Passionate Minds

the inner world of scientists

Lewis Wolpert and Alison Richards

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REFLECTIONS

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CHAPTER 1

Passionate Minds

OUTSIDE their own habitat, scientists, as a species, are little understood. If they feature in popular awareness at all, it is through a limited set of media stereotypes. With a few exceptions, if scientists are not mad or bad, they are personality-free, their measured tones and formal reports implying ways of thinking and working far removed from the intellectual and emotional messiness of other human activities.

We embarked on these conversations, which were originally broadcast on BBC Radio 3, as an attempt to redress the balance. We wanted to give a rare glimpse of the intellectual, emotional and imaginative vigour that is the human reality of scientific life. Even a sample of this size demonstrates with extraordinary force the vitality and diversity of mind and temperament jostling within the retaining walls of 'science'. So vividly does the force of each personality spill out from the transcripts—and these can only hint at the voice and emotional colour of the original exchange—that they give the lie once and for all to the notion that science is in any way an 'inhuman' activity. Scientists think and feel about their work using the same psychological apparatus as the rest of us. It also becomes clear within a few pages that there is no one way of 'doing' science, even within recognized disciplines or groups. Among the experimentalists and the theorists, the biologists and the physicists, there are as many differences in style and motivation as there are in haircuts and accents.

These conversations show that it *is* possible for non-scientists to gain a meaningful sense of how scientists 'tick'; or at least as meaningful as the glimpse most of us get from listening to a poet or painter reflect upon their work. The dialogue is accessible, inspiring and full of surprises. We believe that these conversations make a case that society should—and can—be as interested in finding out about the life and times of Nobel scientists as in delving into the psyche of the latest literary prizewinners.

Such activities afford twin delights. Science—as we have just noted—seethes with diversity. Part of the fun is to explore and savour the uniqueness of each individual talent. On the other hand, it is also illuminating to seek patterns and underlying unities. We don't believe for a minute that there is a grand theory of scientific discovery any more than there is a grand theory of painting, but this collection of conversations also offers pleasurable scope for trend spotting amidst the infinite variety of genius.

For example, a casual glance through the following pages suggests that being an outsider is a definite help when it comes to making important scientific discoveries. The Spanish developmental biologist, Antonio Garcia Bellido, conducted much of his fundamental research in a country lacking an established scientific community or tradition. In the case of the evolutionary biologist, Richard Lewontin, it is his radical politics which set him apart: 'I feel alienated from [the scientific community]. I have little or nothing in common with my colleagues. We take the opposite point of view on almost everything . . . on every issue we struggle.'

In some cases the outsiders' cultural baggage plays a part in their success. The Polish-born poet and chemist Roald Hoffmann refers to being one of 'Hitler's last gifts to America'. The cell biologist Mike Berridge, who grew up in Rhodesia, cites the discipline of a colonial way of life. But being an outsider clearly confers a more fundamental advantage—notably the ability to view a subject or problem with an eye unclouded by history, habit or dogma. The Nobellist, Sir James Black invented beta-blockers and the ulcer drug, cimetidine, because he dared to enter a new field and didn't mind asking questions which were 'really quite preposterous'.

The immunologist and neurobiologist Gerald Edelman talks in terms of an 'innocence' which means you fail to realize how difficult a question it is that you are asking. Had he not been thus protected he would never have contemplated probing the structure of the antibody molecule. Smart people, as Richard Lewontin observes, 'are not crazy enough to spend their lives trying to solve really hard problems'.

Innocents and outsiders may also disregard—or fail to register—conventional barriers. The physicist and historian of science, Gerald Holton, makes the point that Freud, Einstein and Darwin didn't divide up the world in the conventional way: 'Think of Freud. The difference between a mature man or woman and a child . . . [is] obvious; [but] to Freud [it is] not at all obvious, he wants to find a continuity between what to us are completely separate things. The same with Darwin seeing the link between human species and other species.' Neither the particle physicist Carlo Rubbia, nor James Lovelock, originator of the Gaia theory, see science or nature as departmentalized. For Rubbia, 'There is only one type of science and the various fields are chapters of the same book'. In Lovelock's mind, 'The territories, the disciplines, are purely feudal, set up by professors to retain territories over which they have control'.

The ability to take advantage of this kind of intellectual free market is frequently cited as a key to scientific success. The physical anthropologist David Pilbeam sees it as his main strength, 'the kind of synthesis that occasionally opens up new questions and new ways of thinking about things. Putting material, diverse material, together in ways that haven't been done before.' Michael Berridge, who made fundamental discoveries about the way cells communicate with each other, is equally definite: 'If I was thinking of any single gift that one needs, it's this ability to make connections between a lot of disparate facts . . . The gifted scientists that I meet . . . have this facility. They have a very broad view of what's going on and they're able to make connections between different ideas, different disciplines.'

'Freelance personality' is the term that Rubbia uses to describe the kind of intellectual buccaneer that every successful scientist must be. But it is apparent that other qualities are essential, too. High on the list comes persistence, a concept that reverberates like an echo through the conversations. Scientists clearly find science hard. It's 'extremely difficult' says Mike Berridge, 'because you're up against nature . . . [it's] very much like a battle. You're like a general marshalling his forces to try and unlock some of the secrets.' Good ideas don't seem to come up very often: 'I think one's doing extremely well if one has a good idea once every, let's say, six months' observes the immunologist Avrion Mitchison, drily. Even then, of course, there's no guarantee that the idea will work. There may be insurmountable practical problems. Anne McLaren is a mammalian developmental biologist, whose doctoral project required pregnant rabbits, but 'none of the rabbits in the Oxford animal house got pregnant in the whole of the first year that I was supposed to be doing this Ph.D.' In the event she had to find a completely different problem. Even with the practical difficulties overcome, it may turn out that the brilliant idea you had was way off course. The late Nobel chemist, Peter Mitchell, recalled 'I had a particular view . . . and it turned out that at the end of nearly eight years I was wrong.' Moreover, this experience seems to be the rule rather than the exception. According to the inventor of the contraceptive pill, Carl Djerassi, '. . . scientific research most of the time is just a series of failures'. Another Nobellist, the physicist Murray Gell-Mann describes the constant uncertainty and worry that for him is an inescapable part of research: 'No matter what I've come up with, I've doubted it and worried about it and been terrified that it was wrong or trivial.' Science, asserts Mike Berridge, always involves 'long periods of gloom'.

