



GUNTHER STENT

Born in 1924 and is Professor of Molecular Biology in the University of California at Berkeley.

GUNTHER STENT
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CHAPTER 9
TELLING NATURE

GUNTHER STENT
Molecular biologist

GUNTHER STENT has a reputation for holding somewhat heretical and certainly provocative views about science. A great friend of his told me 'Gunther is almost always wrong, but he is always interesting'. Although a leading molecular biologist he is unusual in being, in a sense, anti-reductionist. He believes that certain problems in biology, such as how the brain works, are intrinsically insoluble and cannot ultimately be reduced to explanations at the molecular level. He also does not believe—contrary to the accepted view—that the DNA contained within the egg provides the programme for the development of the embryo. He argues that the genetic material is just one of the components in a complex interacting system, and sees embryonic development in similar terms to that of a coral island: the result of a large number of predictable interactions that nevertheless give rise to a quite ordered structure. In his book *Paradoxes of Progress*, he argues that science is running out: that soon all the problems capable of solution will be solved.

• His pursuit of this argument has led him into the field of hermeneutics, the theory and study of how we should interpret such things as the scriptures or the findings of science. He now supports his thesis with the contention that the paradigms of science are heavily influenced by the essentially theological traditions in which they operate. In this analysis the science of the west is a very different activity to the science of the east, and reductionism is, at least in part, a subjective, cultural phenomenon.

• In a similarly heretical vein, Stent has also argued that style in science is as important in determining outcome as content. If, he suggests, Watson and Crick had not discovered the structure of DNA the way they did, or been the kinds of personality they were, and the information had, instead, emerged in a cautious, piecemeal fashion, the impact would have been very much less and the course of science altered.

¶ Much of Stent's work in molecular biology was done with Max Delbruck's phage group at the California Institute of Technology. Delbruck's influence, not just on Stent, but on the whole field of molecular biology has been profound. Phage are viruses—little more than sequences of genetic material—which invade bacteria, and direct their unfortunate hosts to begin synthesizing viral proteins. Delbruck, who had initially trained as a physicist specializing in quantum mechanics, was one of the first theoretical physicists to see the possibilities opening up in biology. He recognized that the simplicity of phage made them an ideal system for studying the action of genes at the molecular level. He described them as 'a fine playground for serious children who ask ambitious questions', and it was using this system that he was able to establish that DNA was the genetic material. Each summer in the 1940s, he ran an enormously influential course on phage at the Coldspring Harbour Laboratories outside New York. Among those it attracted were people such as Jim Watson and Seymour Benzer who were to shape the new field of molecular biology. Together with his collaborators Salvador Luria and Alfred Hershey he received a Nobel Prize in 1969.

¶ Stent, like Brenner, now works on higher organisms. He studies the development of the leech, and in particular the nervous system. Given what seem to be his rather gloomy views about science, one of the first things I wanted to ask him was why he did it all. I was also curious to know how his thoughts on the way science operates would affect his view of a figure such as Max Delbruck. And as Stent, like Delbruck, Crick, and a number of other molecular biologists, began his career in the physical sciences, I wanted to find out what made him change direction at what was to be exactly the right time.

Stent: - //

When I was a teenager my main interest was railways. I was living in Germany, in Berlin, and all my free time was spent at the Berlin railway stations watching the trains go by. They were my main interest in life. So my plan was to become either something like a railway engineer or a civil engineer. Then I emigrated to Chicago, finished High School there, and went on to University. I decided to study engineering. But somehow, for reasons that I can no longer recall, I signed up for chemical engineering, not for civil engineering, so I studied chemical engineering for one year at the University of Illinois. But I didn't like the drawing.

We had to do engineering drawing, and I was very poor at that. So I decided to get out of engineering and I went into chemistry. So in my second year at the University I became a major in chemistry. Then I found I disliked organic chemistry very much. But I did like calculations, and somebody told me that *physical* chemistry was where you did calculations. So I became a physical chemist and I finally got my degree. My undergraduate degree is in essentially physical chemistry.

'Had you any idea what you were going to do with it then?'

Well, not exactly. I knew that there was such a thing as Graduate School, but this was all very vague in my mind. It was during the war, and I had a job during my senior year on the synthetic rubber research programme, which was then run by the Office of War Production at Illinois. They offered me the opportunity to do a Ph.D. in physical chemistry while working on rubber and that's what I did. So I became a high polymer chemist working on the physical chemistry of rubber, polymerization and so on.

'You're not in biology yet!'

No, no. I got into biology after my Ph.D. There was a woman that I knew at Illinois, Martha Bailer, who was one of the earliest American electron-microscopists. They actually had an electron microscope! This was in the early forties. She knew some biology because she had taken Delbruck's first phage course at Coldspring Harbour in 1945. She gave me Schrodinger's book *What is Life?* to read, and said, 'He's talking about this man I know. He's talking about Delbruck'. And I was absolutely fascinated by this book, and I began to formulate a plan because I was very bored with physical chemistry.

