists, did not have anyone to tell him that all the textbook pictures were wrong. But for Jerry, only Pauling would have been likely to make the right choice and stick by its consequences.
>
> (idem: p. 132)

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

Janus's second dictum:

Figure I.3

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

> 'I want to build a simulator, Tom.'
>
> 'It'll take too long, Alsing. The machine'll be debugged before you get your simulator debugged.'
>
> This time, Alsing insisted. They could not build Eagle in anything like a year if they had to debug all the microcode on prototypes. If they went that way, moreover, they'd need to have at least one and probably two extra prototypes right from the start, and that would mean a doubling of the boring, grueling work of updating boards. Alsing wanted a program that would behave like a perfected Eagle, so that they could debug their microcode separately from the hardware.
>
> West said: 'Go ahead. But I betchya it'll all be over by the time you get it done.'
>
> (Kidder: 1981, p. 146)

The right side's advice is strictly followed by the two men since they want to build the best possible computer. This however does not prevent a new controversy starting between the two men on how to mimic in advance an efficient machine. If Alsing cannot convince one of his team members, Peck, to finish in six weeks the simulator that should have taken a year and a half, then West will be right: the simulator is not an efficient way to proceed because it will come too late. But if Alsing and Peck succeed, then it is West's definition of efficiency which will turn out to be wrong. Efficiency will be the consequence of who succeeds; it does not help deciding, on the spot, who is right and wrong. The right side's advice is all very well once *Eagle* is sent to manufacturing; before that, it is the left side's confusing strategic advice that should be followed.

Janus' third dictum:

Figure I.4

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

> As the summer came on, increasing numbers of intruders were being led into the lab – diagnostic programmers and, particularly, those programmers from Software. Some Hardy Boys had grown fond of the prototypes of Eagle, as you might of a pet or a plant you've raised from a seedling. Now Rasala was telling them that they couldn't work on their machines at certain hours, because Software needed to use them. There was an explanation: the project was at a precarious stage; if Software didn't get to know and like the hardware and did not speak enthusiastically about it, the project might be ruined; the Hardy Boys were lucky that Software wanted to use the prototypes – and they had to keep Software happy.
>
> (idem: p. 201)

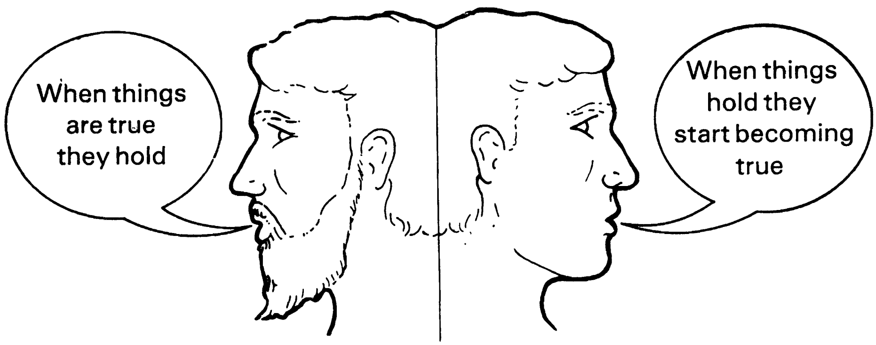
Not only the Software people have to be kept happy, but also the manufacturing people, those from marketing, those who write the technical documentation, the designers who have to place the whole machine in a nice looking box (not a black one this time!), not mentioning the stockholders and the customers. Although the machine has been conceived by West, through many compromises, to keep all these people happy and busy, he cannot be sure it is going to hold them together. Each of the interest groups has to try their own different sort of tests on the machine and see how it withstands them. The worst, for Tom West, is that the company manufacturing the new PAL chips is going bankrupt, that the team is suffering a *post partum* depression, and that the machine is not yet debugged. 'Our credibility, I think, is running out,' West tells his assistants. *Eagle* still does not run more than a few seconds without flashing error messages on the screen. Every time they painstakingly pinpoint the bug, they fix it and then try a new and more difficult debugging program.

