The Agricultural Impasse

W. Gordon Whaley, *Director*

The Plant Research Institute, The University of Texas

THE EFFORTS OF ALL PEOPLES to achieve a place in the sun have turned around the land and what it produces. Among the underlying causes of war, no factor is of importance nearly equal to that of the relation of a population to the productiveness of its lands. This relation involves a complex equation between the utilization of ores, molecules, and atoms and the handling of plants and animals which supply the energy for living. No course can more surely invite disaster than one which fosters headlong improvement in peace and war of industries which consume the products of the land while it all but ignores our relatively poor success in handling the land and its living cover.

The source of man's energy is the green plant. Maintenance of his energy supply or its increase depends upon apt handling of his plants. If he fears the cutting off or liquidation of this supply he will fight for its preservation. The adequacy of the supply and its distribution is, in the last analysis, the key problem. Today we have an alternative to the age-old unbalance of the fat years and the lean, the haves and the have-nots, and the birth of countless millions of human beings to the misery of inadequate food—we can replace "farming" with agriculture, the science of handling the land and its plants and animals.

Great progress in such a revolution has already been made. American agriculture, quite as much as American industry, helped to turn the recent tide of war toward the victory. Many of the peoples of ravaged Europe and Asia owe both their freedom and their postwar survival to the American farmer. But we are moving too slowly. The pressure of war and postwar conditions has so increased the drain of soil fertility and the effects of erosion and inefficient culture that these and related factors are reaching an emergency status earlier than they would have, had a normal peacetime pace prevailed. In much of our supporting agricultural research, too, we have reached an impasse; and in this there is great danger, for the laws of energy will not indefinitely permit increase of demand, without parallel increase in the available supply.

From any long range view, further lifting of the world standard of living can only follow increases in agricultural efficiency. In fact, because of increasing population pressures, even maintenance of the present standard demands a very considerable increase in this efficiency. The Food and Agriculture Organization of the United Nations estimates increases (over previous production) of 21 percent for cereals, 100 percent for milk, and 163 percent for fruits and vegetables as minimum requirements for feeding the expanding world population by 1960. The land available for growing these increased crops has shrunk from deterioration and misuse. Moreover, the hope, often expressed, that such increases can be produced in tropical countries overlooks the severe limitations of these regions. In most tropical regions there is, so far as agriculture is concerned, relatively little usable land. There are no great prairies, and because of the topography only limited areas can be turned to the production of energy crops. Continued high temperatures and excessive rainfall restrict both the types of plants that will grow and man's efforts to grow them. Such conditions also often increase the magnitude of disease and insect problems to nearly uncontrollable proportions. The production of the tropics is destined to be of growing importance with respect to many crops, but the resources of the tropics are simply not such as to make the regions capable of assuming a major position in the production of the energy crops. We must look instead for means of greatly increasing the production of Temperate Zone farms.

The Basic Problem

In emphasizing the need for conservation, it is common to picture the land as a potential resource, a sort of agricultural safe deposit box from which man may draw fertility for his crops for an indefinite period. If he withdraws this fertility too rapidly, however, or allows the mineral elements to be washed away by erosion, he must move on to new land or, when new lands are no longer available, face ruin. All this is true, but it is a concept too limiting to serve as the basis of a scientific agriculture. It is necessary, instead, to think of a farm as a factory and the farmer's managing efforts as attempts to turn raw materials into fine goods, soil elements into plant products. The goal is the most rapid practical turnover of fertility, the highest production consistent with continual availability of raw materials. Many of our soils lack certain essential elements, but most of them contain, in practically inexhaustible amounts, all the required elements except nitrogen, phosphorus, and potassium. Agriculture becomes then in part a matter of supplying phosphorus and potassium as crude chemicals in the right amounts, fixing nitrogen chemically or by the activity of microorganisms, and providing in one way or another such additional elements as may be required to enable the protoplasm of the living plants to manufacture, in mass production, fine goods.

This is the salient fact. In practice the operation is exceedingly complex, and we are in discouraging ignorance of most of its components. The soil in which many key processes go on is a variable and little understood medium, the steps in the functional processes by which plants convert materials from soil and air and the energy of light into fine goods are nearly complete mysteries, and the intricate balances of soil, plants, and climate have hardly been approached. We know only enough to realize how much needs to be learned and to recognize that if we had more knowledge we could make much better adaptions, maintain much better balances, and control the hazards more effectively.

