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SIR FREDERICK CHARLES BAWDEN, F.R.S. (Photograph by P. H. Gregory, F.R.S.)

(Facing p. 1)

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Obituary

FREDERICK C. BAWDEN, 1908–1972

It is not usual to ask members of the family to write an obituary. For that reason, I was at first unwilling to write on Fred: I knew him for 45 years and we collaborated for 38. But the period during which our collaboration was his main research activity lasted for only 10 years (1935–45), and our long association depended on esteem rather than similarity: a measure of objectivity may therefore be possible.

The psychological quirks that lead to prolonged amicable collaboration deserve much more careful study than they have hitherto had. Fred's physiological responses were radically different from mine, and our ranges of chemical and biological interest and knowledge were contiguous rather than overlapping. We agreed on political and social issues well enough to sustain useful argument – with me somewhat to his left. And we found each other congenial company in a pub. Such factors as these are probably at the root of successful collaboration. In the absence of the biological urge, collaboration is as difficult a matter to manage well as marriage. In most of our experiments, I made preparations and he tested them. Our dissimilar diurnal rhythms were therefore convenient for he said that he did not fully wake up till about midday. We shared a cabin on the way to the International Microbiology Congress in the U.S.A. in 1939 and he finished the voyage still wondering whether to be more amazed at the suddenness with which I went to sleep at night or got up in the morning.

Fred's awareness of the problems involved in growing healthy plants was awakened early, for his parents were master and matron of Okehampton Poor Law Institution and took a keen interest in its large garden. This awareness was reinforced at Okehampton Grammar School, where Botany was the main science subject, and it gained him a Ministry of Agriculture and Fisheries' scholarship to Cambridge. At Cambridge, Fred was sport-loving and gregarious. He had an extremely retentive memory and was able, without apparent diligence, to impress the examiners and college authorities with his knowledge of Botany, Chemistry and Physiology. More pedestrian students were equally impressed by the amount of time he seemed to have free for social activities. While taking a Diploma in Agricultural Science he met R. N. Salaman and joined his staff at the Potato Virus Research Station. This was a very important step. Salaman was at that time the greatest authority in Britain on potato varieties and potato diseases. Furthermore, they liked each other. Conditions at that time were primitive in all laboratories, the Biochemical laboratory in Cambridge had one centrifuge, Salaman's laboratory had none – its most elaborate piece of physical equipment was a 'Primus' stove. But Salaman had boundless enthusiasm, and his medical training made him quick to see the merits of serology both for identifying and assaying viruses. Fred was at first mainly concerned with necrotic diseases of the potato. As a sideline he experimented with infrared photography to show up necroses clearly. We sometimes travelled together in war-time, camouflaged Britain – he said then he hoped the Germans had not read his paper because the green paint that was supposed to make factories look like fields and woods would show up dramatically on an infrared plate.

Because of the absence of equipment in the virus laboratory, serological work was done in the Department of Pathology, where he was taught serological technique by E. T. C. Spooner.

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A. A. Miles was also on the staff of that laboratory. He and I were working on *Brucella*. I was therefore a frequent visitor and saw much more of Fred than I had as a student. We started to collaborate on viruses in 1934. At first I merely dialysed some of the virus preparations that were being tested and showed that they contained 99·9 % (approximately) of diffusable matter. However, having available a method of assay, it seemed reasonable to move a little into the then unknown territory of the chemical nature of a virus. We soon found, by studying inactivation by enzymes, that potato virus 'X' *contained* protein, but were careful to point out that this was not the same as saying that it *was* protein. Then, as now, potato virus 'X' was difficult material to handle and we got nowhere with attempts to purify it.

