ROTHAMSTED RESEARCH

Report for 1971

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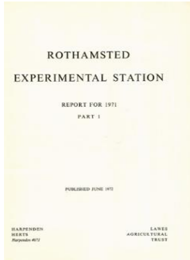
ROTHAMSTED
EXPERIMENTAL STATION
REPORT FOR 1971
PART 1

Federick Charles Bawden - a Review or Rothamsted During His Directorship, 1958-1972

Rothamsted Research

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Frederick Charles Bawden

A review of Rothamsted during his Directorship, 1958–1972

G. W. Cooke

Sir John Russell invited F. C. Bawden to join Rothamsted in 1936. He succeeded J. Henderson Smith as Head of the Plant Pathology Department in 1940, at the age of 32, and Sir William Ogg as Director in 1958. Rothamsted then had 471 staff whereas when he died it had over 700 and there were 48 more at Broom's Barn.

Progress in our work is best measured by reading our Annual Reports; nevertheless some figures of growth are impressive. The Report for 1958, Bawden's first, had 283 pages and recorded 247 published papers. The Report for 1970 was in two parts, had a larger format, totalled 644 pages, and recorded 330 published papers. During his time the Report became more valuable to scientists, agriculturists and general readers. Bawden's advice and help to departments in describing their work was invaluable, but the style of the Report owed much to his own 'General Report', described in a recent review as 'a connoisseur's piece'. These Reports, and his discussions of their relationship with current agricultural problems, were eagerly read by scientists and laymen. His keen pen examined old problems and suggested how our work might help their solution; usually there was comment on farming conditions, and vigorous intervention in debates on topical subjects, particularly if farming lore was being given more weight than scientific evidence. He finished reading the Departments' Reports for 1971 a few days before he died and was about to write his General Report.

Buildings. In 1958 the work of some departments was being handicapped by old and inadequate laboratories, and Bawden worked to provide better facilities. A building for the Orion computer and the Statistics Department was completed in 1961; in the same year Red Gables was extended to provide better accommodation for the Commonwealth Bureau of Soils and our canteen. In 1963 a large extension to the West Building rehoused the Biochemistry and Pedology Departments and the Soil Survey of England and Wales. New glasshouses were built for several departments in the early sixties and the first controlled environment rooms were completed in 1962. In 1965 a new workshop provided 'for the first time reasonable working conditions', and a field laboratory for the Physics Department was built. In 1968 the Computer Department was formed and the proposal to buy the ICL System-4-70 multi-access computer made essential the new building, finished in 1970. Bawden's largest building project, to rehouse the Botany, Nematology, and Physics Departments, and part of the Plant Pathology Department, was started in 1971.

Bawden was keenly interested in the development of Broom's Barn Experimental Station, for Dunholme Field Station had been a part of his Plant Pathology Department. The farm of 200 acres was bought and the station built with money from the Sugar Beet Research and Education Fund. The staff from Dunholme moved there and Broom's Barn was opened in 1962.

At Rothamsted a number of new houses for our staff were built and facilities for sports and social gatherings were much improved by the pavilion built in 1961.

Land for experiments. Our programme of field experiments is always expanding, and often

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we have not had enough land in a suitable state for all the experiments we wished to do. Difficulties at Woburn have been alleviated, but not cured, by renting extra acres. At Rothamsted land needed for future experiments was provided by the purchase in 1965, by the Lawes Agricultural Trust Committee, of the adjoining Scout Farm of 224 acres. The amount of field experimentation has roughly trebled in little more than 10 years. In 1959 the experiments involved 3170 plots, in 1970 9754 plots. In 1964 Rothamsted took charge of the Long-term Rotation experiments at Saxmundham Experimental Station, started by the East Suffolk County Council in 1898.

Education and training. Good staff is more important than land, buildings and equipment, and Bawden helped the training and development of young scientists by personal contact and encouragement, by the immediate help he gave to those working with him, and by more formal arrangements with educational authorities. The link established between Rothamsted and the Imperial College of Science and Technology benefits both the College, which has help in teaching from some of our staff, and students at Rothamsted who are helped and supervised by College teachers. Bawden was made Visiting Professor to the Imperial College of Science and Technology for his work for this scheme. Bawden also gave generously of his time to help research and education in agricultural science in many countries. His membership of many Committees testifies to this, as do many of the honours he received.

