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Farrer Memorial Oration

Phytotrons, Spectrophotometers and Productivity

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In accepting the great honour of the Farrer Memorial Medal, I am conscious of how much I owe to others. Any contribution I have been able to make to agricultural science has depended very largely on collaboration. I have been fortunate to have had the help of students, members of Universities, officers of C.S.I.R.O., officers of State Departments of Agriculture, and many others. This award, then, is not really an honour for me alone but should be divided among many. To all of them, and to the Trustees of the Farrer Memorial Fund, my sincere thanks.

Any biologist who looks at the problems of living organisms as a whole must still feel as Newton did when he described himself as a boy playing on the seashore while the whole ocean of truth lay undiscovered before him. The most any individual can do is to take some small area of that huge ocean and try to explore it with energy, intelligence and imagination. Sometimes he consciously attempts to control it for the good of his fellows and sometimes, perhaps all too rarely, he has the great satisfaction of seeing practical results of such exploration.

Farrer's Life and Work

William J. Farrer, who died almost 60 years ago, was such a biologist. He had come to Australia after taking the Mathematical Tripos at Cambridge because his intention to go on to study medicine had been thwarted by ill health (Russell, 1949). It was a far cry from the placid waters of the Cam to the valley of the Murrumbidgee, but this young man was soon established in a job as tutor to the George Campbell children at Duntroon and later he worked at other stations in this area. In 1873, only a few years after his arrival, he published his first paper, 'Grass and Sheep-farming—a Paper Speculative and Suggestive', which showed his remarkable certainty that scientific research would improve not only agricultural practice but also pastoral activities. This far-sightedness is all the more remarkable when we remember how little science was applied to agriculture in the early eighteen-seventies—the agricultural colleges, Roseworthy, Hawkesbury and Dookie, were not established for another two decades—and how much inertia or resistance opposed academic or theoretical studies of pastoral matters. Farrer said in his paper: 'Those who affect to despise theory would do well to recollect that a function of theory is to examine the foundations of practice and, by this means, to modify it and extend it advantageously.' This statement summarized an attitude which, a few years later, he was able to put into effect.

After a period as a surveyor, Farrer had married and gone to live at Lambrigg Farm on the banks of the Murrumbidgee River near Tharwa, only a few miles from here. There, in 1889, he began the imaginative experiments which led to the development of new commercial varieties of wheat better suited to Australian conditions. For many years, his variety 'Federation' was the most important and widely grown variety in Australia. By the time he died in 1905, he had shown how his examination of the foundations of wheat growing had led to spectacular modifications and extensions of this practice in Australia. Indeed, he not only changed the wheat for commerce but he also changed the landscape, for his browner varieties replaced the more golden ears which had been grown up to that time.

Agricultural and Pastoral Development and Control

Since Farrer's time, agricultural and pastoral activities have continued to change the landscape and to increase our national prosperity. Not only has the sheep, our principal source of revenue, been carefully nurtured with our detailed and enlightened knowledge of its nutrition, physiology and reproduction, but important successes have been achieved with crops which at one time were not regarded as possible in Australia. Poor scrub land and mallee country have been brought into fertile production of a number of crops with irrigation in the Murray and Murrumbidgee valleys. Irrigation has been extended to the monsoon tropics where the Ord River project is now a reality. We still have much potential for development in the rich coastal regions where more intensive activity could produce much greater quantities of various products if the markets demand it. In short, we have pastures developed on steep hillsides, useful plants in what was jungle, fertility in mineral deficient areas, water in the desert, and increased productivity everywhere—many developments which were almost unbelievable a few years ago.