When Fred moved to Rothamsted with the title of 'virus physiologist' work was no longer restricted to viruses that infect potatoes. Like most plant virus workers we therefore switched to tobacco mosaic virus and in a few weeks, using the methods that had been standard in protein chemistry for half a century, got liquid–crystalline preparations of an infective nucleoprotein. We were, of course, aware of other claims for crystalline TMV but treated them with justifiable derision. By hindsight it is clear that some were crystalline but contained very little TMV; others were fibrous but so incompetently studied and described that the description was incredible.

The unusual physical properties of TMV attracted some attention. The presence of nucleic acid attracted very little: for two years it was indeed vehemently contested. It must be remembered that nucleic acids were not, at that time, fashionable. People who read the standard textbooks thought they were tetranucleotides and so too simple to be vehicles for much specificity. I was at fault here because, having read while a student all (this was perfectly easy to do at that date) the relevant literature on nucleic acids, I knew that the tetranucleotide hypothesis was nonsense and wrongly assumed that everyone else concerned with the matter knew this too. So we argued about the presence of nucleic acid simply as a matter of fact and not of philosophy. We did not regard TMV, or other viruses for that matter, as very small bacteria, but we found absolutely nothing surprising about the presence in these large particles of many different types of molecule. We said this in our first paper on 'X'. As a result, although Schlesinger had found nucleic acid in his best bacteriophage preparations in 1933, nucleic acid was not taken seriously until 1944 when Avery and his colleagues found it in preparations causing pneumococcal transformation. The time was then ripe for a change in fashionable assumption and nucleic acids were becoming topical: for getting a point across, it is more important to be topical than to be right.

Fred had, as I have said, a prodigious memory. He had no card index and made very few notes. He remembered all that had been published about the properties of plant viruses and scanned his knowledge to find those that were fairly stable in sap and transmissible by inoculation with sap diluted to an extent that suggested the presence of a reasonable concentration of virus. During 1937 we made liquid–crystalline nucleoprotein preparations of two more strains of TMV and three other viruses, including potato 'X'. In 1938 we got true crystals of tomato bushy stunt virus. Almost all the crystals were rhombic dodecahedra, but a few seemed to be hexagonal or pentagonal. We were aesthetically disappointed at finding no true dodecahedra and, at that date, exhausted our appetite for the now popular sport of manipulating the Platonic solids.

Although these viruses cause widely different symptoms in infected plants, they are not transmitted by insects. Therefore Fred chose the aphid-transmitted viruses, potato 'Y' and hyoscyamus 3, for our next studies because we thought it possible that insect transmission might be associated with a different chemical constitution. Equipment had by that time improved – but only so far as a centrifuge carrying 36 ml and with a top speed of 16000 rev./min. These viruses were therefore extremely troublesome because there was very little in sap and what little there was was unstable. But in the end we got out liquid–crystalline nucleoproteins. A few years later, with better equipment, we separated differing crystalline nucleoproteins from plants infected with six different cultures of tobacco necrosis virus. One of these was not itself infective and Kassanis later discovered satellitism with it.

We were often tempted to make, and were sometimes accused of having made, the generalization that plant viruses were nucleoproteins. We were however well aware that chemical and physical criteria had been used in choosing the 15 viruses or virus cultures with which we worked, so some uniformity in the end product was not altogether surprising. By 1939 we were getting bored with separating viruses in the hope of finding one that was not a nucleoprotein and felt disinclined to spend all our time on that search. With purified preparations, rather than the 'clarified' sap that had been used hitherto, it was worth-while studying the intrinsic properties of some viruses. We examined the disrupting effect of an extensive range of agents on TMV. Many years later at a meeting in New York someone referred to one of the methods for making TMV nucleic acid as 'our' method. Fred interrupted him to ask 'Which method do you mean? I think we were the first users of all of them'.

