available do not permit of any conclusion, but suggest that the Golgi body is involved in both phases of secretory activity. The thyroid gland contributes little to the elucidation of the precise role of the Golgi body, because its study is complicated by the problems of colloid release from the follicles, in which the Golgi material may also participate.

In regard to the function of mitochondria in secretion, an increase in their number is associated, for example, with the degranulating chromophil cells of the anterior pituitary of the rat²⁸ and guinea pig¹ during pregnancy, and after oestrone administration in the rat²⁸. It has also been noted previously that changes in the mitochondrial dispersion in relation to the state of granulation occur in the guinea pig. In the thyroid, Okkels³³, and Bailiff²⁶ observed a massing of mitochondria in the apical poles of the cells following upon the stimulation of secretion into the follicular lumen. Okkels³³, from his observations, suggested that the mitochondria acted as condensers of the secretion. Other workers have recorded changes in the mitochondrial morphology of the thyroid, following upon experimental treatments. Seecof⁴⁵, and Cramer and Ludford⁴⁶ described a filamentous condition resulting from stimulation induced respectively by partial thyroidectomy and low temperature. The same workers observed a conversion into a granular state consequent upon a reduction in cellular activity. These observations certainly indicate that the mitochondria have a role in secretory activity, but there is insufficient evidence to show its precise nature.

Finally, in the adrenal cortex there are no records of changes in the mitochondria and Golgi material which could be correlated with secretory activity. There is, however, evidence to show that when the activity of the cortex is stimulated experimentally, by exposure of the animal to cold, for example, there follows an initial increase of lipid material followed by a decrease, as the stimulation is maintained. Flexner and Grollman¹⁶ regarded this initial increase of lipid as being indicative of increased activity. On the other hand, Dosne and Dalton¹⁵, interpreting similar results, regarded the initial increase as a kind of compensation reaction, further increase in activity being accompanied by lipid loss. This interpretation is supported by the investigations of Bennett⁴⁷, who has produced evidence which indicates that the adrenal cortex of the cat may be conveniently divided into four zones, on the basis of their lipid content, and on their reaction to a histochemical test designed to show the presence of the keto-groups of the cortical sterones. He found that the reaction was practically confined to that zone of the cortex (consisting of part of the zona fasciculata) which is richest in lipids—this he called the secretory zone. Since it was found also that removal of the lipid with suitable solvents resulted in an inability to detect the keto-groups, he concluded that there was probably an intimate relation between the two, so that if stimulation of the cortex brings about a rapid release of the cortical hormone, the progressive disappearance of the lipid droplets is thus to be expected.

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AGRICULTURAL RESEARCH INSTITUTES AND THE FUTURE

By DR. B. A. KEEN, F.R.S.

THE organization and administration of agricultural research, in common with many other national activities, will receive critical—and, it may be hoped—constructive attention as a result of the War. Although the post-war structure of our agriculture cannot yet be predicted with confidence, considerable changes will probably occur, which should be accompanied—and so far as possible, anticipated—by appropriate new perspectives in the agricultural research, advisory and teaching organizations.

This article deals primarily with research and is further restricted to arable agriculture. Although other branches, such as animal husbandry and economics, also have their own vital problems, it is not likely that their framework of organization and administration will need to be vastly different from that proper to research in arable agriculture. Indeed, there are so many problems requiring joint examination that some organization to serve those needs that are common to all branches is not only desirable, but also essential.

If one looks back at, say, the last hundred years of research in arable agriculture, a clear pattern can

be seen, especially in Great Britain. There have been two well-defined stages, the second of which is already changing into a third. The picture is one of orderly and progressive development, and the third stage is a logical outcome of the second.

It so happens that the history of Rothamsted well illustrates the sequence. The first stage—the era of Lawes and Gilbert—was a frontal attack on a problem of practical agriculture: the manuring of crops. The method was one of simple and direct field experiments; the function of the laboratories was to produce a mass of expository analytical data on the soils and crops. It is true that valuable novelties were employed in the method of approach: the problem was detached from its context of crop rotations and agricultural systems by growing each crop continuously. The method well served its purpose, for it was quick, and it not only gave striking new information on crop nutrition but the field results were capable of immediate application to practice. The effects of artificial manures on crop yields were so large that they were not wholly obscured even by widely different soils, climates, and agricultural systems. Lawes and Gilbert took little or no direct part in the repetition of their experiments in other areas. Examination of the increasing mass of the Rothamsted data occupied the greater part of their attention; for very many years Gilbert was absorbed in the nitrogen balance sheet for the various plots and in the amounts of nitrogen that could not be accounted for when the analyses of soil, manure, crop, rain and drainage were put together. Had the study of soil microbiology been as well forward then as agricultural chemistry some of the puzzles would have been clearer. However, the important point was that the conclusions and their technical corollaries might have only a local validity; but the full realization of this was to come only later. The effort, in fact, was somewhat like an attempt to make a synoptic chart for our variable continental-seaboard weather from very detailed meteorological observations taken at a single station.