'Why didn't you like it?'

Because the spirit of research, at least at Illinois, was that all the important things in physical chemistry had been done at the turn of the century and you had to discover something left that hadn't been done yet. That was the art of research! Then you had to do it, publish it, and then find something else that hadn't been done. And, although at the time I couldn't formulate it precisely, I found this very unsatisfactory. Then I read about the gene! I had never taken a course in biology, and the only time I had actually heard about genes was as an undergraduate.

when I took a philosophy course. It was on the great minds of modern times and was about Freud, Darwin, Marx, and Hume. And it was in the course on Darwin that I for the first time heard the word 'gene'. So, my acquaintance with genetics was completely confined to this one philosophy course. Anyway, I left Illinois for a year to go to Germany to work for the military government, and while I was there I reflected more and decided that I didn't want to stay in chemistry. So I went back to Illinois to finish my Ph.D. on rubber and during that year I wrote to Delbruck asking him to take me on. Delbruck, when I first heard of him, had been in Tennessee and wasn't too keen to go to Tennessee. But when I came back from Germany, in 1947, my friend Martha told me that he had become a Professor at Caltech. My dream had been to go to California in any case, so I decided I'd like to go to work with him. So I wrote 'Dear Professor Delbruck, I want to go into biophysics, do you have a place for someone like me?' He sent me back a postcard—Delbruck then only corresponded with postcards—and the answer was 'No, I don't have any place for anybody like you.' So that ended my career in biology at the time.

'That's quite a devastating reply. Were you devastated?'

I was completely crushed, yes. Because it had already taken a lot of nerve to write to him in the first place. But then a friend of mine said that the Merck chemical company had just established a new kind of fellowship, a post-doctoral fellowship. Now, post-doctoral fellowships like this were very unusual at that time and this was specifically for people who had been trained in chemistry to go into biology. So this friend of mine told me 'That's what you want to do. Why don't you apply?' So why not? I applied for the fellowship, saying that I wanted to go and work with Delbruck. And about six months later I suddenly got a telegram: please come to New York next week, for an interview. That was the first time I took a trip at somebody else's expense, my first taste of free travel! But this interview was absolutely devastating. It was terrible. They asked me: 'You know you want to go into biology. What do you want to do?' So, I said I wanted to test whether the second law of thermodynamics applies to living systems. They were all shaking their heads and after the interview I was dismissed summarily. I went back to Illinois totally depressed. Two days later I got a telegram saying that I'd got the fellowship. When I got this telegram I wrote to Delbruck and

said, 'Hello you probably don't remember me any more, but I have this fellowship to come and work with you.' And Delbruck replied that yes, that was fine and so forth, and said 'Undoubtedly you want to work on phage.' So I said, 'Yes, I want to work on phage.' I didn't even know what it was. So, my life changed completely after that, because I had no more contact with any of the people I knew before. It was like being reborn, like being a born-again Christian. I was 24.

'Did your family or anyone disapprove? What did your friends feel about this change?'

None of my family were academics. My father thought chemistry, biology, the whole thing was a waste of time anyway—I should be in a business! So it seemed quite irrelevant what I was doing. But I think most of my friends at Illinois thought I was out of my mind too, because the usual thing for all my colleagues was to go into Dupont or Monsanto. Most of the chemical industry in the United States was then run by graduates of this chemistry department at Illinois. It was immensely powerful. Very few people went into teaching positions and to go into *biology* was considered completely nutty.

'So it was very exciting, being in this new environment?'

Yes. Delbruck told me 'You're going to take the phage course at Coldspring Harbour.' So I said 'Yes sir, I'll certainly do that.' And there it was immediately obvious that research was something entirely different. There were a million problems, and the art of research was to pick which problems from the million of unsolved ones, rather than having to find something that hadn't been done yet. So this was like being let loose in a candy store where you can eat all you like. And that summer at Coldspring Harbour people like Jim Watson and Seymour Benzer were my classmates. These people were all of a quality of individual totally unlike anything I'd known at Illinois.

'Can I just ask you about Delbruck? Do you think important figures, like Delbruck, really shaped the whole development of molecular biology?'

Oh yes. Delbruck's influence in molecular biology is very curious in that he was a kind of Gandhi figure really. His strength was his incorruptibility. He was not a leader in the sense that he actually made

good suggestions or that his intuition was especially good. It wasn't bad, but his role was essentially *moral*. Everyone's intention or aim was to please him. Prizes, or recognition by others, were only secondary. The main thing was, when you did something, you were hoping that Max would approve of it. And therefore things like stealing or competition didn't exist because Max would see through them. It would be like God, you see. If you did something illegal, maybe you'd get away with it with a fellow mortal, but God would know that you'd cheated.