The work on disrupting TMV was done while I was still in Cambridge. We met two or three times a month and exchanged results and samples for assay either at these meetings or by post. Either way there was a 24- to 48-hour delay between preparing the samples and assaying them. Phenol was a disrupting agent used in many experiments. It would obviously be foolish to claim that we would necessarily have observed the infectivity of TMV nucleic acid if we had been working in the same place and samples could have been tested quickly – the loss of about 99·7 % of the infectivity is easy to mistake for the loss of 100 %. It is however certain that the residual infectivity of material as fragile as TMV nucleic acid could not have been observed in the circumstances in which we worked. However, if we had noticed it, the hey-day of the nucleic acids would have started 10 years sooner.

Fred's interest after coming to Rothamsted was by no means limited to work on the properties of isolated viruses as his books clearly show. Besides the four editions of *Plant Viruses and Virus Diseases* he published a general book called *Plant Diseases*, took an active interest in fungus diseases and their control, and encouraged similar breadth of interest in his colleagues. His own words (1970) state the position admirably:

> 'Despite the contrary opinions of those who favour increased specialization and would separate bacteriology, mycology, and virology, my conviction that these are better kept together has become stronger rather than weaker. Why separate mycologists from virologists when their mutual interests should be increasing by the discoveries that fungi both suffer their virus diseases and are the vectors of some viruses that damage crops? Also, despite the great differences between bacteria, fungi, and viruses, the principles and practices of protecting crops from them do not differ. They rest in using varieties that best resist or tolerate infection, destroying sources of infection, planting uninfected stock in uninfested land and away from infected crops, and use of appropriate chemicals to protect a growing crop. A minor difference is that to protect against viruses, the chemicals will usually be aimed against the organisms that transmit them, whereas they will be aimed directly at bacteria or fungi.
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> 'Developing a control measure against an infectious disease in field crops often does not even demand knowledge of the cause, but only of the epidemiology of the disease, to know where the cause comes from, and how and when it spreads, so to know where it is

most vulnerable to attack. It is fascinating to know that aster yellows is probably caused by a mycoplasm instead of, as long thought, a virus; this may allow an extra treatment by antibiotics, but it will not affect heat therapy or control by protecting plants against the vectors. Those working on aster yellows or similar types of disease were appropriately accommodated in departments of Plant Pathology, but would they have been in departments of Virology? And will there now be departments of Mycoplasmology? Possibly, but I hope not because if there were, I fear the workers would become increasingly concerned with minutia of the organisms and increasingly remote from pathology. Pathology needs specialists of many kinds, but will derive most benefit when these are working together with the common aims of understanding pathogenicity and improving plant health.'

This breadth of interest was reflected in the diversity of the themes he studied. Some might be called pure phytopathology. In collaboration with Kassanis he demonstrated that there was nothing mysterious about paracrinkle virus; with skill it could be transmitted by inoculation. They also studied the suppression of potato virus Y by severe etch virus, differences in the reaction of different potato varieties to infection with virus Y, the effects of host nutrition on the multiplication of viruses, and the inhibition of virus multiplication by thiouracil. This work turned his attention towards the physiology of virus infection. First with Roberts and then with Kleczkowski he studied the effects of illumination on susceptibility to infection and the multiplication of virus in the infected cell; the former is increased by darkening, the latter by illumination. With Kleczkowski he went on to study 'photoreactivation' – the ability of plants inoculated with virus preparations, that had been inactivated by u.v. light, to become infected if exposed to ordinary light. The extent of reactivation depended on many environmental factors and also on the interval elapsing between inoculation and exposure to light. Many points that may ultimately clarify our understanding of virus multiplication arose during this work, but await fuller study.