Effective long range use of the soil with efficient cultural practices and adequate control of diseases and insect pests are pressing agricultural problems, but there are others. True to the American pattern, we have made greatest advances where mechanization fits into agriculture. In many crops all operations from sowing to harvesting are now carried out by machines. One effect has been to call for greater efforts to improve plants; for the more highly developed agricultural practices become, the more urgent is the need to produce more efficient plants—plants which can produce more fine goods per unit of raw material, or which, better fitting the pattern of mechanized farming, can produce more cheaply.

Advances in soil practices, control of plant diseases and pests, mechanization of farming operations, and plant improvement already have been great, but not so great as not to be dangerously dwarfed by industrial advances and the factors of population increase of the past several decades. Thus, an unhealthy situation arises. The more the efficiency of industry is increased and the higher wages it can pay, the more difficult it becomes for the less efficient agriculture to find labor and capital. The more efficient industry becomes, the more it expands and the greater becomes the need for new or better agricultural products; and even apart from industrial development, the more population increases the more people agriculture has to feed and clothe.

In any analysis of American agriculture's besetting difficulties a single fact stands out above all—the need for a coordinated and effective program of research.

Agriculture is still, by and large, in the hands of individual farmers with access to relatively little capital as compared with large industrial corporations. Even in good times, individuals are rarely capable of supporting research and experimental development. Then too, the farmer's immediate problems are generally tied to a particular piece of soil in a specific climate, and to the caprices of the weather. It is difficult for him to think in the broader terms which characterize modern basic industrial research.

What is termed industrial research really represents two usually more or less distinct elements. The general fact finding is perhaps accomplished most often by investigators outside the industrial laboratories, by the workers in "pure" sciences, most often in university laboratories. Industry carefully currycombs the findings of such investigators and then tests any interesting facts revealed. Agriculture has no adequate provision for either part of this operation.

The fundamental unit in agricultural research is a living plant or animal and its products. This fact gives a distinguishing breadth to agricultural research needs. Plants and animals are all composed of a common stuff and they all grow according to similar patterns. As a result, research on corn may prove to be just as important for the cotton farmer as for the corn farmer. This means that one has to think in specific terms of cotton or corn and at the same time in general terms of living organisms. The biologist investigating a specific problem in corn soon finds himself knocking at the closed door of what life and growth and sensitivity are. His field is far more mysterious, and, one suspects, far more difficult than those of his colleagues working with inanimate atoms.

The peculiar status of agricultural research needs has been recognized for some time. It was in response to this recognition that the plant and animal research branches of the United States Department of Agriculture and of the various state experiment stations were organized. We have in the correlated branches of these agencies a structure which is theoretically capable of dealing with both short range research, concerned with solution of the farmer's immediate problems, and long range research on fundamental problems. In actual practice, it does a good job at the former and shies away from the latter. The American taxpayer is putting millions of dollars annually into agricultural research. He is getting progressively smaller returns, and too few of these are the kind that will pay off in the long run. Why?

Federal and State Plant Research

So far as the national picture is concerned, crop plant research is centered in the Bureau of Plant Industry, Soils and Agricultural Engineering of the

United States Department of Agriculture. This agency is composed of several hundred professional plant scientists, operating on a budget which, counting both direct and indirect appropriations, approximates ten million dollars a year. Its staff probably contains between one-quarter and one-third of the country's plant scientists, the remainder being distributed fairly equally among other federal agencies, the state experiment stations, and the colleges and universities.

Agricultural research is almost necessarily a ward of the state, and this federal bureau should be the dominant agency in plant research—stimulating fundamental research by its own research productiveness, on one hand, and guiding the application of newly discovered principles on the other. Actually, its accomplishments in the first field are considerably less than they might be, though at one time the Bureau played a much more important role than it now does.