> Eagle was failing its Multiprogramming Reliability Test mysteriously. It was blowing away, crashing, going out to never-never land, and falling off the end of the world after every four hours or so of smooth running.
>
> 'Machines somewhere in the agony of the last few bugs are very vulnerable,' says Alsing. 'The shouting starts about it. It'll never work, and so on. Managers and support groups start saying this. Hangers-on say, "Gee, I thought you'd get it done a lot sooner." That's when people start talking about redesigning the whole thing.'
>
> Alsing added, 'Watch out for Tom now.'
>
> West sat in his office. 'I'm thinking of throwing the kids out of the lab and going in there with Rasala and fix it. It's true. I don't understand all the details of that sucker, but I will, and I'll get it to work.'
>
> 'Gimme a few more days,' said Rasala.
>
> (idem: p. 231)

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

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

Janus's fourth dictum:



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

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

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

The remainder of the double helix story looks like the final rounds of a presidential nomination. Every one of the other contenders is introduced into the office where the model is now set up, fights with it for a while before being quickly

> overwhelmed and then pledging complete support to it. Bragg is convinced although still worried that no one more serious than Jim and Francis had checked the helix. Now for the big game, the encounter between the model and those who for years had captured its projected image. 'Maurice needed but a minute's look at the model to like it.' 'He was back in London only two days before he rang up to say that both he and Rosy found that their X-ray data strongly supported the double helix' (p. 131). Soon Pauling rallies himself to the structure, then it is the turn of the referees of *Nature*.

'Of course,' says the left side of Janus, 'everyone is convinced because Jim and Francis stumbled on the right structure. The DNA shape itself is enough to rally everyone.' 'No, says the right side, every time someone else is convinced it progressively becomes a more right structure.' Enough is never enough: years later in India and New Zealand other researchers were working on a so called 'warped zipper'[3] model that did everything the double helix does – plus a bit more; Pauling strongly supported his own structure that had turned out to be entirely wrong; Jim found biological significance in a like-with-like structure that survived only a few hours; Rosalind Franklin had been stubbornly convinced earlier that it was a three-strand helix; Wilkins ignored the keto forms revealed by Jerry Donohue; Chargaff's laws were an insignificant fact they kept in the background for a long time; as to the metal atom toys, they have lent strong support to countless models that turned out to be wrong. All these allies appear strong once the structure is blackboxed. As long as it is not, Jim and Francis are still struggling to recruit them, modifying the DNA structure until everyone is satisfied. When they are through, they will follow the advice of Janus's right side. As long as they are still searching for the right DNA shape, they would be better off following the right side's confusing advices.

We could review all the opinions offered to explain why an open controversy closes, but we will always stumble on a new controversy dealing with how and why it closed. We will have to learn to live with two contradictory voices talking at once, one about science in the making, the other about ready made science. The latter produces sentences like 'just do this . . . just do that . . .'; the former says 'enough it never enough'. The left side considers that facts and machines are well determined enough. The right side considers that facts and machines in the making are always **under-determined.**[4] Some little thing is always missing to close the black box once and for all. Until the last minute *Eagle* can fail if West is not careful enough to keep the Software people interested, to maintain the pressure on the debugging crew, to advertise the machine to the marketing department.