Clearly, however, good scientists don't give up. One aspect of this seems to be their own certainty, whether in the short or long term, that they are on the right track. They have to have the talent, as Gerald Holton describes it, 'to smell out where it will all lead, before it becomes simple to show it'. It is having the ability to discern when it is the meter readings that are at fault rather than the idea that is wrong. Such people also have to have a hefty helping of courage and stubbornness to sustain their assault on the conventional wisdom. When Edelmen was working on the structure of antibodies 'People thought I was crazy—and although they didn't shy away from me at the lunch table, they thought that my work was pretty preposterous'. Sheldon Glashow, who shared the 1979 Nobel prize for physics for his work towards the unification of forces, describes how it was necessary to believe in quarks for years before 'these crazy ideas . . . these particles that no one had ever seen, and according to the theory today, no one ever will' were validated experimentally. 'If you would simply take all the kookiest ideas of the early 1970s and put them together, you would have made for yourself the theory which is, in fact, the correct theory of nature. So it was like madness, it was everybody's weirdest fancy was right.' For some it took even longer. It was the best part of twenty years before either Peter Mitchell's or James Lovelock's ideas were accepted.

On a day-to-day basis, scientists seem to keep going because, gloom and difficulty notwithstanding, they find science fun. Partly, suggests Rubbia, it is a matter of attitude. It may be painful at times, but 'It is sort of a game. Any fundamental advances in our field are made by looking at it with the smile of a child who plays a game.' Peter Mitchell talks in a similar vein: 'whether you turn out to be wrong, or right, everybody wins. This is something else I think that even quite a number of scientists don't seem to appreciate, that being wrong in science is often much more fun than being right, because the next day you wake up with a new horizon . . . It does make a personal impact, of course, but this is something you need to bear because there are more important things than the particular dent in your own personality.' This sense of a serious 'playfulness' crops up again and again. The palaeontologist Elwyn Simons chose to be a scientist because he wanted 'a career doing something that always seems like play'. He continues: 'It's fun to find fossils because you never know what you're going to find and there's always a chance that you'll find something quite unusual, and that kind of excitement makes it sort of like a treasure hunt.' Another field scientist, Jared Diamond, who works in New Guinea, is equally enthusiastic. 'I loved it. Emotionally I'm half New Guinean . . . My identity is very connected with that work . . . it is so incredibly vivid out there. I really love the jungle, which is beautiful, and the birds are fascinating. Tropical biology is very complicated but I learned how to deal with it.'

Of course, it's not only field workers who find science enjoyable. 'That's the trouble,' says the clinician and molecular biologist David Weatherall, 'everything's so interesting . . . in our particular field, although practical things are always on one's mind, there's a tremendous temptation to shoot off into problems of evolution, population and genetics . . . [so you] become a kind of gorgeous amateur at everything.' John Cairns, another molecular biologist, drifted into bacteriology, initially, because he loved 'the smell of bacteria growing on plates. It is a rather nice smell. When you come into the laboratory in the morning there's this homely smell greeting you.' The applied mathematician Sir James Lighthill has spent much of his career working on fluids: 'I have a sort of general pleasurable feel about fluids . . . And my hobby is swimming; I have a great deal of interest in the ocean—ocean waves, ocean currents, ocean tides—and so I enjoy observing all that when I swim. And then I have a fellow feeling for the swimming animals and I've written papers about almost all varieties of swimming fishes and invertebrates'.

A strong sense of an aesthetic pleasure also runs through scientists' accounts of their work. James Black loves chemistry because 'in an imaginative sense, [its] entirely open ended, and entirely pictorial . . . I daydream like mad . . . it's simply something which is a rich food for the imagination. You can have all these structures in your head, turning and tumbling and moving . . . They're just intrinsically beautiful.' Cairns is a committed experimentalist. 'For me the fun of an experiment is like the fun of doing a lot of washing up. You get everything ready and you make damn sure that you can't go wrong. I like to think it's something of an art if one pair of hands gets through a huge amount of work in a short period of time simply by organizing it.' Antonio Garcia Bellido uses a similar analogy: 'the pleasure of finding something that explains the phenomena . . . This feeling is similar in many respects to the feeling of a creative artist who has finished a painting or a musical composition. It's a feeling of having been in agreement with somebody, with nature'. Another Nobel chemist, Roald Hoffmann, who is also a poet, is quite explicit: 'Poetry and a lot of science—theory building, the synthesis of molecules—are creation. They're acts of creation that are accomplished with craftsmanship, with an intensity, a concentration, a detachment, an economy of statement. All of these qualities matter'. There is also 'a search for understanding . . . a valuation of complexity and simplicity, of symmetry, and asymmetry. There is an act of communication'.

For many the aesthetic satisfaction is what sustains them. 'I think the bit I enjoy most', says Anne McLaren, 'is analysing data. If one is presented with a pile of raw data, and can turn it into a satisfying story, that's very enjoyable. I think that's what I enjoy most.' For the biotechnologist, Leroy Hood, 'what I've been impressed with in science over my twenty-one years is . . . there is a simple elegant beauty to the underlying principles, yet when you look into the details it's complex, it's bewildering, it's kind of overwhelming, and I think of beauty in the sense of being able to extract the fundamental elegant principles from the bewildering array of confusing details, and I've felt I was good at doing that, I enjoy doing that.'