All these developments depend on our increasing ability *to control our environment.* Fortunately, we have simultaneously developed the attitude that proper control of our resources is as much a challenge to the intelligent today as was exploitation to the courageous a century ago. Two examples will serve to illustrate our need for proper control. My first example concerns our attitude to the centre of the continent, where the highly unreliable rainfall averages something under eight inches and there is a precarious balance between plant, soil and climate. Even small upsets at any time, such as occur in the natural cycle of drought, may produce changes which are sufficiently catastrophic to last for years. Mulga destroyed is not replaced by any perennial which is as effective in holding the topsoil which begins to move in wind when the plant cover has gone. Overgrazing can certainly destroy plant cover in some areas. We suspect that it destroys not only the perennials but also the ephemerals which may be prevented from seeding, so that bare areas persist after rain instead of being characteristically covered by the 'herbage', as the ephemerals are termed outback. We have evidence from the work on saltbush and bluebush areas at Koonamore in South Australia that absence of a topsoil blown away following denudation of plant cover by grazing will persist for a very long time. It has been said that it takes 300 years elsewhere in the world to generate one inch of topsoil; certainly negligible amounts of topsoil have regenerated in the 37 years that the experimental plots at Koonamore have been protected from commercial grazing animals. The old exploitation, with complete disregard of the consequences, has largely gone, but the experience of much station country in Australia is a warning against the use of land which cannot be maintained efficiently. I might have taken other examples of bad use of land even in high rainfall areas. How unfortunate it would be to develop either pastoral or farming land on an extensive scale which may be so uneconomic as to go out of production with an irrevocably altered natural habitat.

My second example of the need for care is in the use of agricultural chemicals. Control of insect pests and of weeds has been made possible by the development of agricultural machinery to give large-scale, high-powered treatments. The spray plane is now a common sight and whole areas can be placed under blanket sprays. But what is lethal to an insect pest or a weed may have effects on other organisms as well. Everyone should read 'The Silent Spring' (Carson, 1963); even if one believes that the style overstates the case, I understand that the factual material is undisputed. We are rightly, as the result of such warnings, adopting more cautious attitudes to the indiscriminate use of pesticides. With increasing knowledge, particularly of the ecological balance of organisms, such controls are being used with greater care and with much less danger. We are learning to control our controls.

My first proposition is that, as the result of all this thought, we are aiming at a more intelligent control of productivity, with minimum upset of those areas or natural populations where no economic production is likely. Perhaps our proper aim can be stated as maximum production with maximum efficiency in minimum areas of really suitable land. It is significant that a symposium of this Congress is entitled 'Optimum Land Use in the High Rainfall Zones of Australia and New Zealand'

Plant Physiology and Biochemistry

I have named the phytotron in my title to symbolize the contribution of plant physiology, and the spectrophotometer to symbolize the contribution of biochemistry. Both these disciplines combine with other sciences in our attempts to increase efficiency in production. The main theme of my address is to consider what these two branches of plant science can do for the major problem facing humanity—the welfare of man himself. The phytotron—that large machine for growing plants, as the first one was named—began in a modest way at the California Institute of Technology, and there well controlled conditions were used to study plants in small numbers (Went, 1957) Bigger and better phytotrons have now been designed and are being put into use with a wide range of conditions for experimental plants. They are expensive to establish and to run—on the standards of expense usually associated with biological research, though not expensive by physicists' standards! Sometimes it has been argued that the same money is more profitably spent on field studies, an argument which is dying as the success of collaboration between field and phytotron becomes more apparent. Both are necessary; both have their places in industry, in the C.S.I.R.O., in Universities and Government research institutions, here and overseas. The advantages are considerable: plants can be maintained throughout the year and not merely in the normal growing season, flowering times can be adjusted and previously impossible hybridizations can be made; several generations can be grown in a year; particular stress environments, such as heat or cold, can be applied at will; standard plants can always be available. All these advantages, leading to increased understanding, should enable us still further to control our environment advantageously and refine still further the efficiency of our productivity.

My faith that further understanding of the physiology and biochemistry of plants will lead to increased efficiency is based, like all faith, on experience of the past. Let me remind you of that physiological discovery of the naturally-occurring growth substance in plants—auxin. The main features of the story are simply told. Following Darwin's leads, various workers showed that control of bending in plant growth was probably due to a substance controlling cell enlargement; its presence was demonstrated conclusively by Went and it was subsequently identified as indole acetic acid. This discovery led to the investigations of analogous synthetic substances and opened up the whole field of plant growth control by artificial growth substances applied to practical problems ranging from death of weeds to increase in fruit set.