Research policies and programmes. Less tangible, but very important for the future of Rothamsted, was Bawden's insistence on the right of the Station to determine the kinds of research that it does. He was among the first to speak for agricultural science in the recent debates on organisation and management of government sponsored research and development. He gave energy and time without limit to writing, and to work in committee, to ensure that research to improve agriculture should not be hindered by too close control by Government. We know that his contribution taxed his personal resources greatly; we hope it will be successful.

Some successful research projects

During his Directorship Bawden guided many research projects that are now benefiting agriculture. The following paragraphs mention only a few, but those chosen illustrate well how wrong it may be to try to distinguish between the values of 'applied' and 'basic' research. In much of our work, these two aspects are simply the parts of a whole.

Paracrinkle virus-free seed of King Edward potatoes. In the Report for 1959 Bawden first mentioned a development in which he took particular pleasure because it illustrated the great practical benefit that may come directly from research done to gain fundamental information. For over 30 years all stocks of King Edward potatoes were known to be infected with paracrinkle virus. In 1955 Kassanis started a virus-free stock by excising the apical meristem of a sprout of this variety (the growing points of stems often contain no virus). By 1959 the stock was being rapidly multiplied; in 1960 experiments yields were 10% larger than from the old infected stocks. The clone now supplies all stocks of King Edward seed and we estimate that average yields have been raised by about 10%. Following this lead, the technique, originally developed elsewhere, has also been used to free clones of other potato varieties and other crops of their virus diseases.

Control of beet yellowing viruses. When Bawden became Director the Plant Pathology Department had already shown that yellowing viruses in sugar beet could be controlled

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by cultural methods that prevent the sugar-beet seed crop acting as a source of infection, and by spraying crops with insecticide to control the aphid vectors. In 1958 his department organised a regular survey of the crop from Dunholme Field Station, and warnings of the need to spray were issued when aphid populations reached critical numbers in each district. Experiments showed that spraying at the appropriate times increased yield by as much as 25%, when unsprayed crops were severely infected. It was a remarkable achievement and within two years the spray warning scheme became a routine farm practice.

Docking disorder. Seasonal failure of sugar beet peculiar to sandy soils where seedling root systems are stunted was first described in 1948 in the Docking area of Norfolk. It was unusually prevalent in 1958 and the Report for that year mentioned current ideas that the disorder was associated with soil type and structure and small organic matter contents; it could be controlled by organic manures and taking leys but not by inorganic fertilisers. By 1963 it was clear that, although some examples of the disorder might be due to bad soil physical conditions, damage by ectoparasitic nematodes was often the major factor. The subsequent progress of this fascinating investigation is described in a special article in Part 2 of the *Report for 1970* (pp. 219–236). Damage was alleviated by using nitrogen fertilisers correctly to avoid leaching, and was prevented by nematicides, so that yields were greatly increased.

Irrigation. The first satisfactory experiments made in Britain to test irrigation on a rotation of crops began at Woburn in 1951. In 1960 Bawden summarised the first nine year's results which gave the basis for practical advice to farmers on when to water their crops and how much to apply. He wrote: 'The Woburn experiment more than justifies itself for its immediate value to agricultural practice, but it has value beyond this. Much of the work in the Botany and Physics Departments is done to find how plant growth and crop yields depend on the weather, and the continuing results from the irrigation experiment provide invaluable material for studying this problem'. H. L. Penman summarised the 19 years results at Woburn and discussed their implications in Part 2 of the *Report for 1970* (pp. 147–170). Meantime, irrigation experiments at Rothamsted, begun in 1964, are beginning to yield results that are of practical importance and are also a basis for the Physics Department's work on agricultural meteorology and physiological processes of growth.

Synthetic pyrethrins. Research on the nature and behaviour of the potent insecticides extracted from pyrethrin flowers has been done for many years at Rothamsted. These substances are lethal to many insects (which rarely become resistant) but harmless to mammals, and they act very quickly. In the early sixties this work showed that some new compounds related to natural pyrethrins might be potent insecticides and also more stable. Later the new compounds synthesised were found to be more toxic than corresponding natural pyrethrins. The work was supported by the National Research Development Corporation, and with their help the discoveries were patented and exploited in this and other countries. This work only succeeded because long study of the relations between chemical composition and insecticidal activity made it possible to predict which compounds that could be synthesised would be active.