Fred's serological skill and experience, with both rod-shaped and approximately spherical viruses, helped to explain the already well-known differences between flagellar and somatic antigens. He also put it to good use in clearing up some of the confusion surrounding the tobacco necrosis viruses. Then, with Kleczkowski, he examined the complexes formed when bushy stunt or tobacco mosaic viruses are heated with serum albumin. These complexes no longer precipitate with specific antiserum, but retain the capacity to immunize rabbits and to fix complement. Serological relationships seemed to Fred the best basis from which to start a rational system of virus classification. Problems of virus classification, as opposed to labelling, are still with us and will remain with us until very much more is known about the construction of viruses, their origins, and their relations with the infected host. Fred argued persistently, cogently and humorously against the activities of anyone yearning to become the Linnaeus of viruses. His words (1970) may be quoted again:

'However, many pathologists seem still imbued with the faith I have lost, for how else to explain the increasing numbers being attracted to studying the detailed physical and chemical structure of virus particles? It surely cannot be only that the sophisticated and expensive equipment needed for the work has an irresistible glamour, although it is curious that taxonomy should be fashionable with viruses, whereas it seems to be languishing in mycology and in other parts of botany where it is more simply studied. For, of course, it is in taxonomy rather than pathology that the results of work on such things as size and shape of virus particles, number and arrangement of protein subunits, or position of nucleic acid and ratios of nucleotides, are likely to be useful. Taxonomy is a worthy subject, but I hope it will not attract too many virus workers from pathology, which is even worthier, especially as few pathologists will be likely to contribute as much new information as those already skilled in biochemical and biophysical techniques.

'There is nothing easier than to put a virus through the current range of standard machines, some automatic or semiautomatic, that will purify it, photograph it, measure it, and analyze it, with a paper at the end containing the canonical measurements and pictures editors of journals readily accept, even though in essence it contains nothing new. It is much to ask someone to give up this easy approach to publication and tackle the more difficult problems in pathology.'

From about 1950, Fred's interests moved increasingly away from what viruses are, and towards what they do. By this time we had come to regard virus multiplication as an aberrant aspect of the normal synthetic processes of the host: that is to say, we did not regard the host as an inert, albeit complex, medium in which a virus multiplified like a small organism, instead, we regarded the host as a piece of machinery able to synthesize different types of molecule according to the stimulus initiating the synthesis. At the Oxford meeting of the Society of General Microbiology in 1952 we chose to present this case in terms of a virus misinstructing the protein-synthesizing mechanism, but pointed out that it could just as cogently have been argued in terms of nucleic acid synthesis had more been known about that process. Our point of view did not then gain wide acceptance.

On becoming Director of Rothamsted he had less time for doing, and, more important, for thinking deeply about, his own research. With characteristic eagerness not to lose touch with practice he insisted on doing all the inoculations for experiments in which he was involved and referred to this respite from paperwork as 'occupational therapy'. I had a suspicion that some of our more bizarre results arose because leaves were sometimes inoculated during animated conversations with various members of the staff about their problems – this suspicion was forcefully rejected. Whatever the cause of our often erratic results, we managed to reach some definite conclusions about some substances and systems in leaves that could inactivate tobacco necrosis virus and TMV nucleic acid *in vivo*, and that attached TMV nucleic acid to the insoluble matrix of the leaf. At the time of Fred's death we had finished writing one paper, and nearly finished another, on the manner in which these processes could affect the susceptibility of leaves in different physiological conditions to infection by TMV nucleic acid, and the subsequent spread of virus within the host.

Many of Fred's ancestors were closely connected with farming in Devon; one of them designed and marketed a novel type of plough. As one of the passages I have quoted shows, Fred thought of agricultural research in admirably practical terms. He did not insist that a line of research should necessarily lead to practical benefits immediately, but he thought that scientists in such an institute as Rothamsted should have a clear idea of benefits that might ultimately flow from their work. He was therefore resolutely opposed to any suggestion that control of the direction of research should rest with civil servants and others not directly involved with research. Practical scientists are the only people likely to recognize the more productive lines of investigation. Much of his time during the last few months of life was occupied in writing articles and memorandums demonstrating the falsity of the assumptions underlying Lord Rothschild's proposed changes in the control of research. He was particularly pleased to quote two seemingly academic pieces of work at Rothamsted that achieved their long-term objectives: King Edward potatoes, freed from paracrinkle virus, yield in Britain an extra 100000 tons annually for the same input; and studies on the structure of the pyrethrins led to the synthesis and commercial production of very effective analogous insecticides.

