

The MRC Unit for Molecular Biology

Next month this distinguished research group, responsible for striking advances in the study of proteins and genetic material, will move into a new building in Cambridge. Why has it been inadequately housed until now? The Director explains by recounting the eventful story of the Unit's first 15 years

by Dr M. F. Perutz, FRS

IN a recent issue (4 January) the *New Scientist* expressed surprise and indignation that this Unit, though widely known for its interesting work, should still be housed in a temporary hut, squeezed into a dingy courtyard between the Cavendish Laboratory and the Zoological Museum. This reminded me of a Russian delegation which arrived in Cambridge demanding to see the "Institute of Molecular Biology". When, duly flattered, I took them to the Hut, they asked, bewildered: "But where do you work in winter?" doubtless with visions of our corrugated Perspex skylights caving in under six feet of snow. They next wondered how we had planned the "Institute". As good socialists they imagined that we had planned to people it, like Noah's Ark, with two physicists, two chemists, two biologists, etc. They were surprised when I assured them that the Unit had just grown, like a tree.

The trouble in Cambridge is that there are already too many other trees, and that they cover all the available space. This is why we are still in the Hut and not, as this journal believed, for lack of support by the Medical Research Council or the University.

Actually, the Hut represents less than half the space we have. Our other rooms are scattered over the Cavendish and Zoological Laboratories, but they have served us well. Moreover, though shabbily housed, we are superbly equipped. Without this equipment, mostly bought for us by the MRC, our results could not have been attained. Next month we shall move into a spacious new laboratory. We are all very pleased, but the *New Scientist* wants to know why we did not get it sooner. To

explain the reason I shall have to recount some of the Unit's development.

The Unit came into being in 1947 with J. C. Kendrew and myself as its only members. At that time we both worked in the Crystallography Department of the Cavendish Laboratory under Sir Lawrence Bragg. He and David Keilin, the Professor of Biology, had been intensely interested in our work ever since I had shown them the first X-ray diffraction photographs of haemoglobin crystals, but in 1947 they were at a loss where to find permanent support for us. The University would not take us on. In this predicament Keilin suggested an approach to the MRC.

Bragg explained to the late Sir Edward Mellanby, the Council's Secretary at the time, that Kendrew and I were out on a treasure hunt with only the remotest chances of success, but that, if we did succeed, our results would provide an insight into the workings of life on the molecular scale. Even then, however, it would take a very long time before they would bring any direct benefit to medicine. Mellanby took the risk. This was a courageous decision, for he must have been prepared to face the blame for squandering the Council's limited resources on esoteric adventures of doubtful promise.

I still remember my thrill on discovering that the MRC would actually buy us equipment. Up till then it had been provided by the generosity of the Rockefeller Foundation, and I had had to argue with our parsimonious laboratory steward over every box of X-ray film. I had been in the laboratory for several years before he would actually let me buy a pair of scissors to cut it with.

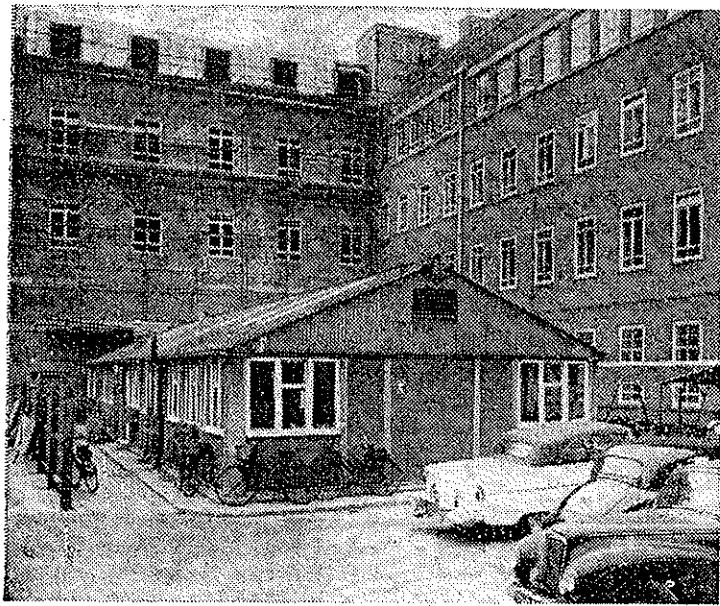


FIGURE 1 (left). The Hut, dwarfed by the buildings of the Cavendish Laboratory, was inherited from the Department of Metallurgy.

