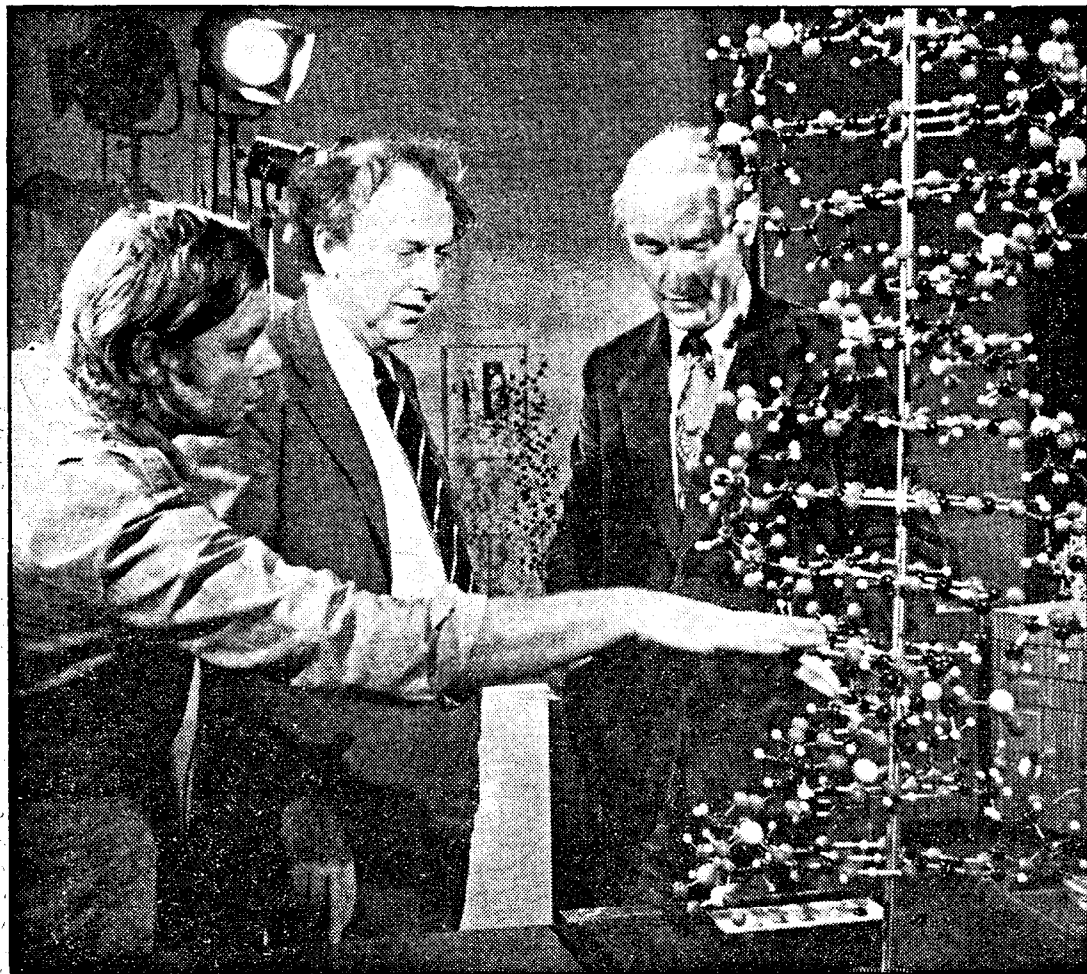


The Double Helix Revisited

—Francis Crick and James Watson talk to Paul Vaughan about their discovery of the molecular structure of DNA



VAUGHAN: James Watson and Francis Crick are two scientists who had the luck—or the ill-luck, depending on how you look at it—to achieve eminence early in their careers. In 1962 they were awarded the Nobel Prize for Medicine for their elucidation of the structure of DNA, deoxyribonucleic acid, or, as the work is usually described, for cracking the genetic code. Their discovery of the way the so-called 'life molecules' of DNA are built, and how they pass on genetic information, was immediately recognised as one of those crucial moments in scientific advance, comparable, it has been said, with Darwin's theory of evolution or Einstein's theory of relativity. When the work was done the two men's paths diverged. Then, in 1968, James Watson outraged some scientists, but delighted many more, by publishing an account of how the discovery was made. *The Double Helix* is probably the most efficient demolition job ever done on the ivory tower of academic science. Not only was it written in a way that non-specialists could easily follow, it also made it clear that all the people involved in the discovery were human individuals with