Not only did Fred expect a deterioration in research if it were subjected to Ministry control, he was scathing in commenting on the research suggestions emanating from departments of government under the present system. His comments, in the Rothamsted Experimental Station Report for 1970, on the alleged deterioration in soil structure under modern methods of farming, are a masterpiece. He appreciated the truth of Swift's comment 'You write with the point of a pen and not with the feather'. The loss of so vigorous a defender of the independence of research institutes is an eminently practical reason why everyone with the welfare of humanity at heart should mourn his death.

Besides research and administrative duties at Rothamsted, Fred accepted membership of an extensive range of committees, and the presidency of an almost equally extensive range of organizations, both in Britain and overseas. His reasons for undertaking all this work are not altogether clear. These activities consumed a great deal of time and he had no illusions about the actual or potential value of some of them. The word 'wasted', which he often applied to some of the time he spent in this way, was obviously not meant to be taken literally – it was an expression of distress at being taken away from more congenial work in the glasshouse or on tasks directly connected with agriculture. With his forthright and practical approach to scientific problems, and his mastery of the art of chairmanship, he undoubtedly made some of these committees a great deal more useful than they would otherwise have been. The motive may have been that he liked doing a job he knew he did well and it may have been akin to his equally puzzling (to me) enthusiasm for cricket and rugby football.

All directors of Rothamsted have been interested in, and knowledgeable about, the practicalities of farming. Fred added to that an enthusiasm for the practicalities of presenting the results of research. No other director took so much trouble over both the form and content of our papers. Like everyone concerned with language, he had a few obsessions. He would, for example, have replaced 'like' in the last sentence by 'as', he would allow 'case' to be used only for a container or in a legal context, and he strove to persuade his staff that the English language had adjectives other than 'high' and 'low' with which to express magnitude. More seriously, he was adept in detecting prolixity and ambiguity and helpful in suggesting an improved wording. In his tiny writing, these suggestions were sometimes illegible, and haste sometimes made him suggest phrasing that was uncouth. But it was invariably worth while rewriting any passage he had begun to amend.

Death, from heart failure, came suddenly. He worked in the laboratory on the 5th of February and died on the 8th. Fred's cheerful, exuberant and humorous manner partly hid a great intensity of feeling on matters connected with research and the welfare of Rothamsted. When interjecting at a meeting on a theme that moved him, emotional tension often produced an unexpected quiver. It would be unreasonable to attribute his early death to overwork, but he worked extremely hard. Saturday and Sunday were the days on which I could most rely on him for research, and when he was in Harpenden he was usually to be found in the laboratory long after most had left. Everyone connected with Rothamsted will miss him – few will miss him as much as I do.

N. W. PIRIE.

The Rothamsted Experimental Station Report for 1971 (published June 1972) contains a complete list of Bawden's publications and an extensive list of his activities. An abridged list and the dates most relevant in the present context follows:

1908	Born in North Tawton, Devon
1926–30	Emmanuel College, Cambridge
1930–36	Potato Virus Research Station, Cambridge
1935	Married Marjorie Elizabeth Cudmore
1936–40	Virus Physiologist, Rothamsted
1940–48	Head of Plant Pathology Department, Rothamsted
1949	Fellowship of Royal Society
1950–58	Deputy Director, Rothamsted
1955	Research Medal of Royal Agricultural Society
1958–72	Director, Rothamsted
1959	Leeuwenhoek Lecturer, Royal Society Honorary Life Member of New York Academy of Sciences
1959–60	President of Society for General Microbiology
1964–67	Chairman of Agricultural Research Council of Central Africa
1967	Knighted
1965–70	Member of Natural Environment Research Council
1965–66	President of Association of Applied Biologists
1968	President 1st International Congress of Plant Pathology
1968–72	Treasurer of Royal Society