One of the most exciting physiological discoveries in recent years may have great practical importance, but it is too early to say. A bright blue pigment, phytochrome, has been shown to be responsible for the perception of light by plants. Borthwick and Hendricks and their associates had predicted the occurrence of this pigment from the responses of plants to different wavelengths of light. They have now extracted the pigment and confirmed its predicted properties. In the red part of the spectrum, at 660 mμ wavelength, phytochrome exists in the P_{730} form. Change the radiation to the near red, i.e., that near the limit of vision at 730 mμ, and the phytochrome changes colour to the other form P_{660}. Leave P_{730} in darkness and it reverts to the P_{660} form with a half time of about 30 minutes; it could therefore be a central factor in the plant's sense of timing after a period of light. Experiments with plants at different wavelengths show that it certainly controls a number of apparently unrelated plant growth phenomena, among which are flowering, seed germination, and etiolation. It is therefore a major factor in the plant's interaction with its environment. It is a protein and is almost certainly an enzyme which triggers the mechanism leading to various kinds of development in plants (Hendricks and Borthwick, 1963). The work is an extremely elegant example of plant physiological experiments leading to a biochemical basis of a central control mechanism in the plant itself. As I said, it is too early to say where such knowledge might affect agricultural practice and I am not going to try a prediction, but I shall be surprised if such a fundamental discovery does not eventually have important application.

Discoveries in plant biochemistry, often dependent on my symbolic spectrophotometer, have been made in their hundreds in recent years; many have been under active discussion in the last week. We know much about the nucleic acid code and its relation to genetics, protein synthesis and cellular development. We have a fairly clear picture of how energy transformations occur in living cells and, thanks to the electron microscope and the centrifuge, we know what the different parts of sub-microscopic structure are doing. I would like to mention only one example—the recent work on protein synthesis in the wheat grain. Some of the last work done by the late R. K. Morton, with his collaborators, showed that the storage protein in wheat is synthesized in special bodies in the cell which can be isolated and retain some of their properties of protein synthesis from amino acids (Morton and Raison, 1963). Better understanding of protein synthesis in wheat would have been of particular interest to William Farrer because he was concerned with the connection between the quality of flour and the protein content of the grain.

Fundamental and Applied Research

I have mentioned only a few examples of the many discoveries of plant physiology and biochemistry. We all have faith that this increase in understanding of physiological and biochemical mechanisms will contribute to desirable practical applications in the future. Despite the excitement of scientific discovery, we are often disappointed in the slowness with which biological knowledge or understanding leads to Farrer's ideal of modification and extension of practice. Speaking as a rather academic scientist who has tried to contribute some understanding of applied problems and has had the responsibility of seeing that applied research is carried out, I am acutely aware of the gap between basic understanding and modification of practice. We must all have seen evidence for the old gibe that the agricultural scientist does years of research to finish up with a detailed explanation of what the farmer had found by trial and error anyway.

There are a number of reasons for gaps between theory and practice. Some are due to muddled thinking. I worry about the common fallacy in many young people's minds that pure research is more meritorious than that which has some practical benefit. Let me mention two examples: a young man, employed by an institution supported by taxpayers' money, told me last week that he 'feared' that some 'pressure' was being put on them to undertake some applied problems! My second example comes as comment from the leader of one of our few industrial biological laboratories where they are both doing fundamental work that a university would be proud of, and solving practical problems of the industry. When he interviews young candidates for jobs, many of them cannot get out the door fast enough when they are told that they will be expected to take an interest in practical problems as well as in fundamental work. In an audience of this type, I am sure I am preaching to the converted when I speak as I do, but I am concerned that anyone should doubt the desirability of engaging in applied research. Like Ian Clunies Ross, I believe that it is more difficult to solve a practical problem and see its application to industry in the way that Farrer did than to continue to make discoveries in plant physiology and biochemistry. Success is correspondingly more satisfying. Pure research is easier and more follow the easy line for simple reasons. If you have a practical goal, you must keep striving towards it no matter what the difficulties en route. In pure research, you can always change your direction when the going gets tough and, as the number of goals is almost unlimited, you will arrive at one. If we are intellectually honest, we should regard this as a failure, but we rationalize it. The new goal was reached by following 'a promising new line which developed in the course of the work'! Obviously, science must develop in this rather untidy fashion and we shall not change it. We might, however, give active thought to better rewards for those scientists who, like Farrer, carry their work through to practical applications. I have a profound respect for colleagues in C.S.I.R.O. or State Departments of Agriculture or Forestry, or wherever they may be, who have done so much to see that practices in the rural industries have been improved by the application of scientific and technical knowledge.