With the deaths of Lawes and Gilbert the first stage of agricultural research closed. The Rothamsted work had shown that agricultural science had extended beyond the scope of any one man. The need for the help of a microbiologist in a problem that began in chemistry has already been mentioned; and, while biochemistry could advance the frontier of agricultural chemistry, it was, in turn, handicapped by lack of knowledge of the physical environment in the soil. Clearly, the help of research workers in many branches of science was needed if soil and plant relationships were to be further elucidated. This second stage of agricultural research began with modest resources, but the Development Commission, and especially the War of 1914–18, gave it a great impetus. It has now been running for some twenty years in substantially the form planned by its designer, the late Sir Daniel Hall. Weaknesses and merits have had time to show themselves, so a review of the organization is appropriate, the more so in view of the many war and post-war agricultural problems.

In this article it is unnecessary to set out the achievements of the second phase of agricultural research. To any unbiased observer they must amply justify the faith of Hall's vision, for the boundaries of agricultural science have been greatly enlarged. To take only one section, a comparison of

the original 1912 edition of Russell's "Soil Conditions and Plant Growth" with the seventh, of 1937, shows in striking fashion how far we have travelled in twenty-five years and how much British agricultural scientists have contributed to that progress. The annual reports and papers published by other research institutes—those dealing with market gardening and fruit growing, for example—show equally striking advances in their own spheres of work.

It is important to study the organization under which this work was done. There were some obvious difficulties to overcome: specialists tend to work in water-tight compartments, and few of the new men were likely to have any agricultural background. Thus there was the double problem of fostering team-work so that the new agricultural science should be a coherent whole, and of providing a suitable environment to ensure that technical applications of the scientific advances would not be missed or ignored by the research workers.

Superficially, one single large research institute for the whole of agricultural science would have seemed the solution, but for two inescapable facts: first, agricultural research embraces such widely different activities as animal nutrition, farm economics, plant genetics, and soil physics; and second, effective central direction of research, whether by a director or a committee, is impossible if the number of workers exceeds a certain limited figure. The alternative adopted was a number of smaller institutes, each of which took an agreed broad section of agricultural research as its main concern. A possible looseness of contact between these sections was less important than the reasonable certainty that within each section team-work would build up a coherent picture. Another far-seeing provision was to give each institute the widest freedom of control over its own research programme, for the reason that decentralized control works best. In the past twenty-five years many members of the institutes have studied the organization of agricultural research in other countries, and many overseas visitors have examined our scheme; the almost universal opinion is that none is better than ours and most are less effective.

Nevertheless, no one, least of all those in the research institutes, would claim that the present organization should be continued unchanged merely because it has been successful in the past. Should they be consulted about their own future—and it is to be hoped that they yet will be—it would assuredly be found that those concerned with arable agriculture are anxious that the third stage of agricultural research should be organized, for its pattern and requirements are already quite clear.

The very advances of the second stage showed that many technical conclusions obtained for one soil, climate and agricultural condition were either invalid, or needed modification, elsewhere. To meet this difficulty some of the institutes had built up chains of experiments as best they could. The problems investigated were necessarily restricted, as only the simplest field procedure could be used. Their scope was further limited because the problems had to be those of immediate interest to official or private organizations able to meet some of the costs and to help in securing sites. Although the experiments were simple, incomplete, and beset by various difficulties, they were amply justified by their results both in agricultural science and practice. The overriding needs of war-time agriculture have removed,

for the time at least, some of the difficulties: contacts with other institutes and advisory officers are increasing, and an informal network of co-operative field experiments (and demonstrations) is developing. For the first time there emerges a possibility of joint work on some important agricultural problems not included within the interests of official and private organizations.