Francis Crick (centre) and James Watson (right) with the model of the structure of DNA which was used in the film

their share of human failings. In the book, as at the time of their discovery, the chief characters are, of course, Watson and Crick—two men in many ways so different: Watson, an American, slightly built and diffident, Crick, a large, confident, jovial Englishman—still, by the way, doing research at Cambridge, whereas Watson is now at Harvard. Not long ago Crick and Watson were together again, revisiting the streets, pubs and laboratories of 20 years before. This time it was to make a film about the now famous period of their collaboration. At the end of five not exactly restful days of filming I met them, and they talked about their work as we sat in the upstairs room of a pub not far from the Cavendish Laboratory in Cambridge. I've often had the rather strange feeling that a Nobel Prize, particularly when you win it so young, is a kind of scientific albatross for ever more. I asked if they'd found that it had had any adverse effect on their careers.

CRICK: Only with journalists.

WATSON: I don't think it's had any effect at all. There are odd occasions when one gets asked for an autograph, but I don't think we ever walk down the street thinking we've won a Nobel Prize.

CRICK: Scientists take a fairly detached view about this sort of thing. They know it's to some extent a lottery: they don't bother very much whether you've got a Nobel Prize or not. It's the layman. I much prefer to be introduced to people without them knowing that I've got a Nobel Prize, because otherwise they treat you as some sort of giraffe.

WATSON: They ask you: would you tell them about DNA? And I find I can't, or the question is just as difficult to answer 20 years from when it happened: it's always the same language problem.

CRICK: And you never know what sort of background they have. I remember, in the early days, one girl saying to me at a party: 'Scientists have created life, haven't they? So where do you go from there?' Nowadays all the kids know about it and people are rather more inclined to ask: 'What are you doing now?' Which is almost as difficult a question.

WATSON: It sounds as if we don't want to tell people what we're doing. It's just that, often, you go out and it's a moment of relaxation, and suddenly to be forced to think again and to phrase your answer seems like working.

CRICK: Besides, to tell a layman what you're doing is much more difficult even than to tell him what you've done, because at least when you've done it you hope it's clear and you can simplify it: while you're doing it, it's a mess, and it's complicated, and you're doing several things. You don't know which way it's going, so it's not an easy question to answer. So you give some broad answer. I say I'm interested in embryology or cell biology, and they look a little blank, and you try and think of some other topic as quickly as possible.

VAUGHAN: One of the things I was thinking of when I put the question was that perhaps some people might wonder if you were going to do the whole thing again: I don't mean the whole project all over again, but win yet another Nobel Prize, or do something equally brilliant.

CRICK: My own view is that work like the discovery of DNA is not something that it's reasonable to expect anybody to repeat. You can do quite good things and so forth, but this was of such a nature, and had such a dramatic impact, that it would be foolish to go round trying to aim for anything like that. One just does what we did before, which was to look for the most interesting scientific problem which was tacklable in biology, and go ahead on that, and not worry about this sort of thing any more than we worried at the time. People don't normally get, nowadays, prizes for the same thing, because there's such an enormous queue: simply because the number of prizes has stayed the same and the number of scientists has increased, so that there's a whole queue of people who really deserve Nobel Prizes, but they can't get through them fast enough. The only thing you can

aspire to, of course, is getting a different prize, just as Linus Pauling got a peace prize. I suppose, Jim, you're hoping to get a literary prize?

WATSON: I need one more book about you, Francis.

VAUGHAN: Do you find, then, on the whole, that it hasn't had any sort of warping effect on your careers?

WATSON: Well, some people might say that it has warped both of us terribly, but I don't think Francis is any different from when I first knew him.

CRICK: No, I think what has a warping effect on your career, I'm sorry to say, is age. It's not prizes. That's the variable you have to allow for, and the fact that you have people—this happens not merely to Nobel Laureates—wanting you to do more things: there are more distractions. They want you to go to more meetings, they want you to review books, to talk on the radio.

VAUGHAN: You become a kind of scientific diplomat or something, don't you?

CRICK: Well, you can do, but there's no reason why you should: there are plenty of people who haven't. Dirac, for example, is probably one of the most brilliant Nobel Laureates, and he's always kept himself very much a private person—wouldn't join in the atomic bomb, hardly took research students, and so on. You have a choice: some people enjoy it, they like all the ceremony and the power that they get, and the administrative thing, the feeling that they're directing large units; other people don't. It's a matter of temperament. It's rather like asking what happens if somebody wins a football pool. Some people are ruined by it and other people behave sensibly. It depends on their character.

VAUGHAN: I think I'm right in saying that your work is unique in the annals of Nobel Prizes in that its value was more or less immediately recognised? There was no question of any time-lag.