But scientific staying power is more than achieving an equilibrium between pain and satisfaction. Another notion which echoes through the collection is that scientists are 'driven'. The conversations are shot through with the metaphors of religious experience, physical dependence and sexual pleasure. 'It's extremely difficult', explains Gerald Edelman, 'to say . . . well, I'm going into the lab again tomorrow because maybe I'll have another epiphany. Experience says forget it . . . that isn't what drives me. Curiosity drives me. I believe that there is a group of scientists who are kind of voyeurs. They have the splendid feeling, almost a lustful feeling, of excitement when a secret of nature is revealed . . . and I would certainly place myself in that group.' Carlo Rubbia describes it as follows: 'We're driven by an impulse which is one of curiosity, which is one of the basic instincts that a man has. So we are . . . driven . . . not by success, but by a sort of passion, namely the desire of understanding better, to possess, if you like, a bigger part of the truth.' Nicole le Douarin, a developmental biologist, was first attracted to the field because 'It is fascinating to start with an egg and twenty-one days later you have a chick . . . and you realize, even if you a very ignorant, that the process is very mysterious. This is what fascinates me, and still does. We are still very far from understanding what is going on.'

This 'driven' quality gives scientists an extraordinary degree of commitment.

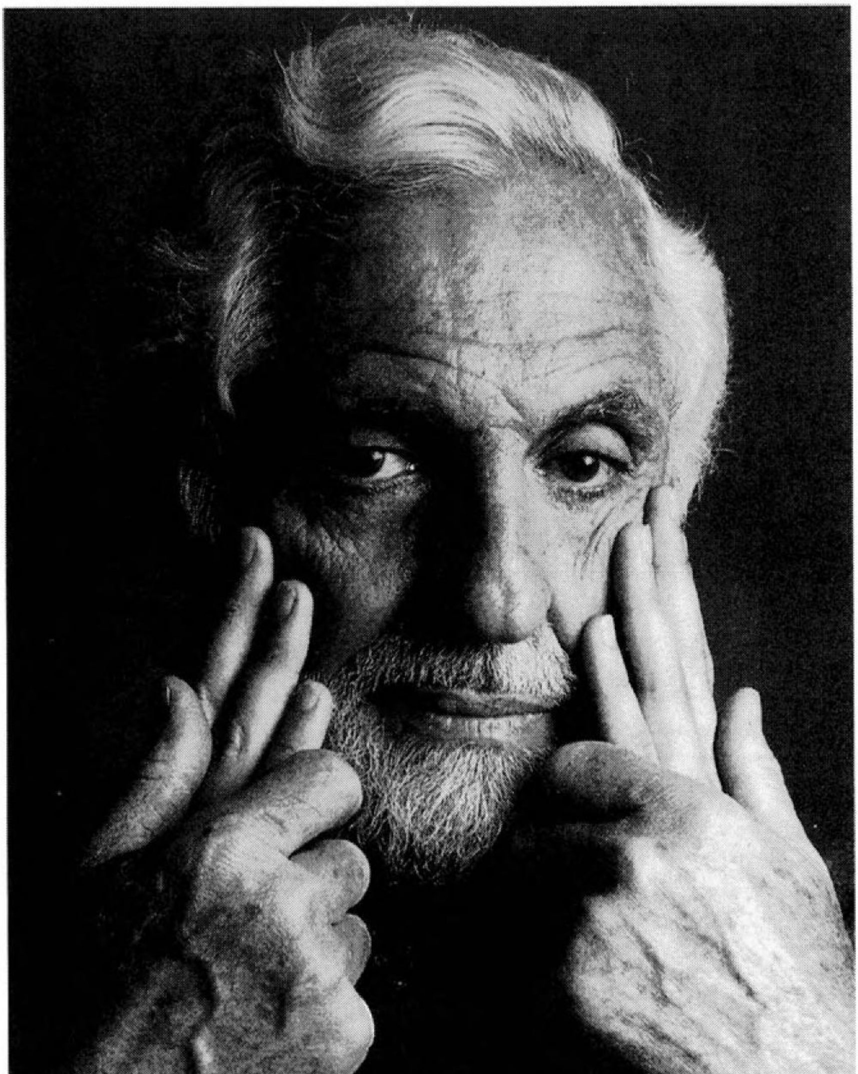
Anne McLaren describes herself as 'obsessional'. Hoffmann confesses to being 'addicted to it . . . it's very hard to let go . . . I go in the library on Saturday and Sunday and there are 200 journals. When I come back from a trip that's what I do, sometimes even before reading my mail, I want to see what's out there, what's been done. I love it.' For Rubbia, '[science] is not a job nine to five. When you do science you have to do science 24 hours a day. When you are at home you should be thinking about science; when you are going to bed, you should be dreaming about science. It's full immersion you see.' Michael Berridge says much the same: 'Almost all my waking hours are spent thinking about science . . . to be a successful scientist you really have to worry away at things all the time and even hope, when you're asleep, that your subconscious is continuing to worry away at some of the problems . . . It is a completely all-engrossing, full-time occupation.' Rubbia does not need hobbies. Avrion Mitchison distrusts holidays: 'A long rest, a nice summer holiday, that's not a great way of having good ideas. Good ideas come from exercising the mind.' Carl Djerassi was always able to pursue other interests as well, but only because he was so single minded and efficient. This left him, he freely admits, with little time for '"ad hoc-ery". People almost have to make appointments with you; you even have to make appointments with yourself. If someone you love, for example, says, "I would like to see you", the silent—or, rather, the loud—question is, "About what? How long will it take?"'

It is this sense of consuming commitment which James Lovelock sees as the distinctive quality of good scientists. ' [many] people who are nowadays called scientists are not really scientists, any more than advertising copywriters are literary people. They may be able to write beautiful copy, but it's not quite the same thing. They are in a job, a career. A scientist shouldn't be, I think. A scientist is much more like a creative artist, somebody who does it for a vocation. It's the only way of life they want'.

