Burnet

thousands tested and it was felt to be unfortunate that it had been synthesized by the Germans before 1939. At Yale and Washington, scrub typhus was the major interest. We had worked with it in Melbourne and we had sent the first culture from the New Guinea area to Washington a few months before. Within another year chloramphenicol would be available to deal with scrub typhus, but many servicemen had died by then and the death of Dora Lush from a laboratory infection in our own Institute eight or nine months previously was very much in my mind.

The Allied attack on Italy was in progress at the time and already I was hearing about the success of DDT in handling the explosive typhus situation in Naples. It was a story of exactly the type that Meyer relished and it lost nothing in the telling. The significance of DDT for the control of malaria was also being whispered about. K.F. told me the story of the set of ponds chosen for controlled tests on mosquito larvae. The experimental ponds were rapidly freed but then the larvae vanished also from the *control* (untreated) ponds. The solution? DDT was so powerful that when ducks moved from one of the experimental ponds to the untreated ones they carried enough DDT on their plumage to produce a larvicidal concentration in the water!

I shall always regard the war and the immediate post-war period as the time when the full possibilities of control of infectious disease were realized or could be clearly envisaged. But a new epoch of biological science was also opening and I saw something of its beginnings on that American visit.

On a number of occasions, I have spent longer or shorter times during the summer months at Cold Spring Harbor but I first saw it on New Year's Eve 1943. It was a lovely winter morning when I arrived, the inlet icebound but cracking with the tides, and there were dozens of seagulls lining the cracks or peppered over the ice. Bare woods, brown fields, nice houses and country lanes made me feel that it would be pleasanter to take a long winter walk than to talk about genes and viruses. In fact, I had an unforgettable time with Demerec and his staff. My most vivid recollection is of seeing for the first time how *Drosophila* was handled by geneticists. I had read much of fruit-fly genetics but had never seen the insect itself. So I was introduced to them living in quarter-pint cream bottles with a nutrient mixture of corn meal, molasses and yeast at the bottom—insignificant little flies but at that time the most highly pedigreed animals in the world. Demerec was at that time a *Drosophila* geneticist. A few years later he moved into bacterial genetics. Signs of the move were only just appearing, perhaps I helped it on. After lunch in the Director's house I gave an informal talk on mutation in influenza virus—the O to D phase change—adopting a genetic approach. I noted at the time that geneticists were just becoming interested in bacteria and bacteriophages. I was assured that the concepts I was developing about influenza virus variation and selection were sound and I came away with the distinct impression that my geneticist audience was delighted to find a microbiologist with a real interest in their science! What a change took place in the next dozen years: by then, most geneticists were bacterial geneticists!

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* But not mentioned in his paper in Med Journal ; while Beadle is. 2:557 '44

At the Rockefeller Institute I called on O. T. Avery who, in the words of a letter of mine to Linda: 'has just made an extremely exciting discovery which, put rather crudely, is nothing less than the isolation of a pure gene in the form of desoxyribonucleic acid.' I think that must be almost the last time I ever wrote DNA in full. Nothing since has diminished the significance or importance of Avery's work. Neither he nor I knew it at the time but in retrospect the discovery that DNA could transfer genetic information from one pneumococcus to another almost spelt the end of one field of scholarly investigation, medical bacteriology, and heralded the opening of the field of molecular biology which has dominated scholarly thought in biology ever since. Avery was an oldish man then, beginning to live a little in the past, and happy to relate to interested visitors how his work with the pneumococcus had reached this climax. He told the story well and with pride. I feel that Avery's work was so important a link between the old and the new that I should attempt to describe it.

We have almost forgotten that pneumonia was once the 'captain of the men of death' but for the period between the two wars the study of pneumonia and the other pneumococcal infections was the most active and successful area of bacteriological research. The Rockefeller Institute was the world centre for that research and over the whole period Avery was its guiding spirit though he had many brilliant collaborators, Dubos and Heidelberger among them.

The key finding was that the pneumococci could be divided into 'types' in the sense that if a man or an animal became