CRICK: The business of the physicists about the violation of parity was, in fact, recognised, I would say, rather more quickly.

WATSON: Physics was a more high-powered subject and there was a real conceptual framework, and they saw what it meant. Biology is so diffuse.

CRICK: We never had any opposition, but it took some time to penetrate, although people find that difficult to realise nowadays. It hit very strongly a small number of people, but the large number of scientists and biologists and biochemists that it affects today were influenced by it more slowly, and it wasn't till, say, Kornberg's work on the enzyme which replicates DNA, and various other technical things, that it gradually gathered momentum. In the case of parity, one experiment was enough to put the idea over.

VAUGHAN: Are there any major directions that science has taken which are a result of what you did?

WATSON: The working out of the whole pathway of RNA synthesis, protein synthesis, the cracking of the genetic code, the total conception of how a virus can multiply—none of this research would have been possible without starting off with the structure of the genetic material. If you pick

up biological journals and ask what percentage of the biology being done today is a direct product of what we were doing, it's maybe 25 per cent.

VAUGHAN: What effect, if any, has this had on the sort of things doctors can do for patients?

WATSON: Very little, so far. There's some anti-cancer research which involves nucleic acid analogues: it will probably be a little faster due to knowing the base-pairing rules.

VAUGHAN: Dr Watson, your book, *The Double Helix*, has been regarded as almost as much of a break-through, in terms of writing about science, as the work you did in molecular biology.

WATSON: Oh I think that's a lot of nonsense. The science was a particular discovery, with a unique place in the history of science. My book is only a—

VAUGHAN: Somebody said to me not long ago that he thought you were two great men till he read the book.

CRICK: I think, Jim, your book is unique in the sense that there wasn't a book quite like it until you wrote it.

VAUGHAN: Why did you write it?

WATSON: Well, it's actually rather an interesting story. It just wasn't the way people would ordinarily think we did it. I got bored with people referring to me as a genius, or something like that. I know what I am. So I just wanted to put the whole thing in perspective.

CRICK: But the way you put it in perspective, Jim, made it seem a little bit easier than it was. What you were trying to do at the time was to make out that we weren't cold characters in white coats, that we were human, and this you did admirably. But owing to the fact that you wanted to make it readable to people who didn't understand the technical side, a lot of the technical bits were left out, so that it sounded as if anybody could have done it.

WATSON: If you want to take the 18-month interval between when I arrived in Cambridge and when we got the structure, what percentage of our actual working days was spent thinking of DNA? I do not think you could come up with more than three months' work, probably less.

CRICK: I wasn't saying you could, but I don't think that matters: I mean, the moment of conception is often brief.

WATSON: Most of the time we were doing something else: enormous periods of just drinking coffee or taking walks, wondering why we couldn't think of the right answer. There were long intervals during which we were stuck and during which the important thing was that you had enough sense to stop thinking about it, so that you didn't get totally frustrated. There were long intervals when we couldn't do anything. In the final six weeks the whole thing went through very fast, but before that there were long periods when you and I were thinking about different things. That was one of the things I wanted to put across.

VAUGHAN: It comes across. In fact, you convey the sense of, now and again, rather boring normality and the monotony of the sort of work you have to do.

WATSON: Yes, there's a large amount of monotony. Sometimes we actually enjoy do-

ing it. In fact, most of the time we enjoy doing it: it's a sort of nice steady task.

CRICK: Experimental work, I reckon, is occupational therapy. Scientists have a nice technique they can use: they come in in the morning, they go to it. It keeps them comfortable; it keeps the neuroses at bay. Theoreticians are a bit the same: they have some technique they like using, and they like using it over and over again.

WATSON: A lot of people said that it didn't look as if I was interested in science. It's a very hard thing to get across, our interest in science, and I thought it would be best not to put that across because it would be corny. It might even be true, but it would be unreadable.

VAUGHAN: Looking back on your research, do you feel that, compared with now, you were starved of funds and attention from the scientific community?

CRICK: We certainly weren't starved of funds. The Medical Research Council supported us, although we were very much a risk for them. They couldn't really see what was coming out. They supported us on a very adequate scale and they would occasionally come round and say: look, you've got too few assistants, we think you ought to have one or two more. One very great advantage was that we didn't have things distracting us, and when we had something, people did pay attention to it. The difference now is that everything's on a much bigger scale.

WATSON: The labs at King's and Cambridge were probably the two best-equipped in the world for molecular structure analysis, so work was not done under an aura of financial hardship. No one painted the walls, the salaries weren't high, people weren't driving cars: but for the scientific problems that one wished to solve at that time, one had enough money.