We could go on. But there is a danger that such exercises draw attention away from the essential individuality of science. These interviews were recorded and presented as series of single and separate conversations because each was intended as a personal exploration and not an attempt to reach great and universal truths. Notwithstanding the inevitable technical details—though we have tried to avoid or elucidate these wherever possible—it is the human qualities of science which come over most strongly: its energy and imaginative richness; the sensations of frustration, love, despair and enchantment which hold its practitioners in its thrall. This is the second collection of our conversations with scientists published by Oxford University Press. We called the first *A Passion for Science* and have deliberately kept the reference to passion in the title because it seems, more than ever, to sum up the power and variety of emotions that these dialogues invite you to share.

A.R.
April 1997

IN TWO MINDS



CARL DJERASSI
was born in 1923 and is Professor of Chemistry at Stanford University, California.

CHAPTER 2

Blemished heroes

Carl Djerassi

Chemist

IT is, to say the least, unusual to find a distinguished scientist who is also successful in the arts. Carl Djerassi, having practically invented the birth control pill, and built up such an art collection that he could afford to donate dozens of works by Paul Klee to the San Francisco Museum of Modern Art, also has a growing reputation as a poet and novelist to add to his list of achievements. It puts him in the unusual position of being able to consider the creative process from the point of view of both the scientist and the artist, and to act as a mediator between the arcane culture of scientific research and life outside. His first novel, *Cantor's Dilemma*, which is set in the high-pressure world of cancer research, attempts to show science as it really is. The public mask of the formal paper is replaced by a very different picture of the scientific process: one coloured by the personal chemistry of temperament, motive and ambition, and darkened by the greyer tones of short cuts, competition and even fraud.

Born in Vienna in 1923, Djerassi trained in the United States, where he has been mainly based ever since. He now divides his life between London, where he spends every summer writing full-time, and California, where he still teaches in the Chemistry Department at Stanford University and has also founded an artists' colony which provides studio space and residencies for up to seventy artists a year. It sounds like the existence of a modern Renaissance man. I wondered whether this was something he had set out deliberately to achieve.

No, but it's probably my character. I'm usually stimulated by doing several very different things. Not exactly concurrently, but perhaps during the same day. This is why even in my scientific work I usually work on several very different projects at the same time. But there's another thing. As you know, scientific research most of the time is just a series of failures. But something every once in a while, of course, works. So if you work on several topics at once, the likelihood is that even though you have some disasters and feel depressed, there is also something you feel good about. I think that's probably another part of the reason why I've been active in various things.

'But that's unusual, because I think most scientists tend to focus very tightly on a particular problem. You seem to have adopted a very different strategy.'

Yes, but you know, I'm a very-well-organized person . . . and that's both good and bad. It's good in the sense that I use my time very efficiently and therefore can do, perhaps, more things per unit of time. It's bad because if you're very well organized you have very little time for 'ad hoc-ery'. People almost have to make appointments with you; you even have to make appointments with yourself. If someone whom you love, for example, says 'I would like to see you', the silent—or, rather, the loud—question is, 'about what? how long will it take?', because it's interrupting your self-imposed schedule. So that is certainly one of the obvious disadvantages, and it took me some time to really learn that.

'Were you always very organized?'

Yes. Except maybe during my childhood. But I was already very well organized by the time I got to college. I was probably the only graduate student at the University of Wisconsin who did not work in the lab at night. I refused to do that, because I wanted to do other things, especially since I was already married at that time. Therefore I did as much work from eight to five, or nine to five, or whatever it was, than others did between, perhaps, ten and eight, or ten and nine at night.

'For you, then, it hasn't been a problem but a pleasure to be active in all these areas. One hasn't interfered with the other?'

No, except as I implied earlier, I think you do pay a price.

'What price?'

Well the fact that I was married three times probably can in part be assigned to this.

'So is the implication that you were so highly organized that you actually had no time for your wife, or your earlier wives?'

It's not so much that I didn't have time for them, but that they didn't speak the same language as I did. They were not scientists, and there was no question during that time that the bulk of my life dealt with very scientific affairs. The theatre, the opera, the music, the art collecting, did all go on but there was much less of it, and I did not write any fiction or any poetry until my second marriage was over. I think that could, perhaps, have made a difference.

'What made you, then, move into the arts?'

Well, 'arts' is a broad word. When it comes to art in the context of art appreciation and music appreciation, that has been with me, probably since my early adulthood. But when you talk about art in the context of actively doing it myself, of writing fiction or poetry, then, literally, I didn't write a word of poetry or prose until my sixtieth birthday.

'So it came very late?'

Oh yes.

'Why did it come then? What made it happen?'

Well, there was an acute reason. It was a traumatic period in my emotional life because I was very much in love with someone who left me. Now, there's a good end to this story because after a year we got together again and we're now married. But during that year I felt very violated. Diane Middelbrook left me for another man. It was unannounced, and I really felt self-pitying and revengeful—all that sort of thing. And for some reason this manifested itself in a very verbal way, in a confessional form of poetry. Now the person I was and am in love with is a professor of literature and poetry herself, so you might say it was a typical macho way of 'revenging' myself on her turf rather than on my own.

'To what extent, though, was your ability to change partly due to the fact that you were financially and professionally secure?'

Well, that had a great deal to do with it. In fact you've probably put your finger on it. It was very important that I didn't have to support myself through my writing and had established a reputation in science that was unlikely to change very much. It gave me the chance to do something totally different. In fact, in terms of intellectual life I can think of nothing that is more different from science than fiction. Fiction is almost the antithesis of science. Scientists never have the luxury of being able to say 'ah, but I made it up'. That's *verboten.* So fiction is a wonderful luxury. You can even brag about it. When you say 'I made it up' people congratulate you! Scientists would kick you out; they would say you are finished.

'Do you, then, see the creative process in arts and science as completely different?'

In some respects I see it as different. First of all there's the manner in which we write about it. In science all we're interested in doing—I'm talking about my own science, chemistry, physics, the hard sciences—is transmitting information. That is all there is to it. You're not supposed to embellish it at all. The moment you embellish it your editors tell you that you're verbose; you must cut it out, be short and succinct.