I worry, too, about the fallacy that any research is worth doing. I like the definition of research which came from a Russian joke that I heard at a recent international congress: 'Research is satisfying your own curiosity at the government's expense.' Clear thinking about priorities in research is essential. Let me put this bluntly. Some are successful in research because worthwhile scientific discoveries are made and recorded in the usual scientific journals; such men are adequately rewarded by the recognition of their peers, election to learned societies, accelerated promotion and prestige in the eyes of the public.

Others are successful in research because their work increases the productivity or more economic operation of an industry: such men are not always accorded the recognition that their important work deserves and we should try to improve our ways and means of rewarding them. This does not mean that merely engaging in research is meritorious in itself—a common fallacy. I remember a young man who complained to my wife that he was paid less for doing research than a bricklayer for laying bricks. He was shocked when she suggested what might have been true at the time that the bricklayer was probably more use to the community! If such people continue to progress slowly in research, they ought, as Professor Donnan said, to find something they can do better. Often such people's technical skill will enable them to do a first-rate job in some other work which would be better than a third-rate job in research.

I believe that there is another impediment to good research relating to primary industry where so much depends on biological training. Simply stated, my proposition is that the research life depends on skill in solving scientific problems; skill in solving problems depends partly on innate ability but largely on practice; in most biological subjects (genetics is a notable exception), our students spend too much time in absorbing knowledge, much of which is not used again, and too little of their time in practising solving biological problems. A student who has completed even first year physics has the sensation that he has had practice in solving physical problems; the student of the biological subjects often has not, even in his second or third year. Many get their first real practice in solving scientific problems in a fourth or honours year. In the scientific or technical career, how you think is so obviously more important than what you know (which quickly passes out of date anyway). We should provide more practice in solving problems. If we do, I believe that we shall both attract more capable people and develop more capable people. Such people will take the difficult practical problem in their stride or, if necessary, will delve into fundamentals to solve it.

Productivity and Food Supplies

Such biologists are already contributing to our aim which, on a national basis, must be that of maximum productivity with maximum efficiency and minimum use of the land which is marginal or unsuitable. Progress is already good and rapid.

On an international basis, we have problems of a different order which have been stated clearly by many scientists. We have often times been reminded by my friend, Dr. O. H. Frankel, that we are facing a critical shortage of food for the increasing population (Frankel, 1963). Already probably more than half the world's population suffers from some degree of deficiency of the essential amino acids—usually known as the protein shortage. In this Congress, I have listened with interest to the discussion of this problem on Australia's doorstep—among the Chimbu in Papua-New Guinea. Certainly the supply of essential amino acids, and later of vitamins, will be the most critical factor facing us in the immediate future. This deficiency will grow rapidly worse as the population increases and we cannot meet increased demands for the amino acids with current techniques. Obviously the production of essential foods must be carried out in centres nearest the areas in which the demands are greatest. Our reasonable practical objective might be to free the world from hunger and make the basic foods—the daily minimum of carbohydrate, fat, protein and accessory factors—the birthright of every man, woman and child. While population control by appropriate methods is equally important, increases in essential foods are urgent now and likely to become critical. All will agree that letting this solve itself by allowing children to starve to death or to suffer from deficiency diseases is ethically unacceptable and morally reprehensible.

How can we ensure the adequate nutrition of all mankind? First, by making all the scientific improvements in conventional agriculture that are possible. Second, by the most accelerated educational programs that are practicable to improve the efficiency of the peasant farmer in the countries most needing the improved production. None of this is new; it has been said often. Third, by imaginative attention to new methods of food production entirely different from anything we are doing today. This I believe is the under-emphasized challenge to the plant physiologist and particularly to the plant biochemist. Almost nothing has been done, but the work of Pirie points the way. The primary source of the essential amino acids is the green leaf. Animals grazing obtain the primary amino acids from this source, alter them, and concentrate them into the forms which are most acceptable in human nutrition, but something like 85 to 95 per cent of the amino acids from the plant leaves is wasted in this process (Stahmann, 1963). Pirie (1961) has shown that the leaf proteins can be extracted in concentrated form from leaves themselves, and Morrison and Pirie (1961) have shown that large-scale production is possible. Cheese from leaves, without the intervention of the animal, is already a possibility: but this is only a beginning. Much work will be necessary to improve the amino acid composition, to increase the palatability, and to secure maximum efficiency of production and of extraction.