Computing. Rothamsted's first electronic computer (an Elliott 401) was installed in 1954. A second Elliott (402) computer was acquired in 1961 but the increasing work made a larger modern machine essential. The Ferranti Orion Computer was delivered in 1963 and worked until 1971 when it had been replaced by the ICL System-4-70 computer.

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A centralised computing service, supported by the Agricultural Research Council, has been established by the new Computer Department; other agricultural research institutes in England and Wales have direct access by land line to the computer. The numbers of replicated experiments analysed by the Statistics Department is an example of how much work is done: in 1955 803 experiments (with 834 variates on the computer) were analysed. In 1970 6494 experiments were analysed, involving 55 984 variates.

Changes in the Classical Experiments

Rothamsted dates its foundation from the start of the Broadbalk wheat experiment in 1843. Research on the crops and soils of this and other 'classical' experiments laid down by Lawes and Gilbert was the main work of the Station in their time. The experiments were maintained with little change of treatments through the first half of this century. Critical surveys of the results and discussions of the potential value of the classical experiments for modern farming were published in the *Rothamsted Reports for 1957* onwards. The largest review was Part 2 of the *Report for 1968*, a 215-page survey of the Broadbalk wheat experiment; we published yield results, described the work that several departments have done on crops, soils, pests, diseases and weeds, and gave the first full description of the soils.

Several years of experimenting with small plots within the old large plots of the Agdell and Exhaustion Land Experiments, begun in 1957, proved that the residues of phosphorus and potassium accumulated from many annual fertiliser dressings may benefit future crops and that most or all the nutrients given can ultimately be recovered. The soil analyses and nutrient balances calculated helped to devise manuring programmes that maintain amounts of soil phosphorus and potassium sufficient for the crops grown without wasting fertiliser.

Other modifications provide information needed to help the new farming systems where cereal growing predominates and few or no stock are kept to make farmyard manure. The Classical experiments long ago showed that fertilisers were as good as organic manures for maintaining yields. The experiments can, however, give even more help to modern farming where they assist our work on solving some of the problems caused by pests, diseases and weeds when cereals are grown often. On Broadbalk and Hoosfield Permanent Barley sites where only wheat and barley had been taken for more than 100 years, modern varieties were grown, the manuring was changed to answer some modern problems and new crops were introduced. Continuous barley and wheat are now compared with these cereals grown in rotation with beans and potatoes. These changes, started in 1967 and 1968, have already shown that a century of continuous cereal growing has caused no permanent damage to our land; they may prove to be one of the more significant developments in Rothamsted's history.

Efforts for agricultural improvements

Bawden was always concerned with initiating and fostering developments that raised yields, recognising *yield* as the chief practical measure of soil fertility. His ideas on the range and aims of research in agriculture discussed in his *Report for 1959* (pp. 25–30) are relevant to recent discussions on the organisation and management of research and development. He always stressed the potential practical value of 'fundamental' work: 'That the work . . . on such things as fertiliser use and the control of pests and diseases has this as an immediate rather than ultimate aim is too obvious to be worth comment, but the practical implications of work that at first sight may seem to be academic or esoteric can also be considerable'. He was also patient with long-term work: 'agricul-

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ture is an industry that must continue indefinitely, and nothing could be nationally more important than to know what practices increase and what practices decrease the fertility of the soil'. He recognised the need for versatility, 'Our work has to be diverse, because agriculture is varied and raises an unending series of interacting problems, with each new practice bringing new consequences in its train'. Quick solutions in agriculture are rarely of permanent value—'The empirical approach can be very successful and sometimes quickly produces answers to practical problems, but alone is wholly inadequate for the needs of agriculture'.

Many times in his Reports Bawden discussed how yields might be further increased. In 1959 he wrote 'Forty years ago it was rare to record wheat yields of 1 ton/acre at Rothamsted, whereas we now sometimes record well over three. The difference has three main causes: better feeding, pests and diseases under better control, and higher yielding varieties'. As a pathologist Bawden always wished to know more about the organisms that damage plants, but he was just as interested in research devised to eliminate them. Nevertheless the Reports also show he was well aware of the dangers of residues of pesticides and herbicides in soils and crops. Rothamsted yields continued to increase in Bawden's time. In 1969 and again in 1971 a plot of Broadbalk yielded nearly 3 tons/acre of wheat grain. On other experiments some plots yielded over 3 tons/acre of wheat as has happened in most seasons since 1960. In the last few years over 20 tons/acre of potatoes have been commonplace average yields in some experiments at Rothamsted and Woburn. This year at Woburn one treatment gave the incredible yield of 37 tons/acre. Increases in national average yields are partly due to farming being improved by applying research results. Bawden himself deserves much credit for the continued improvement in national average potato yields from about $7\frac{1}{2}$ tons/acre in 1950 to 11 tons in 1971; much of this gain is from the work on virus diseases he began nearly 40 years ago and from efforts of other pathologists who have worked with him.