Your readers aren't interested in anything other than information. They usually read your paper only once, unless they want to repeat some experimental recipe, and then they file it in their mind if it's useful. If it is not useful for them, it becomes mental garbage and gets thrown out. You can't afford to keep it all in your mind. So scientific papers are a very ephemeral form of literature. People very rarely re-read them. But in poetry, on the other hand, the ultimate compliment is to have someone re-read your poems, to remember that book, to remember some metaphor, some nuance which would count for nothing in science.

'That's a major difference in the end product, as it were, but is there any similarity in the actual creative act between art and science?'

Yes, the fact that in both you're doing what hasn't been done before. You flatter yourself that you are the first to open up some terrain, or see something in a new way. Most of the things that fictional artists make up have already been around to a large extent. In one way or another the number of plots can be reduced to very few. So you have to find a new perspective. And that new insight into something is a very exciting but very short-term feeling. It's almost, you know, like sexual pleasure, you have a real high and then it's gone.

'And you get that pleasure in science too?'

Oh, absolutely! I'm absolutely convinced that the pleasure of a real scientific insight—it doesn't have to be a great discovery—is like an orgasm. But it's short. And after a while, the more important it is, the more obvious it becomes in the end. People ask why did you not think of it before? And then—and this is the ultimate compliment in science—they just accept it and forget whose insight it was. It becomes common knowledge.

'I still have no idea, though, what it means to be creative in chemistry.'

Well, it is not different to be creative in chemistry than in any other discipline but, even so, it depends very much on your sub-discipline of chemistry. You have to some extent made a very complicated comment. The real division, and it was certainly true in my own life, is between a synthetic chemist—a chemist who synthesizes something whose structure is known or has been seen—and a chemist who elucidates structures. Someone, in other words, who isolates material from natural sources and then establishes its chemical structure.

I initially started out as a synthetic chemist, and it was during that time that we invented the birth control pill. And here I think the analogy is a very simple one. Synthetic chemists are both architects and builders. We first design a building on paper, and then become the builders who actually construct it. We know all along what we are going to do from having a picture of the house that we want to build, to—unless you're a very unsuccessful builder—eventually constructing it. By contrast, an analytical chemist who is interested in establishing the structure of an unknown compound is in a much more difficult position—one that in the end interested me intellectually much more. Because here at the outset you know nothing about what you're working on. It's like being in a pitch-dark room. First, chemists started feeling around to get an idea of what's in there, what the materials are from which it is made. Later they started to acquire a sort of a candle or penlight and eventually had powerful flashlights. These were the various spectroscopic methods—ultraviolet, infrared, and NMR spectroscopy, mass spectroscopy and the like—which are the chemist's 'flashlights'. Gradually you get to know more and more about the room and eventually you put all the information together, right down to what the colours are and everything else about it. That is intellectually very, very exciting.

It's not the same nowadays, however. It's all changed dramatically through the development of one discovery, one sub-discipline of chemistry, which is particularly highly developed in Great Britain, and that is X-ray crystallography. It's the equivalent of having the little flashlight taken away and suddenly being given a flash camera. You take one quick picture and immediately know everything that's in the room. Thus intellectually the actual structure elucidation process is not very exciting any more, but in terms of information it gives you everything you need. So then the question is, what do you do with that knowledge? Which, translated into chemistry, means what is the physical or biological utility of this structure? How and why does the organism synthesize this particular compound? What can you use it for? What are the advantages, the disadvantages, the dangers? And it's in this area—the practical applications of your chemistry—that the real challenges now lie.

'But I want to pursue this idea of the creative process. When you set out to synthesize the birth control pill, how difficult a problem was it? What was your particular cleverness? Was it, to use your metaphor, the actual design of the building or compound, or its construction?'

Well, in some respects it was both these, but it was something else as well, which was much more important. Let me use an analogy which is a musical one. Say you decide you want us to design a concert hall. We can draw all the plans; tell you how large it will be, what materials are going to be used, and so on, but in the end we can only guess at what we think will be the best design for the ideal acoustics. We will not know whether it works until you play the first concert. Well it was the same with the chemical structure of what became the birth control pill. Endocrinologists had to do the final biological test to see whether what we had predicted did, in fact, happen. And it was the ability to make the right predictions that I think was the important thing. We really didn't know whether we were right until finally that experiment was done.

'What made you choose that problem then?'

To be quite honest, at the time we chose it, the problem was not really directly connected with birth control. What we wanted to do was to synthesize an orally effective progestational hormone. Progesterone is one of the two female sex hormones, and the one that acts as Nature's contraceptive. A woman does not ovulate and become pregnant again during pregnancy because during that time, and only during that time, does she continuously secrete progesterone which inhibits further ovulation. But at the time (1950–1951) we began working on this problem, progesterone was not used for birth control, but to treat menstrual disorders and infertility. Also, and most importantly for us, it was thought to be a potential treatment for certain forms of cancer, particularly cervical cancer. As progesterone itself was only biologically active if given by injection, it meant that large volumes had to be injected locally into the cervix, which was very painful. So we wanted to develop—to invent, really—a synthetic progestational hormone which would still work if it was given by mouth. When we had done that and knew that it really worked, the next jump was, well, what else could you use it for? And the answer to that, of course, turned out to be its widest application: birth control.

'In your novel, *Cantor's Dilemma* you give a picture of science which is very competitive. The image is one of a scientist driven by the desire for personal success and not terribly interested in the science itself. First of all, do you think that's an accurate description of the novel?'

