

# SCIENCE *and* FRUIT

*Edited by*

T. WALLACE. C.B.E., F.R.S.

AND

R. W. MARSH. M.A.

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*Commemorating  
the Jubilee of the  
Long Ashton Research Station  
1903 - 1953*

UNIVERSITY OF BRISTOL

1953

# Foreword by the Chancellor

AS CHANCELLOR OF THE UNIVERSITY I congratulate the Long Ashton Research Station on its distinguished services, and on the attainment of its Jubilee.

More food is needed everywhere in the world, and we in these islands must remain leaders in scientific research into problems of food production. By its past achievements Long Ashton is known wherever these problems are studied.

I trust that these very proper celebrations of past achievements may increase the determination of those who work at Long Ashton to further efforts in the future.

*Ernest S. Churchill*

# Preface

by

D. M. WILLS

C.B.E.

*Chairman of the Agricultural Committee  
of the University of Bristol and of the Governors of the  
National Fruit and Cider Institute*

THE JUBILEE OF THE LONG ASHTON RESEARCH STATION is an event of importance not only to the University of Bristol and the National Fruit and Cider Institute, but also to all connected with the science and practice of horticulture.

Although research in agriculture and horticulture is carried out on a vast scale throughout the world today, the institutions concerned are relatively young, and in Great Britain they were mainly established subsequent to 1909 when the Development Act provided funds for research and advisory work in agriculture. Long Ashton, established in 1903, must thus be regarded among its sister stations in Britain as one of the older centres of research, second, in fact, in point of age, to Rothamsted, which celebrated its centenary in 1943. It was founded before government grants had become generally available for research in agriculture or industry and at a time when the initiation of research depended on the vision and enterprise of individuals and of small groups of enthusiasts banded together into societies for educational purposes.

The story of the origin of the Long Ashton Station is typical of such action, initiated by a Somerset landowner, Robert Neville Grenville, and the Bath and West and Southern Counties Society.

In celebrating this Jubilee it has been thought desirable to record in book form the main facts relating to the establishment of the Station and to its development and activities during the fifty-year period. In doing so reference is made to those who played prominent parts in the early days of the National Fruit and Cider Institute and on whose untiring efforts the very existence of the Station depended at that time. In



this connection the services rendered by the signatories of the Articles of Association of the Institute deserve special mention, for it was largely through their personal efforts in their respective counties that support for the Institute was organised and sustained throughout the cider-making districts of the South-West, and the value of cider as a national beverage made known to the general public. In addition to Mr. Neville Grenville, to whose early work reference is made elsewhere in this volume, a tribute must be paid to Mr. G. W. Radcliffe Cooke, J.P., M.P., who not only broadcast the need for research on cider and the benefits likely to accrue from it to farmers and cider-makers in Herefordshire, and put his own knowledge of cider-making into book form, but who also took every opportunity of referring to cider matters on a national basis in Parliament, so much so that he became known among his fellow members as the 'Member for Cider'.

It has also been considered of interest in preparing the volume to compile lists of the Chairmen of the Governors of the National Fruit and Cider Institute and the main University Committees and of the complete scientific staff during the fifty years, and to record in full the members of the main Committees and staff during the Jubilee Year. It is hoped that these records may prove of interest to those who will be intimately concerned in future years with the work of the Station.

The specialist articles included in the volume have been selected with two main objects in view: first, to provide a general account of some of the more important subjects that have been prominent in the research programme over a long period of years and to which the Station has made substantial contributions; and, second, to summarise the present state of knowledge of some subjects of outstanding importance that have been under investigation at the Station in recent times. The former papers, among other things, illustrate the lack of exact information on horticulture and cider-making that existed during the early years of the Institute, and the two series together serve to show the great progress that has since been made both in scientific research and in its application to practice.

In presenting in this way the general picture of fruit research at the Station over fifty years it is of particular interest to publish simultaneously in the volume the Jubilee Lecture of Lord Rothschild, on the subject of agricultural research in 1953, for we thus have brought together the story of the work of a single station, starting from very small beginnings, over the past fifty years, and an account of the present-day organisation and of some of the achievements of the highly developed national research organisation—the Agricultural Research Council—within whose scheme the Station now operates. Corresponding accounts fifty years hence should provide interesting reading and comparisons.

