matologist, a pathologist, a pharmacologist, a veterinarian, an oncologist. . . . Even when there is an acknowledged nutritionist or two on the committee, they are usually basic scientists with expertise in highly specialized areas with no experience or obvious sensitivity in the clinical area of nutrition." He said it was time to change the "ivory tower" attitude that something as deceptively obvious and apparently simple to understand as nutrition was not worthy of research dollars.

McGovern took the criticism one step further. He complained that even when a project gets funded, it tends to be for an "old boy" who has many NIH grants, rather than for a young researcher with new ideas. Fredrickson later sent the subcommittee a report that took the bite out of this criticism. It showed that in the past decade, new investigators on all NIH grants rose from 8.9 percent to 13.7 percent. If renewal grants are left out, the picture gets even better. In 1978, for example, new investigators walked away with 51.6 percent of the new grants.

Taking another tack, Commoner said that secrecy was the root of the problem in peer review. "The way to get at mistakes is to make them public," he said. Then, in what Fredrickson later called a "somewhat paradoxical" act, Commoner pulled out a study section critique of a grant he had submitted and proceeded to read aloud the study section's criticism. Commoner's larger point was apparently that more nutrition research would get through the NIH mill if the deliberations

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were made public. Increased exposure, he said, would help reviewers to stop "nit-picking" and to do a better job. Fredrickson countered that "It is very difficult to discuss the track record of scientists, their course in the last few years, whether they have slipped, what progress has been made, at a public meeting at which the scientist himself may be present." He also noted that priority scores and a critique of each proposal was given to the researcher. And the

Crick Looks Back on DNA

As much, or perhaps as little, as 20 years separates the beginning of analytic biology from that of synthetic biology. 1953 was the year of the discovery of the structure of DNA; 1973 the moment when the recombinant DNA technique was first described. James Watson's account of the first discovery is well known. Francis Crick, whose biography has yet to appear, recently described his view of events in a lecture printed in the September issue of The Sciences, *from which the following excerpt is taken.*

I think what needs to be emphasized about the discovery of the double helix is that the path to the discovery was, scientifically speaking, fairly commonplace. What was important was not the way it was discovered but the object discovered—the structure of DNA itself. One can see this by comparing it with almost any other scientific discovery. Misleading data, false ideas, problems of personal interrelationships occur in much if not all scientific work. Consider, for example, the discovery of the basic structure of collagen. It will be found to have all these elements. The characters are just as colorful and diverse. The facts were just as confused and the false solutions just as misleading. Competition and friendliness also played a part in the story. Yet nobody has written even one book about "The Race for the Triple Helix." This is surely because, in a very real sense, collagen is not as important a molecule as DNA. . . .

But what was it like to live with the double helix? I think we realized almost immediately that we had stumbled onto something important. According to Jim, I went into the Eagle, the pub across the road where we lunched every day, and told everyone that we'd discovered the secret of life. Of that I have no recollection, but I do recall going home and telling my wife Odile that we seemed to have made a big discovery. Years later she told me that she hadn't believed a word of it. "You were always coming home and saying things like that," she said, "so naturally I thought nothing of it." W. L. Bragg, Cavendish professor, was in bed with 'flu at the time, but as soon as he saw the model and grasped the basic idea he was immediately enthusiastic. All past differences were forgiven and he became one of our strongest supporters. We had a constant stream of visitors, a contingent from Oxford which included Sydney Brenner among them, so that Jim soon began to tire of my repetitious enthusiasm. In fact at times he had cold feet, thinking that perhaps it was all a pipe dream, but the experimental data from King's College, when we finally saw them, were a great encouragement. By the summer, most of our doubts had vanished and we were able to take a long cool look at the structure, sorting out its accidental features (which were somewhat inaccurate) from its really fundamental properties, which time has shown to be correct.

For a number of years after that, things were fairly quiet. I named our house in Portugal Place "The Golden Helix" and eventually erected a simple brass helix on the front of it, though it was a single helix rather than a double one. It was supposed to symbolize not DNA but the basic idea of a helix. I called it golden in the same way that Apuleius called his story "The Golden Ass," meaning beautiful. People have often asked me whether I intend to gild it. So far we've got no further than painting it yellow.

Nowadays most people know what DNA is, or if they don't, they know it must be a dirty word, like "chemical" or "synthetic." Fortunately people who do recall that there are two characters called Watson and Crick are often not sure which is which. Many's the time I've been told by an enthusiastic admirer how much they enjoyed my book—meaning, of course, Jim's. By now I've learned that it's better not to try to explain. An even odder incident happened when Jim came back to work at Cambridge in 1955. I was going into the Cavendish one day and found myself walking with Neville Mott, the new Cavendish professor (Bragg had gone on to the Royal Institution in London). "I'd like to introduce you to Watson," I said, "since he's working in your lab." He looked at me in surprise. "Watson?" he said, "Watson? I thought your name was Watson-Crick."

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