It is not. The first part is quite correct, about the drive and so on, but I don't think it correct to say that its protagonist, Professor Cantor, was not particularly interested in science itself. This may be a reflection of the way I wrote the novel. Instead of writing about science itself, which many people such as science journalists do very well, I wanted to do what very few authors do, and that was write about the behaviour and culture of scientists. And because that's what I wanted to emphasize, I underplayed the actual science that Cantor and his student were doing. But I'll tell you this: the drive of the scientist—or, rather, of the super-scientist because I'm not talking about all scientists here, only about the élite—that drive to succeed means that the élite is necessarily blemished. And I put myself in this category—I'm not saying this just about other people. But we should not be ashamed of this because the urge to compete is also the fuel, I think, that really leads to many of these achievements.

'You mean the ambition is also a blemish?'

Absolutely, in the context in which you meant it, namely, why not do science for science's sake? I do not believe that there are very many scientists who, if they were very honest, would say they only do it for its own sake. Because if that really were the case, you could say 'all right, but publish anonymously'. Very few would; not for the salaries that they're getting. That is actually the main topic of my second novel, *The Bourbaki Gambit.*

'You also give an impression in *Cantor's Dilemma* that great ideas, or important ideas, come almost by the "eureka" effect; that you're sitting quietly and suddenly you see it. Do you think that's true?'

Great discoveries, real intellectual conceptual breakthroughs, do have that moment of clarity, yes. Now, most of scientific advances do not occur that way, but remember that I'm talking about super-scientists, and really sensational discoveries. The DNA structure, for example. There must have been a 'eureka' moment in the life of Watson and Crick.

'I think I take a slightly contrary view because there is the story of Newton who, when asked how he arrived at the theory of gravity, said "By thinking about it continually." And so I'm always a little worried that the cry of "eureka" conceals the enormous amount of work that has to precede it.'

I don't argue with you at all on that, I really don't.

'Another feature in the novel is that you give a feeling of the greyness of scientific observation.'

Well, that really has become a preoccupation in my present teaching. I really believe that the colour we use least in our metaphors is grey. In our political metaphors, we talk about the 'greens', we talk about the 'brown shirts', we talk about the 'blacks', the 'reds', we rarely talk about grey, except, perhaps, when referring to old people. Yet grey in my opinion is *the* politically realistic colour of this century. All really major problems that we have to face are grey problems, none of their solutions are black and white solutions, they are all grey. We do not want to listen to grey questions. We do not want to hear grey answers. And that is what I want to talk about in a way.

'Does your novel really reflect your own experience of science?'

It reflects it in a number of ways. First, of course, the drive for the Nobel prize. I've been a foreign member of the Royal Swedish Academy of Sciences for many years, and the only privilege I have as a foreign member is to be able to nominate people for the Nobel prize every year. One cannot vote for them as a foreign member, but one can nominate them. So I do know what is involved, and how people really count on it and use it. So in that context the book is realistic. It is basically about noble science and Nobel lust.

Another message in the book is the importance of the mentor–disciple relationship, and how the disciple really learns almost everything from the scientific behaviour of his mentor. And I purposely say 'his' right now because it's still very male oriented in that context. The learning comes about through a form of osmosis, which also works both ways. The mentor who has a star student really has the feeling that he has a son. Hopefully the day is coming when we have more mothers and daughters. There is an umbilical cord in this situation that is stronger than in almost any other situation where you have mentors and disciples; an umbilical cord that stretches but hardly ever gets cut even when the mentor dies.

I think the only event in my novel that I have not experienced personally is being involved directly in a question of possible fraud. That has not happened to me, thank God, in my own work. But I would say, 'there but for the grace of God go I', because the sequence of events described in the book and the way things have happened on a number of occasions, even quite recently, could happen to almost anyone and we'd better be aware of that. I think in the United States, particularly, the question of fraud—and I'm now talking about real fraud rather than the grey area of massaging results or something like that—is, in my opinion, very rare, a real aberration. But I think we should realize that we do take shortcuts, that we misbehave in this regard once in a while, though people have assumed that scientists are totally unblemished in this context. That is absurd—no group is perfect.

'How was your novel received by your colleagues?'

You hear me humming and hawing, because the reaction was mixed. I would say first of all that quite a number of my truly intimate colleagues probably never read it, which shows that chemists, in particular, just don't read fiction. Nevertheless a lot of scientists have read it and there were some people who really felt that I was washing dirty lab coats in public. I really felt that a lab coat is only a lab coat when it gets dirty. In that case there's nothing wrong with doing the laundry in public and people really should know about the dirt. I didn't feel that I really maligned our discipline or anything like that, so I consider myself not guilty of that charge. It's also interesting that *Cantor's Dilemma* was reviewed very widely in the States both in the scientific literature and general press. On the whole the reviews have been very complimentary, but a few scientific ones, in fact right here in England, were much more pricklish. But that's fine, because I feel that I've touched a raw nerve and in parts I wanted to do that.

'You have many honours. Do you think the honours system is good for science?'

Yes and no. The system goes some way towards satisfying an unsatisfiable drive. I have yet to meet a scientist who thinks he's gotten enough honours. Even those people who have won a Nobel prize would like to win a second Nobel prize and maybe some of them would like to have a third one, though no one has gotten three of them so far. I would say in some respects, yes, honours are a good thing. But in other respects, no, because we overemphasize the importance of honours. Many people who have not gotten them deserve them just as much, but are not really treated as well as they should be. Someone, I forget who, said quite correctly: 'The Nobel prize is very good for science, but terrible for scientists.'

'Winning a Nobel prize, of course, is a very potent prospect. Have you been disappointed that you haven't had one?'

Rather than as a 'yes' or 'no', I prefer to answer that in a slightly more complicated way. To me the Nobel prize, as it is to everyone else, is the paradigm of the ultimate scientific honour, and most scientists who have made some very important discoveries or have worked at the cutting edge of science probably dream at some stage or other that it may be very nice to have won a Nobel prize. Now there is no question that I belong to that group of dreamers. I would be a liar if I'd said 'no'. On the other hand, I also know that there are many more people who deserve a Nobel prize than could possibly get it and I guess I'm probably in that group. It is not really one of the big tragedies of my life, because I have won a number of other awards. But again, like most other scientists, I would like to get just one more, it's a never-ending thing. On the other hand, I am now moving into literature, so I would be just as pleased to get a literature award here or there. In fact it would probably please me more than a considerable number of larger scientific awards, excluding the Nobel prize of course.