I am glad to have the opportunity provided by the publication of this volume of paying my personal tribute to those pioneers of agricultural research whose efforts led to the establishment of the Station, and to all those who, as members of the Committees and of the Staff, have throughout the years made their own contributions to its development.

In addition it is with great pleasure that I acknowledge the generous donations towards the costs of publication of this volume that have been received from numerous friends of the Station.

In conclusion, I must express the grateful thanks, on behalf of all connected with the Station, to the Chancellor of the University, for his interest in their work and for his kindness in contributing the Foreword to the volume.

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*Chancellor of the University of Bristol*

## Preface

By CAPTAIN D. M. WILLS, C.B.E.

*Chairman of the Agricultural Committee of the University of Bristol  
and of the Governors of the National Fruit and Cider Institute*

## The Jubilee Lecture

AGRICULTURAL RESEARCH, 1953

By the Right Honourable LORD ROTHSCHILD

G.M., Ph.D., Sc.D., F.R.S.

*Chairman of the Agricultural Research Council*

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## THE JUBILEE LECTURE

# Agricultural Research, 1953

by

LORD ROTHCHILD

G.M., Ph.D., Sc.D., F.L.S., F.R.S.

WHEN YOU, MR. VICE-CHANCELLOR, and the Governing Body of Long Ashton, did me the honour of inviting me to deliver the Jubilee Lecture, Professor Wallace suggested that the subject might be connected with what he called 'some broad aspect of agricultural research'. I came to the conclusion that the broadest aspect of agricultural research which I could think of was agricultural research itself, including of course under that heading horticulture, which is worth £120 million a year to the country; and it seemed fitting that on this occasion, when we are celebrating the completion of fifty years of research, all of which has been exclusively devoted to benefiting and not harming human beings, we should sit back for a while and turn over in our minds what is good and what is not so good in agricultural research in our country. You will not of course expect me to cover the whole field, which now extends over a fantastic spectrum of subjects ranging from cheese to electronic calculating machines. We in this country are a modest people, not given to patting each other on the back; in fact we are prone to run down our own efforts, so it will not surprise you if I start by discussing some aspects of agricultural research which come into the not-so-good category. The four subjects I wish to discuss under this heading are:

- Inadequate contact between the research worker and the farmer;
- Inadequate pressure on short-term problems;
- Inadequate knowledge of the order in which these short-term problems should be tackled;
- and lastly,
- Inadequate organisation of agricultural research.

*Contact between scientists and farmers.* On paper the Agricultural Research Service is not supposed to be in contact with the Industry at all. Statutorily this function is discharged by the Agricultural Improvement Councils of the Ministries and by the National Agricultural Advisory Service. They are supposed to translate the findings of the scientists into language acceptable to the Industry and to pass back to the Research Service problems with which the Industry is confronted. I am glad to say this arrangement does not work in practice and many of our agricultural research institutes are in direct contact with the industries they serve. But this contact is not intimate enough. Few farmers know about the existence of the Agricultural Research Service as a whole, and even fewer what it does. There is still a tendency for farmers to think of scientists as long-haired Left-Wing boffins, and for scientists to think of farmers as Right-Wing octogenarians with their leggings, or spivs who, when they are not reclining on their feather beds, career about the country in their Bentleys and Jaguars. How can the research worker understand the problems of the farmer, or the farmer the problems of the research worker, unless they are in contact with each other and not subject to the misinterpretations of intermediaries? There appeared at one time to be organisational advantages in keeping farmers and scientists apart, but as so often happens in this country, we have devised a sort of compromise whereby on paper scientists are not supposed to talk to farmers, but in practice they do, with the knowledge and approval of all the Government Departments concerned. And I believe that the more the regulations are broken the better it will be. Recently I had occasion to break the rules and talk to a number of farmers. I was amazed at how valuable their observations were in helping us to design experiments to increase food production. I was equally amazed to find how little these farmers knew about the methods and limitations of the rigorous type of scientific research we believe to be essential.