Each time I have spoken about these possibilities in suggesting that there might be a revolutionary change in our methods of producing food, I have had several standard reactions. Some accuse me of dealing in science fiction, or of being in 'cloud cuckoo' land. The reply is obvious: we have seen the science fiction of the physical science of yesterday become the fact of today. Some express the hope that they will not live to see the day when they cannot sit down to a good juicy steak: of course they won't. We must concern ourselves with the people who have never seen, or are never likely to see, the equivalent of a steak: for such people, we need the handful of the appropriate amino acids to add to the daily ration of rice, sweet potato or other common carbohydrate, which incidentally are relatively easy to produce. Finally, there are those who see such revolutionary methods as a danger to the economic structure; increase in prosperity is more likely in well-fed communities than in those set back by hunger, or inefficient due to malnutrition.

We have not begun to use the profound knowledge that we now have of enzymes and enzyme systems for any large-scale production. Enzymes might appropriately be used with plant protein extracts to modify the amino acid contents. The kind of problems to be overcome is no greater than the biological problems which arise in cheese making, or brewing, or penicillin manufacture. Further, if we can intensify food production, there are three obvious advantages which will work in its favour:

- Control of processes on factory scale or, at most, on small, very intensively farmed areas becomes easier than control of those which range over wide areas of diverse microclimate and uncertain weather.
- The less land we need for production, the less we are likely to spoil by upsetting the existing ecological balance.
- The more intensive and better controlled the food production methods, the more it will be in the hands of technicians and the less farmers would be engaged, thereby minimizing the difficult problem of educating the backward to apply maximum improved farming methods.

In toto, my suggestion is an extrapolation of what we are doing in the way of control. Our trend is to produce more efficiently on better land under better control with fewer people engaged. Bring this to something more like a factory scale process, and we raise this most important human activity from an art, mercilessly dependent on the elements, to a worthy technology.

We now understand the mechanisms of synthesis of many compounds. On a laboratory scale, we can control photosynthesis in isolated chloroplasts. Does this open the way to large-scale biochemical engineering in which we could, under controlled conditions, produce quantities of amino acids and other intermediates of metabolism? Every plant physiologist is concerned at how much water must pass through a plant to produce unit weight of dry material. In a world where water for agriculture is a widespread problem, could we manipulate chloroplasts isolated from plants for photosynthesis? Such a controlled process might lead to more efficient use of areas of land. The amount of light energy which is fixed is a small fraction of that which falls. Such a process might also lead to a more efficient recycling of the mineral elements which must, from the nature of things, be used so inefficiently in agriculture. I suspect that in centuries to come our broadcasting of superphosphate—so absolutely essential today—will look a crude and wasteful way of achieving the phosphorylated compounds associated with energy transfer in bio-synthetic reactions.

This is the period of the Freedom from Hunger Campaign, and soon a five-year International Biological Program will be launched under the auspices of the International Council of Scientific Unions. We should think out a program of research specifically designed to use existing biochemical knowledge to step up useful biochemical reactions from laboratory flasks to pilot scale production, and eventually the manufacture of some essential food constituents will be possible. I believe that imaginative applied scientific investigations in this sphere constitute an exciting challenge to the biologist and biochemist. What can the biochemist, from his wealth of understanding of plant processes and enzyme reactions, contribute to a revolutionary method of making essential foods? At present, ideas are more important than money—though it is a strange commentary on our present world outlook that, in spite of all the money spent in physical research aimed at putting men and vehicles into space, Pirie has experienced difficulty in getting support for large-scale experiments on his techniques.

In this talk, I have suggested that the problems of feeding the world depend on increasing efficiency of control. I am satisfied that we must rise to the challenge of improving conventional agriculture and arriving at maximum use of suitable land. The trend is towards reduction of land worked, and decrease in numbers of workers, per unit produced. I have made the extrapolation that we might go still further with even more efficient systems of biochemical engineering to supplement, and perhaps to replace, some of the less efficient methods of producing specific food substances such as amino acids.

To do this, we shall require imaginative enthusiasts inspired, like William Farrer, with belief in the application of theoretical knowledge to revolutionize practice. Such men will do for world food supplies what William Farrer did so successfully in his day for wheat production in Australia.

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