Bawden was always interested in scientific questions affecting farming and ready to offer our results where these could clarify a situation or resolve a problem. He discussed the role of organic matter in soils, often considered by public and farmers to be the key to soil fertility. However, he always needed evidence that would stand scientific scrutiny; typically, in discussing the Agricultural Advisory Council's Report he wrote: '*Modern Farming and the Soil* is rather depressing reading for research workers, because the authors seem more impressed by hearsay than by the results of controlled experiments'. His criticisms of statements that poor yields were attributed to bad soil structure, and that soils with less than a critical organic matter content would disintegrate, both without proof, were among the few published critical comments on the Report and he was invited to continue his arguments at the 1972 Oxford Farming Conference, one of his last public appearances.

Insistence that as scientists we require proof of evidence before making proposals for practical improvement, that we should not confuse practical expedients with permanent solutions, and that we *must* find knowledge whereby farmers can do what is best for themselves and the nation, are important parts of the inheritance Bawden left us.

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Frederick Charles Bawden

An appreciation by D. J. Watson,
Deputy Director until 1971

Sir Frederick Bawden was a distinguished plant pathologist and a world authority on plant viruses and virus diseases, who had a profound knowledge of agriculture and of the diseases of crops in many countries. This was recognised by the many honours he received (pp. 40). One of his greatest pleasures was his work for the Royal Society, of which he was elected a Fellow in 1949 and Treasurer and Vice-President in 1968. He was a most effective member of many official bodies and committees, not only because of his wide experience but because he spoke only when he had something to say, and what he said was clear, authoritative and often decisive. His work was not confined to Britain. He was Chairman of the Agricultural Research Council of Central Africa throughout its existence from 1964–67, and for many years he advised on agricultural research in the Sudan, the West Indies and Ceylon, becoming familiar with the cultivation and diseases of cotton, cocoa, tobacco, sugar cane, cloves, tea and other tropical crops.

Early in his undergraduate days at Cambridge, Bawden decided to become a plant pathologist, so after taking a First Class in Part I of the Natural Sciences Tripos in 1928 he joined the course in plant pathology taught by F. T. Brooks in the School of Agriculture, and obtained the Diploma in Agricultural Science in 1930. He was then appointed research assistant at the Potato Virus Research Station with K. M. Smith as a colleague, and under the inspiring direction of R. N. Salaman began the researches on virus diseases of plants and on the properties of viruses that continued throughout his life. He unravelled the confusion of virus symptoms in potato and distinguished their causative agents, partly by serology in collaboration with E. T. C. Spooner. With N. W. Pirie he began attempts to purify viruses and to study their chemical and physical properties. By 1936 they had already succeeded in producing liquid–crystalline preparations of tobacco mosaic virus, and made the momentous discovery that viruses are nucleo-proteins.

Bawden's book *Plant viruses and virus diseases*, first published in 1939 and revised in three further editions to 1964, was the first to formulate this branch of plant pathology as a science, and his many scientific papers, often with N. W. Pirie, A. Kleczkowski or B. Kassanis show his great contribution to our knowledge of virus diseases. But the list of his publications does not reflect the much wider influence he had on plant pathology. Only those who worked with him can appreciate fully the breadth of his interest and understanding, but it is illustrated in his readable, accurate and comprehensive introductory text book *Plant diseases* (Nelson, 1950). His assessment of the value of new methods and devices depended on their relevance to the real problems of agriculture, and much of his contribution is represented by the epidemiological studies of virus diseases of crops he led his staff to make. The present scarcity of the aphid-transmitted viruses of potato and sugar beet in Britain, and the avoidance of severe damage by foot and root rots of cereals, owe much to work that Bawden promoted and encouraged, at first under the stress of war-time scarcity.