The freedom of development that characterized the second stage of agricultural research has not only produced the third stage as a logical outcome: it has also provided the technical machinery that makes the next stage possible. The statistical design of experiments to secure comparable results of an ascertainable degree of accuracy enables the maximum of information to be obtained from a given amount of experimental effort, by combining several related questions in one design. Thus, problems concerning two or more research institutes could be dealt with in the same experiment. Such a scheme of field experiments would need co-operative control by the interests concerned through a committee which might work under the aegis of the Agricultural Research Council. The committee would be responsible for selecting the programme from the various proposals put forward and for combining the selected problems in the most economical manner. Each experimental site would be typical of an important soil and farming system.

It is certain that a system of well-distributed replicated experiments will be of the utmost value to the research institutes. It is equally certain that it will enable rapid answers to be given to many urgent post-war agricultural problems of a practical and economic nature, for, as already stated, it is growing up even now under the compelling pressure of immediate war-time problems.

Wars and their aftermaths are conducive to centralized control of many activities. The majority of research workers regard such controls as temporary necessities which have an unfortunate tendency to persist after the need for them has vanished. Lest it should be thought that the scheme outlined above is merely another fashionable war-time centralization, it is desirable to stress the following points: the need for it was apparent before the War; it is a natural development from a second to a third stage of agricultural research. Finally, it does not call for any significant changes in the control and administration of the research institutes. Their record during the twenty-five years of the second stage of agricultural research, and the fundamental work now in hand and projected, are enough to show that they are still efficient instruments in the furtherance of agricultural science.

As stated at the outset, this article deals only with the organization and administration of research in arable agriculture. Contacts with colleagues in various other branches lead me to believe that among agricultural research workers there is general agreement on the situation briefly discussed here and on the best method of dealing with it. But agricultural science ranges so widely that workers in other branches will have their own proposals to offer for the general plan of research organization. One purpose of this article is to promote discussion in these columns and elsewhere, in the belief that a common ground of agreement will emerge; there is no doubt that, especially at the present time, such discussions and expressions of opinion will be very opportune.

OBITUARIES

Dr. Arnold Berliner

The death occurred, on March 23, of Dr. Arnold Berliner, well known to the scientific world as the founder and editor of *Die Naturwissenschaften*.

Berliner belonged to the first generation of 'technical physicists' and worked in close connexion with Emil Rathenau as head of the physics laboratories of the A.E.G. during the period of rapid expansion of this firm. The development of the incandescent carbon lamp and of X-ray bulbs owes much to him—he introduced the first 'getter', phosphorus, in the manufacture of lamps, and the large scars on his arms bore testimony of the early stages of experimenting with X-rays when their dangers were not yet realized. Among other technical problems advanced by Berliner was that of the phonograph: he was the first to introduce the disk form for records (1889).

As an author Berliner was successful with a textbook on physics which appeared in several editions. It is conspicuous for stressing the application of physical knowledge to technical problems, many years in advance of a recognized 'technical physics'. In "Physikalisches Handwörterbuch", edited by Berliner and Scheel, many of the articles defining concepts or describing methods used in physics are from Berliner's pen.

These and other literary products, sound as they are, have not attained the importance of Berliner's creation of *Die Naturwissenschaften* in 1912. Framed on the model of *NATURE*, for the achievements of which Berliner had the greatest admiration, this journal for general science was to draw closer together the physical and the biological, the experimental and the descriptive sciences. Berliner addressed himself mainly to the then young generation of men of science. Much of the success of the journal was due to Berliner's vivid personality, his close contact with the majority of young physicists and mathematicians and his initiative in formulating the subject of articles he wanted written for his journal. Thus *Die Naturwissenschaften* became a mirror reflecting the development of science during 1913–30. To what extent the success of the journal depended on its editor became apparent when his work became limited by a serious eye trouble which made reading nearly impossible to him for many years. His field of vision was filled with a misty haze; after having vainly consulted many oculists, Berliner found the physicists' solution of eliminating much of the light diffusely scattered in his eye by filtering away the short-wave part of the spectrum and reading by red light only.

Berliner's desire was to complete the twenty-fifth volume of *Die Naturwissenschaften*, his very own creation. In the "Aryanization" of the publishing firm of Julius Springer, the old man was turned out of his office in 1935. He had since lived a life of reminiscence of his many friends—artists, men of science and politicians. Various attempts to find a new home outside Germany were unsuccessful. Now his friends deplore in his tragic escape from life the loss of one who represented much of the best cultural traditions of a bygone Germany, and of a warm-hearted and helpful friend. P. F. EWALD.

DURING the years following the War of 1914–18, Arnold Berliner was a prominent figure in German