(3) The first rule of method

We will enter facts and machines while they are in the making; we will carry with us no preconceptions of what constitutes knowledge; we will watch the closure of

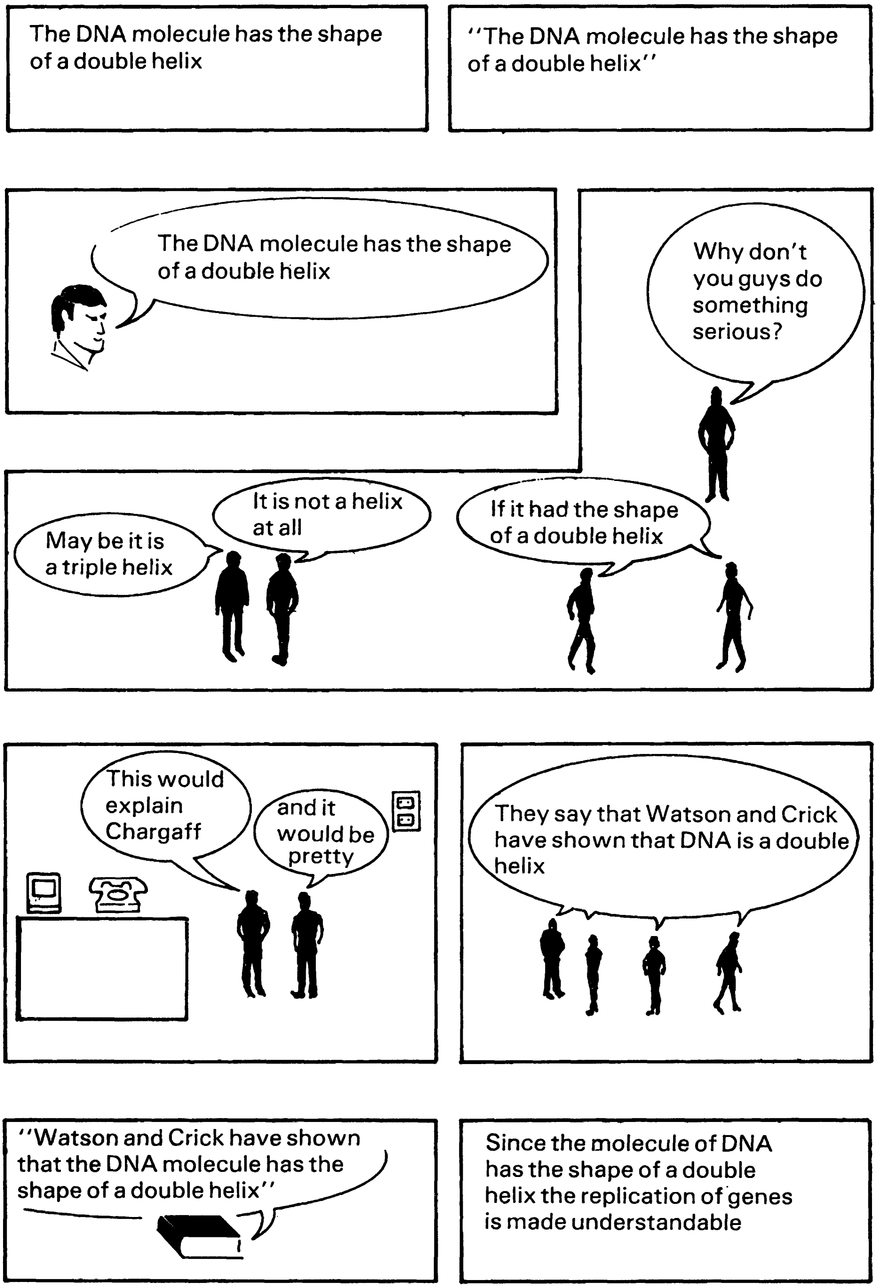

The DNA molecule has the shape of a double helix
"The DNA molecule has the shape of a double helix"
The DNA molecule has the shape of a double helix
Why don't you guys do something serious?
May be it is a triple helix
It is not a helix at all
If it had the shape of a double helix
This would explain Chargaff
and it would be pretty
They say that Watson and Crick have shown that DNA is a double helix
"Watson and Crick have shown that the DNA molecule has the shape of a double helix"
Since the molecule of DNA has the shape of a double helix the replication of genes is made understandable

Figure I.6

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

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

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

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

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

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

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

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

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

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

Part I

From Weaker to Stronger Rhetoric