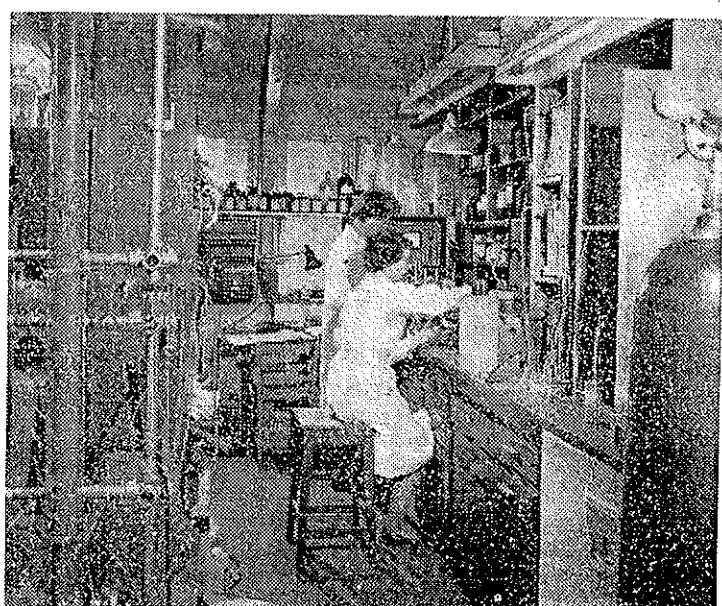


FIGURE 2 (right). Interior of the Hut, tightly packed with people and precious equipment.

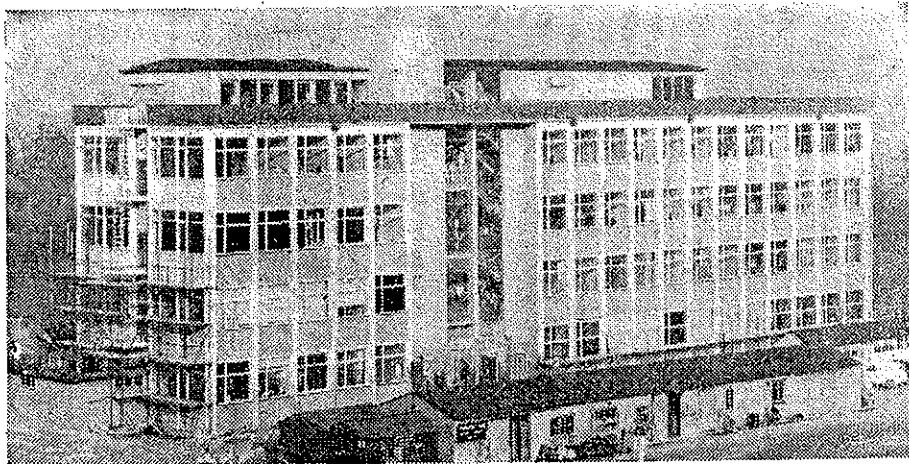


FIGURE 3 (left). The new MRC Laboratory of Molecular Biology (right wing) and University Department of Radiotherapeutics (left-wing). The huts belong to the builders and are not intended as laboratories. Architects: Easton and Robertson, Cusdin, Preston and Smith.

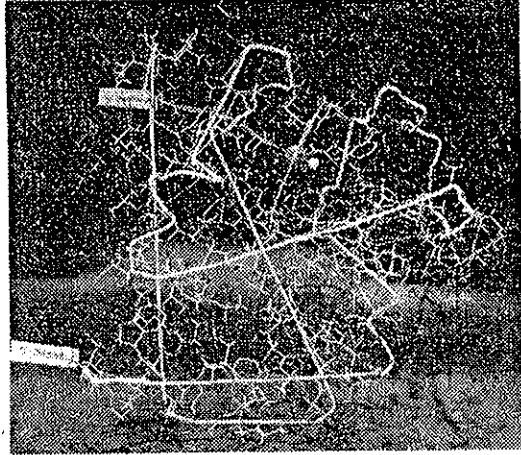


FIGURE 4 (right). Atomic model of myoglobin. (Reproduced by permission from J. C. Kendrew, H. C. Watson, B. E. Strandberg, R. E. Dickerson, D. C. Phillips and V. C. Shore, Nature 190, 663, 1961.)

