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RESEARCH ADMINISTRATION

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DR F. W. DRY, a Life Member of our Society, once said that university lecturers and Members of Parliament are usually quite untrained for the work they undertake. To this company we may add many of those who play a part in the administration of funds for agricultural research in New Zealand. However conscientious they may be, they are under the disadvantage that they have had no opportunity to serve an apprenticeship during which to increase their understanding of what is involved in research. They have an additional handicap in that some of the peculiarities of the business of research are probably more easily seen and appreciated by those working at it than by those on the sideline.

One way in which a lack of understanding reveals itself is shown by the opinion of some lay administrators that they should support with funds only those projects which they can readily justify to those who elected them, or to the taxpayers in general. Whether those so consulted in fact, or in spirit, bother much about such matters is not clear. While the opinion suggests a feeling of responsibility, it also indicates something rather like "passing the buck" to those less well-informed than the lay administrators themselves.

Another consequence of a lack of understanding is the detailed information of future requirements sometimes asked of applicants for research funds. Such requests may be legitimate for projects to build bridges but seem strange in proposals to investigate the unknown. The worker cannot fairly be expected to be certain where the work will take him or what difficulties will have to be overcome, and therefore, what his detailed requirements are likely to be.

Because I feel that a greater understanding of some aspects of research would help many of those who administer research funds I am going to discuss a few rather arbitrarily chosen topics. My point of view is that of one engaged in research work, with no conscious desire to become a research administrator.

At a meeting of this Society in 1960, I. D. Dick discussed agricultural research from the point of view of an administrator.

With many of his statements I agree, particularly with his claim that the people in a research organization are much more important than the form of the organization. I think that most of my beliefs in relation to research administration are currently held by many research workers. Most of these beliefs, and many others, are put more fully and effectively by W. I. B. Beveridge in his book, *The Art of Scientific Investigation*, a study of which is even more valuable for administrators than for the administered.

#### DIRECTION OF RESEARCH

The comments here are concerned with administration of research in the sense of deciding what to do at the level of those actively engaged in research.

The term "direction", taken to mean something akin to "dictation", in relation to research is likely to mislead. In general, successful research is more likely to be led than directed. In addition to ability, a leader of research must be able to keep up with advances in knowledge in his and, to some extent, in other related fields. He may lead only himself, or a group. If those who work with him effectively contribute ideas and discussion, the leadership may come from the whole group rather than an individual.

One who cannot keep up with progress in a field is unlikely to be an effective leader. Unfortunately, some directors of research organizations, because of weight of other duties, are in this position. If they insisted on directing in a dictatorial sense they would be a positive danger. However, by suggestion, discussion, and making sure that those more actively engaged in the work are aware of developments, problems and possibilities which they might otherwise have missed, they may exert a valuable guiding influence. Direction from a distance, by postal and telegraphic communication, which is not unknown, is obviously worthless.

One of the problems facing research administrators is that of getting work started in fields thought to be important. If a good leader is interested, there is no problem because he will attract others to him. Directing people to particular work may result in having the man but not his interest, and interest is a tremendous asset. Questions of training, ability and adaptability are involved. Attempts to force people to work together in groups are liable to failure. Apart from difficulties of per-

sonality, some may not respect the standards of thought or work of others, and this is a valid reason for not wishing to work with someone. If groups build themselves up voluntarily, such difficulties are avoided. Acceptability as leader or member of a group does not depend as much on conventional "good mixer" qualities as on scientific merit. Some socially quite difficult people have led good scientific groups.

#### DUPLICATION OF RESEARCH

Duplication which is purely waste of effort and resources is indefensible. However, this probably occurs very seldom, so seldom that attempts to organize research to avoid the possibility of it ever happening are likely to introduce a cure worse than the ill.

Duplication, or the possibility of duplication, in the form of repetition of work, is insurance against shoddy research. Cases of poor experiments and misinterpretation of results do occur, but are not always recognized outside scientific circles. These experiments are extremely costly because at the worst they may mislead and, at the best, have to be repeated. The continued possibility that work may be repeated is the best insurance against them.

Sometimes it is so important to be sure that certain conclusions are correct before further work is undertaken that duplication of previous work is not only excusable, but very necessary. What appears to be duplication to a superficial observer may not be so. Differences in materials, methods or thinking may lead to differences in results or in interpretations which lead workers to correct conclusions. It is common practice for experimentalists, particularly when dealing with complex and not fully understood phenomena, to repeat their own experiments. The experiments are duplicates in as many respects as possible, yet often yields differing results. This seems to have happened sufficiently frequently for the practice of duplication to be regarded as highly desirable in this type of work.

Another consequence of a tight administrative control to avoid duplication would be to give certain workers or groups monopolies in particular fields. This, apart from the risk of providing no safeguard against poor work, is likely to permit a slow working pace. Some workers will keep going well because of their own drive. Others will not, and it may be found that hobbies and lucrative sidelines become their main interests.

## "CURRENT PROBLEM" VERSUS "INFORMATION" RESEARCH

Lay administrators are usually in favour of supporting research into known problems in their industries. This might be called "trouble shooting", or "current problem" research, and it is favoured because the administrators can see the possible benefits clearly, and justify their action happily. The distinction between this type of work and "information" research, aimed more generally at supplying knowledge of the things important in agriculture, is clear in objectives but not in results. "Current problem" research may lead to knowledge of very wide interest. J. D. Bernal, discussing research in physics, has maintained that, in some cases, undertaking applied research of the "current problem" type has helped people to advance knowledge by jolting their thinking from lines traditional in their subjects.