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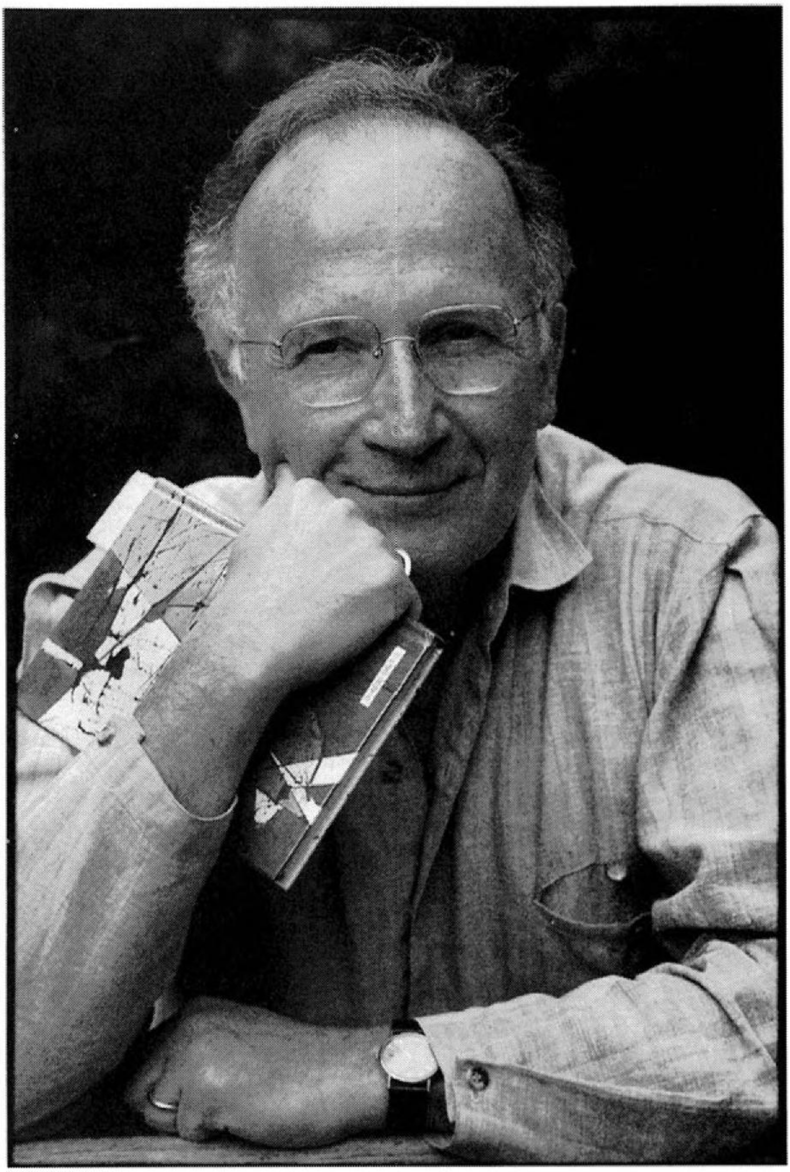

ROALD HOFFMANN
was born in 1937 and is John A. Newman Professor of Physical Science at Cornell University, New York.

CHAPTER 3

Tapeworm quadrilles

Roald Hoffmann

Theoretical chemist

THE notion of reductionism is essential to modern science. It is the belief that the properties of everything, including the brain, can ultimately be explained in terms of physics.

It is surprising, therefore, to find that Roald Hoffmann describes himself as an anti-reductionist. He and his collaborator, Robert B. Woodward, did after all win the Nobel prize for applying quantum mechanics to chemistry. It is also surprising, though perhaps it shouldn't be, that a distinguished chemist is also an established poet. Hoffmann's two volumes of verse, both published in the past few years, have received considerable acclaim.

I set out to ask him about both his chemistry and his poetry. He is a theoretical chemist rather than one who cooks up things in the lab, and I wanted to know what that actually means. I also wanted to know how doing chemistry compares with writing poetry, do the two activities have anything in common? First, however, the beginnings of both. Hoffmann was born in Poland in 1937. How did he get into his chosen fields?

I could be a good piece of propaganda for the immigrant ethic of what the United States does for people. 1937 was not a particularly good time to be born Jewish in Poland. The family was very happy, but around us there were gathering storms, and we had a very difficult time during the war. My father and many others were killed by the Nazis. Eventually we got out of Poland in '46 and came to the United States in '49, after a number of years as refugees in Europe. So, I'm one of the last generation of Hitler's gifts to America! I got into one of these marvellous science-oriented high schools in New York City, mine was Stuyvesant High School, another one is Bronx High School for Science; many scientists have gone to these élite, yet state, schools. I wanted to do mathematics, but just one year in high school showed me there were people who were better at mathematics than I was, and at the end of high school I guess I was to some extent sure of science, but certainly not committed to chemistry at all. I had done my share of chemistry experiments at home when I was a teenager, but I can't say there was an abiding urge to become a chemist. Even when I was at Harvard in my first year of graduate school I sat in on courses in astronomy (planetary atmospheres, I remember), and a course of science policy. And I applied to a summer programme in archaeology to do some excavations in Turkey. I think all of this is evidence that I wasn't sure what I wanted to do.

'You were already a chemist at graduate school?'

Yes, I was at graduate school in chemistry. I had made that decision but I wasn't that sure that that's what I really wanted to do. I think it's just by a hair that I became a chemist, I could have become, well, something else.

'Yet you speak in your writing about your love for chemistry and how beautiful it is.'