'And did everyone feel the same way about Delbruck?'

All of the group, the so-called phage group, felt that way. So it created order. Now the point was that very often Max's opinions were incorrect, and so it was also a test. You often had to do something against his views, but he would respect it. You would say 'I want to do this', and he'd say 'It's nonsense. It will never show anything.' But one did it anyway, and if you could then show him good results, then he would of course immediately honour them. He would feel even better that against his advice, you had prevailed. But he was always the standard of integrity, and that is what made this movement possible. And the people whom he thoroughly disapproved of were just out. Of course, if the leader is bad then of course it could be bad for the field. It's not without risks. But I think it's probably essential that there is a leader. And so the field that you and I are interested in, development, is partly in a bad state because there is no ideology, no leader who is setting the tone, who is some kind of court of appeal.

'You were now within the court, as it were, of Max Delbruck, and you found science satisfying in a way you hadn't found before?'

I would say I already found science itself satisfying at Illinois, even as a physical chemist. Because what I liked most was being 'a scientist'. My interest, strangely enough, in science itself, is not all that deep. My main interest when I got into science was to be a scientist. I liked that as a lifestyle.

'I want to know what that means.'

Well, the line of work—to be in a lab. To work in a lab is to have friends with whom you discuss problems of mutual interest. Then you discover that you can travel as a scientist, and go all over the world. You have

friends everywhere. So, I like the social aspects. I consider that one of the tremendous satisfactions of being in science. And when I find something—the rare times you do find something new, which isn't very often—I would say my main satisfaction in discovery is the thought that next time I go to a meeting I really will have something to say.

'It's not an intellectual gratification then?'

No, the intellectual gratification is much less than the expected reward that I'll have when I see my buddies the next time, and tell them 'Look here man, I found this!' This is what I like best about science. I'm always thinking about the papers you see. Even before I have found something, I'm already thinking of the opening phrase of the paper in which I will describe this discovery.

'You're already thinking of the title, and the opening phrase?'

Yes, before I've found it, yes.

'And publishing the paper gives you great pleasure?'

That's right, yes. And, you see, many of my collaborators, although they're excellent men, they've hardly published. It's always a tremendous struggle to get them to publish papers and so on. I can't understand that. So there's some kind of gap between my dear collaborators and myself. Because if I didn't push them, they wouldn't publish for years.

'And the gratification of publishing is really so that other people will see your work—it's a sort of an ego-trip?'

I don't want to deny that. The expectation of admiration probably is there too, but I think it's not only that. I think it is just the pleasure. I like conversation very much, and to make conversation you have to have something to say. And so it's partly that, I think.

'And so the paper is almost like a way of initiating the conversation?'

That's right, yes. It's a bit like a novelist. I imagine with a novelist there's probably an ego-trip, that he would like to be famous and have his novel admired. But I think that's not all of it. He has some urge to tell what he knows.

'But what about discovery? You've said that the gratification really lies

in having something interesting and new to tell people, but isn't the actual solving—the moment of solving the problem—not an exciting moment for you?

Of course it's exciting, but I think the final excitement, the real source of the gratification, is not so much beating nature as being able to tell it. Of course, if I have a theory, 99 per cent of the time the theory is wrong, but that 1 per cent of the time when I have a theory, and I get experiments that confirm it, then of course I'm very happy for it to turn out that way. But I think the happiness mainly derives from being able then to write a nice paper. Let's say, if I was on a desert island, Robinson Crusoe, I think I wouldn't do science.

'Because there'd be no one to tell it to?'

Exactly. If I was marooned on an island with a lab, I don't think I'd do any experiments.

'That's a very nice way of putting it. Now, I know that you're interested in the philosophy in the science. Do you think there's something called scientific method?'

Not that you can formulate. It's not the way I think. One can make some generalizations, but it's more an a posteriori justification . . .

'So how do you actually go about your own science? It's a widely held view, for example, amongst people who are not scientists—and even some scientists believe it—that the way to go about science is to have an absolutely open mind.'

No, that's complete nonsense. You can't. There's no such thing as an open mind. First of all, it's a psychological impossibility. But even if it were true, then you would be condemned to inactivity, so there's no such thing as an open mind. You have to have some kind of prejudices and approach the world with some kind of theoretical framework.

'I'm so glad to hear you say that. Scientists are very prejudiced and, it seems to me, that is what actually gives the dynamism to science. That's what scientific imagination is.'

Yes. The picture of the scientist as a man with an open mind, someone who weighs the evidence for and against, is a lot of baloney. Scientists

are studying the world, the outer world, and that presents us with an infinitude of phenomena, so you could not possibly address all things. You have to make abstractions. You must select a sub-set of phenomena to attend to. And this selection must, by necessity, be theory-guided. So by the very fact that you focus your attention on just some limited thing, you're prejudiced from the very beginning. This open mind business is a lot of nonsense.