*Short-term problems.* When I first became Chairman of the Agricultural Research Council many people impressed on me the importance of getting really good scientists into the Agricultural Research Service and letting them do whatever work they were best suited for, even if their research did not show any immediate promise of successful application in agriculture. It was said that such scientists would increase the prestige of our Service and also that they would attract good younger men who might gradually become infected with the enthusiasm that so many of us feel for our peaceful activities, so that as the years went by, the standard of our work would improve and everyone would benefit. During the last five years we have tried very hard to get these good scientists to work for us. As a result we now have a formidable

group of botanists, zoologists, physiologists, biochemists, geneticists and biometricians working for the Agricultural Research Service. It is perhaps invidious to single people out by name, but in these subjects it would I think be difficult to find any comparable organisations in this country or elsewhere which could better, just for example, Bawden and Robin Hill in botany, Medawar and Wigglesworth in zoology, Blaxter, Folley and Hammond in physiology, Peters, Hanes and Synge in biochemistry, Darlington, Fisher, Mather and Waddington in genetics, Brunt and Childs in physics, or Fisher (again) and, need I hardly say, Yates in biometry. All these are closely connected with agricultural research in this country, so that now we get what we need from 'pure' science, if I can use that rather distasteful expression. We must, however, accelerate the tempo at which the more pedestrian experiments, or trials as they are sometimes called, are designed and carried out. It would be impossible even to begin to try and make a list of all the *ad hoc* experiments which, at the moment, need to be done. I will only give one example. A farmer recently suggested to the Agricultural Research Council that they should do experiments to determine what adverse effects, if any, pigs would sustain if they were not fed at week-ends, but were given enough food on Saturday morning to carry them over until Monday morning. We discussed this question with some of our nutrition experts: they were not in favour of the idea; protein metabolism would be disturbed and growth would be impeded; the curvilinear relationship between the retention of food and the calcium content per calorie might have a small but deleterious effect, and so on and so on. The short point is that most farmers appreciate the difficulties of week-end feeding; the pigman wants his time off like the rest of us. So in spite of these gloomy forebodings this *ad hoc* experiment ought to be done, not only for pigs but also for other farm animals. Even if the animals lose a little weight on Sundays because they have eaten all their Sunday ration on Saturday morning, the economic and other advantages of the week-end off may easily counterbalance these losses in efficiency. This is a good example of the class of problem which I believe we should tackle more actively; and there are hundreds of them. When these experiments are done they will undoubtedly provoke questions of fundamental as opposed to applied importance, and it is at this stage that the nutrition experts will be specially valuable. The economic necessities of practical farming produce questions which the research worker in his streamlined Institute may not have occasion to think of. The only way we can get these questions and answer them is by talking to the farmers. The only way the farmers can gain confidence that our answers to their questions are right even when, as sometimes is the case, they think they know the answer beforehand, is by contact with the scientists, respect for their



abilities and by discovering that it pays to follow the scientists' advice.

You may agree that it is fair to classify scientific research workers into four groups: the very good, the good, the competent and, shall we say, the not-so-good. It is about the last group that I should like to say something. Should these people, who often are conscientious and careful workers but without much imagination or drive, be allowed to scan a list of scientific subjects soon after graduation and almost at random select one to work on, for nobody's benefit and for an indefinite period? If this individual is working in an academic post at a university, the answer is clearly Yes. People often say it is impossible to predict in what way scientific research may prove to be of practical importance. Though this may be true as regards the work of a good scientist—and I shall give an example of this later—is it likely that the work of a not-so-good one, let us say, the morphology of fossil fish, will prove to be of great scientific or practical value? In Great Britain today, with our severe shortage of scientific manpower and materials and the vital necessity of producing more food, I believe there is a case for trying to influence such persons' work into more profitable lines than the morphology of fossil fish. After all, there is nothing new in this idea. During the war distinguished physiologists, who normally worked on such recondite questions as the properties of the membranes of single nerve fibres, were persuaded to work on such unfamiliar subjects as radar, to which they made most important contributions. Now I believe that the more efficient production of food in this country is just as important as the more efficient production of swept-back fighters, the only difference being that more efficient food production is a problem which is always with us, while mercifully the more efficient production of war weapons waxes and wanes with the times. If, during the war, great scientists made these important contributions in fields that were entirely strange to them, should we not seriously consider whether the not-so-great among us could not devote some part of their time to lines of work of vital importance to the country now?