During the following years we were joined by F. H. C. Crick, H. E. Huxley, J. D. Watson, V. M. Ingram and others. Good work was done, but no decisive results emerged. The turning point came in 1953 when Watson and Crick solved DNA; Huxley and Jean Hanson, by then at MIT, discovered the sliding mechanism of muscular contraction, and I found that protein structures could be solved by the method of isomorphous replacement with heavy atoms.

This was the moment, perhaps, when I should have proposed to the MRC the setting up of a proper Laboratory of Molecular Biology. But was public opinion ready for it, or, for that matter, were we? At that time our work stirred up little enthusiasm in this country. When I discussed the implications of Watson and Crick's discovery with a leading geneticist, he assured me that, as far as his subject was concerned, it had none. Most of our crystallographic colleagues continued to be highly sceptical of the prospects of solving protein structures by X-ray analysis, and it was true that Kendrew and I were still facing great difficulties. I thought it wiser to continue on a modest scale until we felt surer of success. It was shortly afterwards that the University gave us the Hut.

By the spring of 1957 the outlook had brightened. Kendrew's work on myoglobin had progressed to a point where we both felt that this structure, at least, would be solved, even though my own work on haemoglobin was still in the doldrums. Sidney Brenner had joined us, and by his dynamic work had created the bacteriophage laboratory in which the recent discovery concerning the nature of the genetic code was made. Ingram had discovered how genetic mutations affect the sequence of amino acids in proteins. Finally Frederick Sanger, whose famous work on

the chemical structure of insulin had also been supported by the MRC, said that he would like to join us.

Our proposal to build a Laboratory of Molecular Biology met with immediate response from Sir Harold Himsworth, the Secretary of the Medical Research Council. He put it before Council which approved it in a single sitting, and he persuaded the Treasury to provide the funds.

The next problem was the finding of a site. My colleagues and I wanted to carry on within the University where our work would benefit from the interchange of ideas and where we could attract young people. On the University's side our presence was welcomed by many members of the scientific faculties. However, when the proposal was placed before certain officials of the University we were told to put it out of our minds. The University, we were firmly reminded, would not permit any further expansion of research within its precincts, especially if it was divorced from teaching, and it would oppose the setting up of research laboratories by outside bodies in its vicinity.

Fortunately the University's constitution, like that of the United States of America, provides for a system of checks and balances, and no man's word need be taken as final. Nevertheless, in the face of such policy it took a year's negotiation and much hard work by our friends in the University before a suitable site was finally offered to us. Much to their regret, and ours, it proved impossible to find one close to the main science laboratories in the centre of the town, because every available square yard there is already built up or allocated.

Instead, the laboratory is two miles from the centre of the town on a site intended for the development of a Post-graduate Medical School. Our building,

which we share with the University Department of Radiotherapeutics, was to be the first of that new School, but the further development of the School has now been jeopardized by the vote of the Regent House against salary differentials for clinical staff.

The new Laboratory of Molecular Biology will have a research space of about 22,000 square feet. It will be divided into protein chemistry under Sanger, protein crystallography under Kendrew and molecular genetics under Crick, with myself as Chairman. In addition to the members of Sanger's and our Unit who are already in Cambridge it will be joined by H. E. Huxley, the electron microscopist now at University College, London; by the Virus Research Group of Aaron Klug, now under Professor J. D. Bernal at Birkbeck College, London, and by J. D. Smith, the well-known nucleic acid chemist. J. D. Watson, now at Harvard, is coming back for a period, and many other visitors from abroad are hoping to come and work with us.

The Laboratory will be well equipped for X-ray crystallography, electron microscopy, biochemistry and virology, and it already has an excellent workshop.

Could it have got ready sooner, and could better accommodation have been provided for the Unit in the meantime? A year's negotiation with the University might well have been saved. But then the University is a very old lady indeed, apt to say *No*, at first, to any new proposal, and it takes patience to get her to come round. As to the second question, the fault really lies with an earlier generation who built their scientific laboratories on two confined sites in the centre of Cambridge, where budding branches of science can unfold only by waiting for old ones to die and drop off. What is wrong today is that this has been adopted as the official University policy.