'Do you think then, in that sense, that there is any analogy between art and science? There's a lot of slightly romantic discussion about the creativity of scientists being the same as the creativity of artists.'

I think there is some similarity. There's also a difference between art and science. The world that the artists address is the inner world, so the fundamental difference between a scientist and an artist is that the artists address the inner world and the emotions, whereas the scientists address the outer world of physical phenomena. But I think the similarity is the act of discovery. I don't believe that art is only interested in entertainment. It is similar to science in that it endeavours to discover truth. The artist endeavours to discover truth about the emotions, the inner world. It's not a question of tests or proof, but of validity, whether the experience seems valid to you or not. It's a subjective judgement. If you feel that reading Dostoevsky provides you with some insights, then either you feel that or you don't. There's no way of proving it, and there's actually no need to prove it.

'Is the process of discovery in art and science really very different?'

I don't think it's so different. If on the one hand you go to physics classes wondering 'Why does the sun rise, and go down?' and they tell you about gravity, and Galileo and Newton, then you feel good. At last you understand what's going on up there, right? I think this is not so different from when you read Dostoevsky's let's say, or when you gain insights from Shakespeare. You understand about people, what makes them do what they do. It enlarges your understanding of the world. So I think, in that sense, the act of discovery is not so different. But it's a different phase of the world.

'But you're talking about responding to science and responding to art. What about the actual process of doing art or science?'

That is very different, but the end result is still this psychological gratification.

'Is that why you've moved into this new field of hermeneutics?'

I have developed a side interest in philosophy. I'd already studied it as a student, and I was interested then, but my enthusiasm was really awakened when I worked in Japan. I was there in 1960 on a sabbatical. In the labs, there were people in white coats working with ultracentrifuges, phage, mutations and so forth, just like anywhere else. But I was amazed to discover that they were actually fundamentally different in their approach. Their view of what they were doing seemed to be quite different from mine. So, my interest in epistemology and philosophy was aroused by this personal experience of the radical difference between Japanese science and what I knew to be Western science.

'So what was the difference?'

It has to do with the notion of reality and truth. When I wanted to please Delbruck, I wanted to publish something that was true, since only that would please him. But what I found in Japan was a much more aesthetic notion. The element of truth was not paramount. For them, writing beautiful papers was very important, and the beauty of the paper was paramount rather than the truth. I first got onto this when I realized that the notion of a controlled experiment seemed to be foreign to the Japanese. They're mostly positive thinkers. To them a controlled experiment is negativism you see, like trying to tear things down. They don't like that. Also, during seminars, the type of questions that were asked would never be critical questions. At first I thought it was just politeness, because Japanese are very polite. But it actually has a much deeper philosophical and religious basis. It's Buddhism as opposed to Christianity. I think Western science depends on the notion of law and order. Historically, you can trace the development of this notion from that of an orderly universe created by God, who made the laws. Moreover, He created us in His image and therefore it is given to us to divine the reasons that He, in His infinite wisdom, had in designing the world. And so the whole enterprise of science, metaphysically—I'm speaking about the metaphysical basis of Western science—depends on this credence: God, the Creator, made the laws, created us in His image,

and therefore we dig for what His notions are. There's some chance of finding out, you see. Whereas, for the Buddhists, this context is considered to be the height of naivety. Because, for them, anybody who has any sense knows that the world is infinitely complex. So that's the radical difference. Because if you believe that there are no laws, and that there is no orderliness, therefore, there is also no truth. Thus it's all a question of subjectivity which is exactly what avant garde philosophers of science are saying now. But the Japanese people have felt this for centuries, for millenia, in their heart of hearts.

'But I've always believed in orderliness, simply because it's apparent to me that the world is orderly. I've always seen patterns in the world.

Regularity, of course, is a fact of experience which we see, and that we get in the cradle. But that is different from believing that this orderliness is, in fact, out there. We're taught that orderliness is a reflection of underlying laws which it is given to us to discover. That is not necessarily the same thing. That I think is somehow a reflection of a deistic belief. And I do believe that while the Buddhists were wrong in the short run, because it turns out that the world is more comprehensible than they thought, in the long run they were right. Now that we've pushed science to its limits, we see that they were right after all.

'Are you then not a reductionist? Do you not believe that all human behaviour can be simply reduced to molecular biology in the long run?

On the contrary, I believe that science is, by nature, reductionist, but I also believe that reductionism will not carry us all the way. One of the reasons why I think science will eventually peter out is because you must always explain some higher level in terms of some lower level—that's what scientists have to do. But I think that when finally we get to sufficiently complex things, this will not be possible. It is precisely because I think reductionism will have to fail, that I believe that science is coming to an end.