However, "information" research is very important in relation to current problems. The more that is known about plants, soils, animals, and so on, the easier it will be to avoid or solve problems. A very simple example comes from the breeding of beef cattle overseas. In some herds dwarfs occurred. Because of basic genetic work done previously, the way in which this character was inherited was known, and steps to prevent its future occurrence could be taken straight away.

The value of a pool of information may be very great in relation to our pastoral industries. We are familiar with troubles such as grass staggers, and ill-thrift among grazing animals, and to such specific upsets we might add low productivity. Research going on in New Zealand and overseas will yield increased knowledge of pasture plants, animals, and the relationships between them. This should make it possible, by choosing more suitable species and varieties of plants and more appropriate husbandry and management methods, for future problems to be reduced. The successes of this work will mainly go unnoticed, because they will take the form of troubles which do not occur. Such work promises to be more worth while than a similar effort expended in catching up from behind--*i.e.*, in "trouble shooting".

A further argument in favour of sustaining "information" research is that it is often important to have available people with specialist knowledge and skills. These are most likely to be the ones engaged in research, and if there is no research they will not be there to turn to when troubles arise.

## SOURCES OF SCIENTIFIC ADVICE

Most lay or predominantly lay administrative bodies concerned with research have some arrangement for obtaining scientific advice. It is unlikely that those in need of scientific advice will be good selectors of scientific advisers. Some bodies seem to do well and other poorly in this respect.

Sir Charles Snow has recently drawn attention to one of the errors into which a lay administrative council may fall while obtaining scientific advice. In his book *Science and Government* he gives an account of the results of adopting a "Court Scientist" whose opinions were the only ones submitted to a governing body. These opinions were not subject to examination and question by other scientists.

Sir Charles illustrated his lesson with characters of large stature, concerned with matters of vital importance to national survival. While I do not pretend that we are concerned with cases of such dramatic importance, I think we should remember this lesson from real life.

However convenient it might be to have a single source of scientific advice, and as a result have meetings run smoothly and quickly, it may result in harm to the efforts of the industry or organization concerned. It is much sounder to include as advisers several scientists who are competent to understand and discuss the issues involved. Their disagreements can be used to help determine the best course of action. The key to clear advice and quick decisions is usually an adequate supply of reliable data relevant to the problem. There seems no reason why the composition of advisory committees should not alter with the problems to be considered.

## CENTRAL DIRECTION OF RESEARCH

This is concerned with administration of research including allotment of funds at the highest levels and is a favourite topic for discussion and the stating of categorical views.

The main problem in setting up a central, overall directing body is that of seeing that it is wiser than those to be directed. This is not likely to be achieved by making it up from a representative of each interested body, partly because the required ability is unlikely to be distributed in that way, and partly because there is a risk that the interests of the appointing bodies would loom larger than those of the community as a whole. If some such plan of appointment were adopted for democratic reasons, and to facilitate putting forward views, it would be

necessary to have an additional, technically well-informed group of non-partisans to play a major part in making decisions.

As others have pointed out before, an administrative authority with a monopoly in the allotment of funds for agricultural research would leave no alternative source of finance for those turned away penniless. The members of such an authority might make wrong decisions, but, no matter how often they did ~~so~~, it would be difficult to convince them or others that they were wrong, because the results which the work that was not supported might have yielded would not usually become known. For this reason, too, it is not easy to judge the effectiveness of such bodies set up in other countries.

Yet funds and resources in money and people are limited and have to be allocated in some way. The principle of allocating funds to individuals or research groups rather than institutions is sound because people, not institutions, do the work. But, even in a small country like New Zealand, the members of a single high-level council are unlikely to know the research workers, their abilities, and the difficulties of their work intimately enough to do this well. In any case facilities usually reside at institutions and need to be maintained.

The alternative of leaving the allocation of research funds for agriculture as it is done at present seems to be to allow it to occur mainly in the voting of money for Government Departments in Parliament, but it is very doubtful whether any attention would be paid to the merits of lines of research proposed by individual research workers during the consideration of Parliamentary estimates. Some might be eliminated and other less worthy ones promoted rather blindly by this process.

Perhaps a sounder method would involve having sufficient funds allocated through the administrators of appropriate institutions, allow them to maintain facilities and a good volume of research. In addition, there might be a central committee of exceptionally knowledgeable, painstaking, able and unbiased men, provided with funds to enable them to encourage work in selected fields.

#### CONCLUSION

In this address a few rather arbitrarily selected aspects of agricultural research administration have been commented upon in a very incomplete way. No firm conclusions have been attempted.

Some of those in our primary industries have become very impatient with the present system of administration of agricultural research. This is partly because of difficulty they have

had in getting sufficient attention paid to some important problems, and partly, I feel, because of incomplete understanding of the objectives and methods employed in some work which has been done.

The present system is far from perfect, and impatience on occasions has certainly been justified. However, it would be a great pity, if, through lack of understanding of what is involved in getting sound research done, and of what *is* sound research, the measures aimed at improvement left the situation worse than it was before.

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