Some of its deficiencies are fairly easy to indicate. Its staff is made up of a very considerable number of topflight scientists, but it also has more than a few mediocre persons who value the security of federal civil service retirement more highly than real scientific accomplishment. Unfortunately, the system is such that the potentialities of many of the best men are not realized. Too often the civil service system and good productive research don't mix. This is a serious business, for it has driven many of the best of the wartime federal scientists out of the government service into industrial or university work, and usually away from pressing problems concerning the national welfare. Many factors are involved. One is bureaucratic insistence upon establishing organizational patterns without appreciation of the need for flexibility in research. Another is the too frequent reservation of higher grades for administrative employees. The effect of this is to make administrators out of the more capable men, and conversely to keep actual research activities in the hands of the less capable, or less advanced. Productive research is a coupling of ideas with experimental techniques. Only in rare instances can each component come from individuals at different levels of responsibility, separated perhaps by thousands of miles. Seniority, so necessary to maintenance of a good organization, often precludes obtaining the best qualified investigators for particular problems. The civil service efficiency rating system, though it is handled in general with attempts at the utmost honesty, often complicates the picture, for the employee's advancement chances turn directly upon it. It is probably possible to make estimates of the comparative efficiency of twenty-five typists working in the same office. In fields of scientific endeavor, however, such comparative ratings are literally impossible. You simply cannot measure a man's scientific productiveness in terms of his industry, his ability to get along with his superiors, the degree of neatness with which he conducts his work, and so on. What you have to evaluate is his brain power. What the civil service system evaluates is his capacity to fit comfortably into an organized pattern. The organization is the thing. If a man fits it—well, let's not worry too much about the fact that he likely is not the most brilliant man in his field. Some signs are appearing which seem to indicate fettered steps on the part of the Civil Service Commission to attempt some modifications of its system to deal with the needs of research agencies. This could be hailed as progress were it not that the organizational difficulties of the agricultural research bureaus would likely be self-perpetuating even without civil service.

But the personnel difficulties are not the only important ones. The present plant scientists of the federal system could turn out far more productive research if they were not hampered by the nature of the system itself. Let's examine the system.

A group of working-level scientists conceive a research project which they feel, on the basis of their specialized training and experience, may reasonably be expected to answer some questions which are bothering them in their attempts to improve a certain crop plant. They draw up an action plan, with all the necessary attention to organizational detail. The plan is reviewed by their division head and the chief of bureau, with his assistants acting as advisors. If approved by these men, it is sent on for approval by the Agricultural Research Administration. This is a superstructure agency whose functions include overseeing the work of all the Department of Agriculture research units and their cooperative work with state agricultural groups.

The concept behind the organization of the Agricultural Research Administration was that it should coordinate the efforts of different groups, eliminating unnecessary duplication, and in general tie together the work of federal and state agricultural scientists, and guide their research along profitable lines. It was a noble concept. With vision and a degree of freedom, the Agricultural Research Administration could lead all the country's agricultural research, stimulate important allied researches, and very effectively raise agricultural efficiency, not only in this country but over the world. It could be one of the greatest factors in our world leadership, for little we could do would be of greater benefit to mankind. Actually, the task of the Agricultural Research Administration is, at least in part, one of dignified pussyfooting. To it falls the responsibility for sorting out of the federal agricultural research proposals those which Congress will approve and shunting the others to the side lines. It is the liaison agency attempting to maintain long term productive research against the backdrop of Congressional political interests. It must see that only projects likely to have enough Congressional support or public interest are presented for consideration, for the support for the research comes, with a few minor exceptions, from the public treasury. The funds are authorized by Congress on an annual appropriation basis. These appropriations, woefully inadequate if judged against the magnitude of the problem, are relatively small items in the great Department of Agriculture Appropriations Act, which provides for the many important and unimportant services of the department.

Here is a major difficulty, for the basic problems of plant improvement are hardly likely to be intriguing to either Congress or the general public. The Agricultural Research Administration attempts to be certain that the scientists never use language which would sound as though they were talking down to the legislators. Hence its scientists, to start a research program, must sloganize it, must optimistically forecast great results from it, and report its progress in journalese. In short, prosaic scientific research, whose interpretation requires the precision of its own scientific terminology, must be dramatized—and in words of one syllable. The Agricultural Research Administration cannot permit an attack on problems that Congress might feel are too remote. Then there are issues to be attended to like the demands of the state agencies for research assistance. With such responsibilities the Agricultural Research Administration has little time for anything like deep-delving consideration of basic agricultural research needs. If the research structure is to survive with Congressional support, it must occupy itself with the questions of the moment. There is no time to worry about the farmer's grandchild.

Congressional interest in research projects introduces other trouble, too, such as making it much easier to go on spending money on something established than to institute anything new. Hence, the scientists have to try to extend already established researches and bend them around new needs, often getting only half as far as they should. And they sometimes continue researches that might have been stopped years ago at no great loss. This situation is particularly true where experiment stations have been set up. The presence of these stations, some of them very small, becomes important in the congressman's constituency. The Department of Agriculture budget supports several such stations which offer little but vain hope of ever serving any purpose that cannot be served more effectively elsewhere. Once in a while Congress gets too excited, or its prodders do, about some question involving agricultural research. When this happens, the scientists have to occupy themselves for a year or two with attempts to spend short term money effectively. Then Congressional interest in that particular problem wanes. The men in the research organization are left with nothing but the knowledge that a few thousand dollars a year for ten years and freedom to attack basic problems would probably have solved the difficulty. That the final determining review of research basic to the country's agricultural future should be in the hands of Congress is logical only if Congress seeks and is guided by the advice of the best specialists in the field. The record doesn't suggest that Congress operates in this way. Even if it did, the system would remain inadequate until the major part of the research could be relieved of the compulsive uncertainty that it reflects from the single-year appropriation scheme.