VAUGHAN: Would you say that scientific research is now, if anything, over-funded?

CRICK: Essentially there's been a large increase in the number of very able trained scientists, especially younger ones. That means you need more money. The other thing is that there's been more equipment, and the automatic equipment is actually more expensive. Therefore, any particular scientist needs more money nowadays, so the total sum has become so large that it's not something that the people supplying the money can't notice. People are having to pay more attention to the money going to science, simply because there is more of it. But the problem isn't the money: the problem is that the results are coming in at such a rate that it's jolly difficult to keep up with them all.

VAUGHAN: Do you find that in the position you've reached you're expected to take a very firm and definite line on the question of scientists' moral responsibilities?

WATSON: In the days when they were attempting to ban atmospheric atomic tests, I put my name to several things, because it really seemed like you should do it.

CRICK: I don't like doing it, but that's a matter of temperament. I have occasionally done things of this sort, but I've got to the stage where I feel I don't want to moralise for other people, and I don't feel that, except in very special cases, I have the expert

knowledge which will allow me to do it. I feel the appeal to experts and big names for laying down this type of thing is often mistaken.

VAUGHAN: You have, in fact, Dr Watson, recently put your name to a statement deploring the inadequate attention devoted to heredity in things like social anthropology, sociology and social psychology.

WATSON: I made a statement that I thought the work of Edwards and Steptoe would be very important if they'd go through with it. The book has been so misinterpreted that I thought someone in America should say something. Most people are quite afraid of saying that science will have consequences, uncertain consequences: the scientific Establishment or governmental agencies never like to say this. I thought, therefore, I'd say it, and force the agencies to have a policy on it. For example, the National Science Foundation would like to forget about 'test-tube babies'. It's a very emotional issue and they hate to get involved in emotional issues—understandably.

CRICK: I think that if science were grossly misinterpreted, that would be one case. If scientific research was being restricted for non-scientific reasons, one would wonder about it. The difficulty isn't that. You see, taking a public stand is, no doubt, a nice thing for one's conscience, but the question is: by taking a public stand, do you do any good? One doesn't want to get into the position of always being the person who signs this sort of petition, and there has been at least one case where I was prepared to do something about a scientist in another country and where we were advised it would be better if we didn't do anything. These really come down to political issues, which, it so happens, I personally am not very interested or involved in. So it would have to be something rather extreme for me to want to do anything. When the war against Nazi Germany came along, I didn't have any doubts in my mind that I should join in the war effort, although I don't regard war as something which I would support in a general way. I would only feel moved if I regarded the thing as something rather extreme, and if I felt my intervention would make some really significant difference, and there aren't too many cases like that. I would like to see other scientists who like to be involved in the political process doing more, doing steady work: I particularly admire, for example, Professor Matthew Meselson in the States for what he's done on the question of biological and chemical weapons. But I don't like getting too much involved in that sort of thing. On the other hand, I suppose if I noticed that there was a really severe lack of scientists who were doing it, then I would feel a greater obligation than I do at the moment, when, in fact, the younger ones are prepared to take up issues of this sort. If I felt they were being persecuted, I would come to their support privately.

VAUGHAN: Looking back on it all, is there any part of the scenario that you would rewrite if you had the chance? Anything that you wish you hadn't done?

WATSON: Lived in Pasadena in the smog for two years.

CRICK: I can think of lots of things I wish I had done and discovered, can't you, Jim? I mean, that's all too embarrassing—the number of discoveries which one saw and had under one's nose and didn't take advantage of. That's quite a long list. As for things one did which one wishes to undo, or wishes one had done with a slightly different emphasis, well, you might have made things worse than they were.

WATSON: I think our lives have been very interesting, because the discovery did open up a whole new world. It's been a very exciting scientific period. It hasn't dried up: in fact, more phenomena exist than I actually like to read about. In the old days you had to search hard for something which really seemed pertinent. Now, if anything, one is overwhelmed with data.

CRICK: I've never thought this before until

you asked me the question, but I suppose I could have asked: shouldn't I have made an attempt to be more friendly with Rosalind Franklin, because, in fact, we did become quite friendly after the DNA structure. But, for all we know, that might have made things a lot worse, so I don't waste my time in regrets and worries of that sort. It's not my temperament. I prefer to look forward. I don't lie awake at night thinking: ah, what a pity we didn't do this. Somebody else did it, so let's move on to the next thing, because it's the science that's the interesting thing. One likes to contribute; one likes to do things oneself. But what one really likes is to see things moving and ideas that you and other people have had firming up and becoming facts, so you can go on to the next thing.

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