*Order in which short-term problems should be tackled.* But before we can do much about the previous question we have got to decide what the most important short-term problems are, and here we are up against the difficulty that so far we cannot classify even in an approximate order of priority the problems which most urgently require attention. We do not know, for example, whether potato-root eelworm costs the country more than mastitis; we do not know whether mineral deficiencies cost us more than pneumonia in pigs, or bracken, or scrapie, or blight in potatoes. This classification of problems in order of priority is one of the most important questions we have to face and it is



one that can best be dealt with by a central organisation containing scientists and, of course, statisticians. I hope we shall have the answers to these questions so far as the main agricultural commodities are concerned, within two years. When we have got this information we shall be able to plan short-term research efficiently, without interfering with the longer-range and purer scientific work to which I have referred already. And one can say now that this intensification of work on short-term problems is going to cost money for land, buildings, animals and men. Even as far as we have already got in tackling this problem of priorities, it has become clear that, in terms of the annual values of our principal agricultural commodities, our research effort is exaggerated in some fields and inadequate in others. This can be rectified but it will need public-spirited co-operation from our Research Institutes throughout the United Kingdom; it may involve some hardship, in the sense that people may have to devote some part of their time to tackling problems which, if not strange to them, are at any rate not precisely what they have been working on before. I hasten to say that this does not involve any regimentation or pressure, both of which are so distasteful to all of us. It does, however, mean that the Treasury, who control our expenditure on agricultural research, are in future likely to be more sympathetic to our continually increasing demands if we can put our hands on our hearts and say that such and such a piece of work is of direct and immediate importance to the country in increasing the efficiency of our national food production. Much of the work of our Service could not possibly be held to come into this category, though we shall continue to subsidise it as before, because one cannot live only on past capital. One must continually create new scientific capital. But if, as I hope and believe, we shall in the years to come be granted more money for agricultural research—at present the amount we spend each year is a ridiculously small fraction of the annual value of the commodity, 0.3%—we must be able to justify the increased burden on the taxpayer. We shall be able to do this without the slightest difficulty, because even now it is painfully clear how many urgent problems there are which, when tackled, will produce answers of national importance.

*Organisation of agricultural research.* During the war one of my duties was to study the structure and organisation of a part of the German Secret Service. This Service was so well organised that we knew the room numbers of most of the members of their Head Office and, in some cases, even where the numbers were on the doors. This information helped us in our interrogations of enemy agents because they often thought that if we knew the numbers of the rooms their bosses worked in, there was really no point in their trying to hide anything

from us. At the same time, it used to be said that as it was impossible for anyone, even in *this* country, to understand the confused structure of our own Intelligence Service, the Germans were bound to have more difficulty in combating our activities than we had in combating theirs.

If the plants, farm animals and germs which we cherish and fight could speak, I wonder what they would say about the structure of our agricultural intelligence service; for in a sense the farmers represent the nation's armed forces, while agricultural research represents the Secret Service, both from the point of view of helping our agricultural allies and destroying our enemies. Would the plants, germs and animals say that the Service contained brave, boastful but ineffective men like Otto Skorzeny, the much vaunted German saboteur, whose main contribution to the German war effort was the doubtful one of returning Mussolini to Hitler; or would they realise that our Service contains dangerous but deceptive men like Dr. Kenneth Smith who, though head of a unit for studying plant viruses, has recently dealt a mortal blow to the clothes moth? At any rate they would be as confused by the organisation of agricultural research in this country as the Germans may have been by our Intelligence Service during the war. They might perhaps ask each other, even though we may know the value of decentralisation, why an Institute like Long Ashton has a group of distinguished men, including eminent scientists, called a Committee or, more often, a Governing Body, to look after it; and, at the same time, another group of distinguished men, again including eminent scientists, called the Agricultural Research Council, also looking after Long Ashton: but apparently not for the same reasons as the Governing Body. They might ask why, when Long Ashton wants a new building, both these bodies are consulted independently and, after agreement has been reached, two other bodies, composed of equally distinguished people, the Ministry of Agriculture and Fisheries and the Treasury, also have to be consulted and give their approval to the project. The birth of a building in these circumstances is bound to be slow and painful, and it may surprise the plants and animals that a building is ever born when the parents are so polygamous and heterogeneous. One could of course explain the situation by invoking the aid of historical accidents and evolution. But one cannot escape the conclusion, however much we wished to avoid the streamlined inefficiency of Nazi Germany, that the present organisation of agricultural research in this country is clumsy, irritating, and ripe for rejuvenation.