The federal plant scientists have another big handicap. Like the rest of the government machine of these United States, the Department of Agriculture is a bureaucracy—too large a portion of its staff has to be concerned with the functioning of the machine. And the scientist has to spend too much of his time making reports, reports, and reports on his organization for research. The research he cannot find time to do. A man who is good enough to get along soon finds himself occupied most of the time with reports on and justification of the efforts of those behind him in the line of ascent.

The agricultural research efforts of the states are conducted by the state experiment stations, which are affiliated with the land grant colleges. These agencies have many of the same difficulties as the federal system. They are too often staffed with people of insufficient breadth—partly because for many years most of the state experiment stations could pay only very low salaries, partly because the state scientists are under pressure by local legislatures and populations to direct all their attention to matters of immediate concern in the locality. The logical field of effort for such experiment stations concerns problems that are more or less local, having to do with the particular crops of a given state and their behavior and management under the environmental conditions in that state. It is generally considered poor economy for the state experiment station to allow its staff members to devote time to problems not immediately related to the needs of the state's farms.

The Evolution of the Problem

Many state research scientists and federal scientists share one characteristic that is somewhat less readily recognized than those we have been discussing, but that is perhaps the most important in an over-all consideration. This is their failure to foresee what might be called the evolution of agricultural problems and to prepare themselves to deal with the products of this evolution. When the great agricultural agencies of the nation were organized, the compelling problems, for the most part, had to do with handling crops in the field—determining the proper soil for a given crop, the best time of planting, the most productive spacing, and the most effective fertilizers. With the development of agricultural engineering, there were the added problems of building machines for planting, cultivating, and harvesting different kinds of crops under different growing conditions, and developing methods for the control of insect and disease enemies. The agricultural agencies were founded for attacks on such problems as these. The present status of American agriculture is a tremendous tribute to the achievements of the agricultural research scientists in meeting these problems and solving them effectively. Much remains to be done, but what is not clearly enough recognized is that by solving these problems so effectively these same scientists have opened up new frontiers of research, have pushed the research needs along to another stage.

We are now at a point where major agricultural advances can come only after we have gained much more knowledge concerning the fundamental biology of our crop plants. Improvement in field practices may be expected to give perhaps another ten percent in increased yields. It will result in saving some land and it will bring further lightening of the farmer's labors. This sort of progress must be continued, but major advances will come only if we supplement such investigations with enough basic biological research to enable us to begin to learn something more than superficial facts about how plants grow, how they reproduce, and what their relations to soil and climate are. The agencies which have been built up under civil service regulations are staffed almost entirely with the researchers to whom goes the great honor of having solved the first line problems. However, it follows from the very nature of these investigators' training and interests that they generally do not recognize or have an interest in the now basic biological problems. Hence, the agricultural research agencies are tending to neglect seriously the biological problems which are at the base of the next century's improvement in agriculture or even the next century's continuation of agriculture at its present highly productive level. They are neglecting these problems sometimes for lack of time to deal with them, often on the assumption that the colleges and universities will produce a generation of men better prepared to deal with them.

This last point needs examination in the light of the facts. The avowed purpose of most agricultural colleges is to train agriculturists. To this end their faculties are organized and their curricula developed; and to this end they are generally most successful. There is, however, much confusion between the requirements of agriculture and the requirements of agricultural research. Young men going through agricultural curricula are encouraged to stay on and work toward advanced degrees, and then to accept positions of responsibility in agricultural research. If they are to deal with the kind of problems which were the predominant ones in agricultural research during the last half century, they are generally adequately prepared. We shall need many more of them. But we also need a different type of investigator, a member of a group in step with the newer problems in the second line of research, with the fundamental biological and biochemical problems. It is a mistake to assume that a man trained in agriculture is qualified for this type of specialized agricultural research.

If the agricultural colleges must continue to devote their efforts mostly to the training of agriculturists, then where are the people capable of solving the more complex fundamental biological problems to come from? One might expect that colleges and universities which are less directly connected with agriculture should be in a position to train such people. Though they may be in such a position, too often they are not interested. Their botanists and zoologists tend, more or less purposely, to concentrate their efforts on investigations which have no bearing upon practical problems. Their chemists and physicists are busy responding to the demands of industry, which recognized long ago the need for continuing to harvest annual crops of fundamental researchers.