After 1945, Bawden's department became a training centre for virus research, because as yet there were no university courses. Many post-graduate students and visiting workers came from Britain, the Commonwealth and other countries, and later when he travelled widely, he everywhere met old students and colleagues who had often consulted him

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about their work, so he already knew much about the problems on which his advice was sought. Those he visited have said how greatly they were stimulated and encouraged by his interest and wise counsel. When he became Director of Rothamsted he continued his collaboration with Pirie, and after official working hours was often to be found in his glasshouse, inoculating plants or counting lesions. At such times he was prepared to talk to anyone about whatever they wished to discuss, or about things that interested him or had recently happened to him.

Bawden not only contributed to the scientific literature but worked hard to improve the standards of written English and presentation in the Rothamsted Report and in many journals. He became well known for his strong views that scientific writing must be clear, concise and precise, avoiding long words and involved sentences. He served on the editorial committee of the *Annals of Applied Biology* for 23 years, and when he resigned in 1965 the Editor said 'the many hours he spent criticising scripts profoundly influenced the content and presentation of *Annals* papers: the present reputation of the journal owes much to his insistence on conciseness and clarity'. When not constrained by scientific discipline, his writing was just as clear and direct, but with a larger vocabulary that enabled him unerringly to find the *mot juste*, and unexpected, but felicitous and witty, turns of phrase. He wrote charming and exactly appropriate congratulatory letters that warmed the recipient. He could also write biting, even blistering, letters to bureaucrats of whose actions he disapproved. Indeed, he enjoyed using words and liked to repeat for the enjoyment of others the sentences he thought most successful.

It is easy to write about Sir Frederick Bawden's achievements, but much more difficult to describe Fred Bawden. He was a big man physically and his personality and influence were just as large and pervasive. He liked people of all sorts and treated them all with the same cheerful interest and consideration, except when they were stupid or pompous or mean. Then he could be angry and sometimes devastatingly blunt, but his anger was directed not at the person but at his actions; he would suffer fools but not their folly. He was a great talker—another aspect of his skill in using words—and was a wonderful companion, whether he was boisterous at a party or alone and serious. He was kind and generous, and could be very gentle. He talked to children as easily as to grown-ups and in return they liked and trusted him; he was especially attached to his two granddaughters. He had great self-confidence and seemed not to feel awe of anyone. This gave strength to all he did, but he also used it unselfishly to support and encourage others not so staunch.

Bawden was a prodigious worker; he understood situations and made decisions quickly and worked long hours. He read, criticised and usually improved all the scientific papers written by Rothamsted staff (over 300 in 1970) before they were published, so that he had a far more complete knowledge of the current work of the Station than any other member of the staff. Just before he died he finished reading and correcting all the typescripts of the *Rothamsted Annual Report for 1971*, was spending one day each week at the Royal Society, and was deeply involved in the protracted discussions about the Rothschild proposals for reorganising the work of the Research Councils, in addition to his usual work as Director. Administration—the business of keeping the wheels turning smoothly and the staff happy, that is alleged to be beyond the competence of even the most intelligent scientist—he took in his stride, and probably did far more than he need. But he was no autocrat; he would seek advice and listen to arguments but never shirked the responsibility of making decisions. On the Field Plots Committee, the most important at Rothamsted, he served and behaved as an ordinary member and never over-ruled its decisions except on matters that were clearly his personal responsibility.

His life was so filled with his work that he had little time for hobbies or public affairs, but he had wide interests and a strong social conscience that made him support good

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causes whenever he could. Lady Bawden's skill in providing generous, informal hospitality and a warm welcome enabled them frequently to entertain many friends and a steady stream of official visitors with great enjoyment. He had played rugby for his College, and still enjoyed watching rugby and cricket matches, especially when his sons were playing. He was fond of music, preferably loud and symphonic. But people were his absorbing interest, and the chief pleasure he derived from all his travelling was meeting and talking to more and different men and women of all kinds from Emperors and Presidents to plant physiologists. In this way he had acquired a host of acquaintances, many of whom continued to regard him as a special friend. Now they feel bereft, and incredulous that so vigorous and splendid a life could so suddenly be lost.