Yes, I love it. I like the subject, its richness, its position in between, its compromise between simplicity and complexity. Chemistry is positioned between the simple world of atoms and the complex nature of biological molecules and real materials. This is something I like. Because I think in science most scientists are pulled to the simple, they simplify the world around them. They set up a box of those problems which admit of a unique solution, of a resolution, and then they do very well at solving those problems. And then there is complex reality, and a coming to terms with the world as it is around us, which is awfully complicated. I like that, I think chemistry is nicely positioned in the middle. You have to talk about molecules that are simple, and you have to talk about haemoglobin, something that looks messy, like a clump of pasta that congealed from primordial soup, like a tapeworm quadrille. And you can't run a body with hydrogen and 92 elements, you need molecules, you need thousands, millions of molecules, you need complexity. And then, on the other hand, there's the human mind which always tries to reduce to simplicity.

'But you're a theoretician?'

Yes.

'But you did do experiments?'

Not many. The first two papers I published are experimental papers, one on thermochemistry of cement, another one on some radiochemistry. They're essentially juvenilia, they come from things I did when I was at college and summer jobs. I'm a theoretician but I'm very close to experiment. I take my inspiration from experiment, I try to explain things that other people have found, and then I build general theoretical frameworks, but I'm always very close to experiment.

'I need to understand the relationship between the theoretician and the experimentalist. Is there a tension there?'

I think theory and experiment are always in contention, there is a love and hate relationship between the two. The experimentalists say that theorists build castles in the sky, they don't pay any attention to reality; the theorists say the experiments aren't the right kind of experiments to test their theories. Then we also have a framework of reductionism which values theory in excess, makes us believe that understanding is theoretical. And this occurs in every field of human endeavour, not just in science. We have lines drawn between people who build fancy economic models, very mathematical, and people who deal with the realities of the stock market. We have critics and writers in English departments. But the truth is that they desperately need each other, which is why they are in contention, of course. Theory and experiment in chemistry are in such a love–hate relationship too. Most theorists are in a business of rationalizing, that's the most kind way of saying it, things that other people have found experimentally. Now, I like to play the game a little bit in another way. I think that a theoretician can get out of the situation where he or she is just helping an experimentalist by making slightly unreasonable predictions. The operative word is 'slightly' here. Very unreasonable predictions that take ten years' work on the part of somebody will never be tested. 'Slightly' is defined probably on a time scale of a graduate student's existence in graduate school. Anything that takes one to three years to do, a research director is likely to devote the effort. I think sometimes that I'm very good at making such slightly unreasonable predictions so that people test them. I love that interaction, with experiment; this is very close to the heart of what I do and what motivates me.

'Are these theories of yours general, like a physical theory, a theory of everything, or is it that you take many different chemical reactions and provide a theory for each of them?'

I take many different chemical reactions. I am a firm believer of moving from specifics to generalities, not of building general theories. First of all, the work that I do is almost amoeboid-like in character; I'm reaching out to various pieces of the chemical world. One day it's copper gallium compounds, another day it's a molecule that's shaped like a triple helix built up from tetrahedra—there is one that's standing in a model on my desk here. Another time it's what happens to aluminum alkyls, compounds of aluminum falling apart on a metal surface, as they fall apart. I take these individual problems and they're like pseudopodia going out in a different direction. Underneath I have a theoretical framework which happens to be called molecular orbital theory, of how electrons move in molecules. I have a way of looking at my molecules. I have also a conviction that everything in the world is connected to everything else, and if I send out enough of these pseudopodia they'll merge into something which will be an understanding of all of it. By going off in different directions I guarantee for myself that I am not locked in on one set of compounds, that I am forced to see the relationships between different ones. I think the beauty is in the complexity of nature. So I come back to something I said before, that beauty is in the reality of what's out there, residing at the tense edge where simplicity and complexity contend.

'It's a slight paradox because I have a feeling, both from what you've said and from your writing, that you're somewhat anti-reductionist.'

Yes.

'Yet your very work by applying quantum mechanics to chemistry is what any naïve person like myself would call pure reductionism.'

That's right, there is a paradox in this. I am a theorist, I'm in a business of providing explanations, my tool is quantum mechanics, a method of physics applied to chemistry, but the way I use quantum mechanics would drive most physicists crazy.

'Are you against reductionism in general?'

Oh yes. I think reductionism is unrealistic, it's just an ideology that science has bought. I think understanding comes in two types, horizontal and vertical. Vertical understanding is reductionist understanding, or analysis. Horizontal understanding is the understanding of a concept in a field in terms of concepts of equal complexity, of equal categories. Let me give an example of a *reductio ad absurdum* example which you need not accept. If someone sends you a poem, let's say, the phrase from T. S. Eliot's *Murder in the Cathedral*, 'The last temptation is the greatest treason; to do the right deed for the wrong reason.' Let's say you get that in the mail, an unsigned poem. To ask what is the sequence of firing of neurones in somebody's mind when they read that poem, and the biochemical actions behind it, and then the wondrous chemistry and physics behind that; knowing that will get you a lot of Nobel prizes. But it has nothing to do with understanding that poem—when it was written by Eliot, or read by the reader, or the person who sent it. That understanding is on a level of the English language and the psychology of the moment. That's horizontal understanding to me. What I'd like to say is that even within two fields as close to each other as chemistry and physics, embedded at the heart of science, that there are concepts in chemistry which are similarly not reducible to those in physics, or if they are so reduced they lose, just like the poem, everything that's interesting about them.

'That comes as quite a shock to me, but to turn to your analogy with poetry, you are an established poet, how did you become one?'

I didn't try to write one until I was about forty or so. I always thought it's an interesting way to try to understand the universe around us. And I then tried writing. I wrote in a vacuum for myself, I made a mistake, I should have taken a course. I was too ashamed to take a course because when you're forty and an established professor, you don't do that. Or so I thought. So I wrote poems. I sent them out and they came back with rejection slips. These are not like referees' reports on a scientific paper: nothing comes back, just a piece of paper saying 'no', and you can paper your bathroom with such pieces of paper because the acceptance rate of poems is much lower than it is for science. Scientists