One result of bad organisation is that it fosters a comparatively new but virulent disease in British public life, the committee system. Smallpox and dropsy have disappeared, and in their place we have this new and insidious malady which, with apologies to any classical scholars there may be in the audience, I might call boulitis. As no

bacterium or virus has been shown to be the causative agent, as scientists somewhat ponderously put it, we may suspect that broulitis is a deficiency disease which Professor Wallace, Dr. Hewitt and Dr. Nicholas should investigate with their recently developed ultra-spectroscopic methods of bioassay. But perhaps there is a simpler but less elegant solution. Shortly after we went off the Gold Standard in 1931 I said to the late Lord Keynes:

'Why did you say it would be wrong for us to go off the Gold Standard?'

He replied:

'I made a mistake.'

Should we not try and emulate Keynes—on a lower plane of course—by sometimes risking a mistake, using our minds and coming to our own decisions, rather than always relying on collective recommendations beautifully roneoed in an infinite series of minutes, which ceaselessly circulate throughout the country. It has been estimated that this disease, our committees, costs us half a million pounds a year. As one grows older it seems inevitable that one should sit on more and more committees, and from time to time become Chairman of some of them. All of us who are in these unenviable positions would, I believe, be helping our country if, twice a year, we were to ask ourselves, 'How many of the committees with which I am concerned should be liquidated, and how can I help to finish the job?' During the last twelve months, thanks to the assistance of (among others) Lord Radnor and Professor Sir David Brunt, the Agricultural Research Council has succeeded in liquidating four of its thirty-six committees. We are going to keep it up even if some people wrongly think we are ignoring those subjects which previously were the victims of these happily defunct bodies.

That concludes what I have to say about the ways in which I believe our agricultural research service could be improved. Let us now turn for a few moments to the credit side. It is difficult to know what to mention and what to leave out. Should one mention, for example, that Dr. Mattick, of the National Institute for Research in Dairying, saves the cheese industry £100,000 a year by his discovery of how to control the bacteriophage which parasitises the bacteria essential for Grade I cheese manufacture; or should one mention the work of the Moredun Institute in Scotland in controlling and curing braxy, louping-ill and lamb dysentery in sheep, or milk fever and pine in cattle, which saves the country at least £300,000 a year?

Grass. One of the most fascinating combined operations of the Agricultural Research Service, combined because institutes in England, Scotland and Wales are all concerned, involves grass. The growing of

good grass, that commonest but perhaps most valuable of all our agricultural commodities, has been the subject of a combined operation by a number of famous agricultural research institutes. First, Rothamsted, with its classical work on the effect of fertilisers on the growth of grass during the last hundred years; secondly, the development of improved types of grasses, notably by the Welsh Plant Breeding Station at Aberystwyth, at the beginning of this century; thirdly, the application of Rothamsted's and Aberystwyth's results to the farm and its management, by the Grassland Research Institute at Drayton and Hurley; fourthly, the work of the Hannah Dairy Research Institute in Scotland on gains in grass yields following the use of nitrogenous fertilisers; and fifthly, the utilisation of grass by farm animals, which has been and is being investigated so successfully by the National Institute for Research in Dairying at Reading, the Grassland Research Station, the Hannah Dairy Research Institute and I.C.I. at Jealott's Hill. This work, together with simultaneous researches on grass conservation, has had a profound effect on British agriculture and could have an even more profound one if we cared to put into practice what we now know. In the United Kingdom we produce on an average 100-150 gallons of milk per acre of grass grazed by the dairy cow. With reasonable management and increased use of fertilisers this figure could easily be raised to 300 gallons per acre. Ideally it could be increased to 500 gallons, but this is a dream for the future; but the figure of 300 gallons could be achieved now. This means that we could feed our dairy cattle in summer on half the acreage of grass that is now used for this purpose. Although it is not the function of the Agricultural Research Service to make observations about what should be done with the products of their work, I cannot resist making two comments on these figures. First, I have given the products of our united research effort in terms of gallons of milk per acre. Although nowadays it is fashionable and no doubt desirable to think in terms of meat rather than in terms of milk, cheese, butter and cream, it is worth remembering that one can get three times as much human food from an acre of grass *via* the cow and her milk as one can by any other known method. The cow is a remarkable converter. The second observation concerns the farmers. We believe that the top ten per cent. are already doing what is necessary; the bottom twenty per cent. will be extremely difficult to persuade to do what is necessary; while the middle seventy per cent. require further inducements of one sort or another, or further education. How this education is to be speeded up and what form of inducements would be best, seems to be a problem which one cannot and should not try to solve by inspired guesswork or empirical decisions. It requires the type of Operational Research which Blackett and others carried out so successfully during the war.