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Frederick Charles Bawden

Thirty-eight years of collaboration

A personal appreciation by N. W. Pirie

Perhaps this appreciation of Fred Bawden should have been written by someone else. I knew him for 45 years and we collaborated for 38. That degree of intimacy could be held to debar me from writing: a relative or spouse is usually debarred lest threnody should approximate too closely to autobiography. However, our long association depended on esteem rather than similarity. Objectivity may therefore be possible.

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 Sciences 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 labs; the Biochemical lab in Cambridge had one centrifuge, Salaman's lab 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. During our rare journeys together in war-time, camouflaged Britain, he often said 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 lab, serological work was done in the Department of Pathology. I was a frequent visitor there, working with Ashley Miles on Brucella; Fred and I therefore saw more of each other than we had done as students and started to collaborate 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 diffusible 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

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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 text-books 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, as I have said, had 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. Fred therefore 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. With the equipment of the time, e.g. a centrifuge carrying 36 ml and with a top speed of 16 000 rev./min, they proved 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 generalisation 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

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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 so that samples could be 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 viruses 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 specialisation 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.

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 ultraviolet light, to become infected if exposed

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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 them 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 immunise 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 analyse 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 multiplied like a small organism; instead, we regarded the host as a piece of machinery able to synthesise different types of molecule according to the stimulus initiating the synthesis. We chose to present this case in terms of a virus misinstructing the protein-synthesising mechanism, but pointed out that it could just as cogently have been argued in terms of nucleic acid synthesis had more been known about its mechanism. Our point of view did not, in 1953, 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

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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 nearly finished writing two papers 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 recognise 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 100,000 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 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 organisations in Britain and overseas. These activities consumed much time and he doubted the value of some of them, but his forthright and practical approach to scientific problems and his mastery of the art of chairmanship contributed much to their success.

All directors of Rothamsted have been interested in, and knowledgeable about, the practicalities of farming. Fred added to that an enthusiam 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

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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. He said that he did not fully wake-up till about mid-day but, often having trouble in getting to sleep, he worked late. We shared a cabin on the way to the International Microbiology Congress in USA 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. The psychological quirks that lead to prolonged amicable collaboration deserve much more careful study than they have hitherto had. Our physiological responses were radically different, 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. No one but Lady Bawden, herself a botanist and as dedicated to the welfare of Rothamsted as Fred was, will miss him more than I do.

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Frederick Charles Bawden

Honours

1946	Cantor Lecturer, the Royal Society of Arts.
1949	Elected to Fellowship of the Royal Society.
1955	Fernhurst Lecturer, the Royal Society of Arts.
	Awarded Research Medal of the Royal Agricultural Society of England.
1959	Leeuwenhoek Lecturer, the Royal Society.
	Elected Honorary Life Member of the New York Academy of Sciences.
	Elected Fellow of the Royal Society of Arts.
1960	Elected Honorary Life Member of the Indian Botanical Society.
1961	Elected to Foreign Membership of the Royal Netherlands Academy of Sciences.
1966	Elected Honorary Member of the Association of Applied Biologists.
1967	Created Knight Bachelor.
	Elected Honorary Fellow of the Indian Phytopathological Society.
	Elected Honorary Fellow of Emmanuel College, Cambridge.
	Elected Honorary Member of Imperial College of Tropical Agriculture Association (U.K.).
1968	Elvin C. Stakman Award of the University of Minnesota.
	Elected Fellow of the Institute of Biology.
	Elected Treasurer and Vice-President of the Royal Society.
1970	Elected Foreign Member of the Lenin All-Union Academy of Agricultural Sciences of U.S.S.R.
1972	Elected Honorary Fellow of the Indian Academy of Sciences.

Honorary degrees

1964	Awarded Honorary Degree of Doctor of Science, University of Hull.
1967	Awarded Honorary Degree of Doctor of Technology, Brunel University.
1969	Awarded Honorary Degree of Doctor of Science, Bath University of Technology.
1970	Awarded Honorary Degree of Doctor of Science, University of Reading.
1971	Appointed Visiting Professor jointly in Departments of Botany and Technology and of Zoology and Applied Entomology, Imperial College of Science and Technology, London.

Presidencies of Learned Societies

1959–60	President of the Society for General Microbiology.
1965–66	President of the Association of Applied Biologists.
1966–68	President of the British Insecticide and Fungicide Council.
1968	President of the First International Congress of Plant Pathology, London.
1968–71	President of the British Crop Protection Council.
1969–71	President of the Institute of Biology.

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