It behooves these colleges and universities to examine the thesis which guides their training of young men and women for productive lives. It behooves these institutions and the agricultural colleges to look further into the basic training requirements for agricultural research. It behooves a lot of professors to examine the relative worth of the problems with which they occupy themselves and a lot of practical-minded agriculturists to look far enough into the ivory towers to see whether anything in the way of useful knowledge may be written on the walls. Yale University and the University of Connecticut have recently established what may become a pattern for the training of agricultural research scientists. In a cooperative scheme, students are given two or three years' work in the fundamental biological sciences and directly allied fields at Yale and then they go to the University of Connecticut for a year or more to study application of the facts and techniques of these fundamental sciences to the problems of agriculture. Although it would not seem adaptable under most circumstances, this scheme recognizes the key to the problem. Agricultural research in the commonly accepted sense is not enough. It must be expanded to include far more biological research. In the case of plants and their products, we must direct major rather than incidental efforts toward unraveling the facts of growth, development, and reproduction, and the interrelations of these with soil and climate. When enough such background has been accumulated, then agriculture's practical problems must be reviewed against it.

To gain this critical knowledge we need to make certain changes. Our federal research agencies must be relieved of the requirement to concentrate their efforts on sure-fire, immediately solvable problems, while their scientists defend Congressional mandates for fundamental research. They must expand their research to determine how the green plant combines carbon dioxide and water to produce carbohydrate and then transforms the carbohydrate to thousands of useful substances, and to attack a selected group of other fundamental problems. This calls for a revision of the setup, a new deal from civil service, new types of Congressional authorization and support—all matters of federal government concern. But it calls for other things, too. There must be leaders who can lead, men who can appraise trends in all the sciences and interpret their significance for agriculture, who can recognize the gaps that stop progress, and furnish the individual scientists with charts for action. Such men are likely to come only after a renaissance in teaching and training has directed students in basic biology, chemistry, and physics toward agricultural problems. This calls for reorientation by the colleges and universities with direct or indirect interest in the field, closer cooperation between pure and applied science groups, and the development of programs which will train much more highly and educate more broadly at the same time.

The Significance of Meiosis in *Allomyces*

Ralph Emerson[1] and Charles M. Wilson

Department of Botany, University of California

and *Biological Laboratories, Harvard University*

TWENTY YEARS AGO Hans Kniep (*7*) described in the water mold *Allomyces* a life cycle and a type of sexuality which were previously unknown in the fungi. The cycle, which now serves to distinguish the subgenus *Euallomyces* (*3*), is outlined in Fig. 1. Sexual reproduction is accomplished

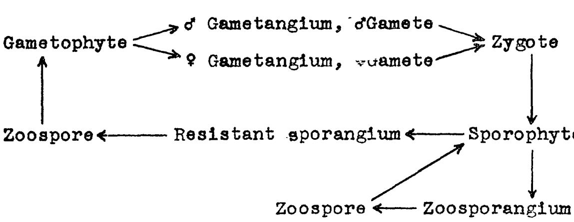

FIG. 1. Life cycle of *Euallomyces*.

by fusion of a small, motile, pigmented male gamete with a somewhat larger, motile, unpigmented female gamete. A year later, having discovered that the nuclei in sporophytic hyphae had about twice the volume of those in gametophytic hyphae, Kniep (*8*), postulated that meiosis occurs in the resistant sporangia of *Euallomyces* and that there is an alternation of haploid gametophytes and isomorphic, diploid sporophytes. Similar studies subsequently led Sörgel (*12, 13*) to accept this concept, and Emerson (*3*) presented genetic evidence, obtained from interspecific crosses, which gave strong indirect support for Kniep's interpretation of the life cycle. Although Hatch (*4*) had made a detailed cytological study of dividing nuclei in zygotes of *Euallomyces* and concluded that meiosis occurred at zygote germination, he later (*5, 6*) denied the validity of his own interpretations and accepted Kniep's hypothesis.

Kniep himself was quick to recognize the unusual possibilities which *Allomyces* presents for experimental investigations of sexuality and reproductive behavior, and subsequent studies by others amply testify to the keenness of his insight. A variety of basic researches using this phycomycete to investigate phenomena of apomixis, sex determination, irradiation action, nutrition, metabolism, and the physiology of spore-maturation, -dormancy, and -germination are under way in a number of laboratories in the United States at the present time. Nearly all of this work

[1] Guggenheim fellow at Harvard University.