PRESIDENTIAL ADDRESS

"ANIMAL RESEARCH IN NEW ZEALAND"

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Societies such as ours select their presidents from one of three classes - intrinsically great men, men who have rendered conspicuous service to the Society, or men who represent an important section of the members. It is only natural that there should be a general correlation between the subjects of presidential addresses and the classes from which the presidents have been chosen. The distinguished man often speaks on topical problems, and his opinions are listened to with a respect that is sometimes determined more by the reputation of the speaker than the soundness of his views. The old and faithful servant may be expected to give an historical review of the Society's progress during his term of membership, and the older members at least disguise their boredom because of their genuine affection for the speaker. The section representative feels duty bound to choose a sectional theme, and I can only hope that its reception depends more on the importance of the subject than on the skill with which it is presented.

In electing me as your president I assume that you wished to record your recognition of the importance of animal research, and to show your appreciation of the efforts of animal research workers, and that, therefore, you will not be surprised that I have chosen "Animal Research in New Zealand" as the subject for this year's presidential address.

In order to prevent any possible misunderstanding, it is perhaps desirable to emphasise that the term "Animal Research" is used in a restricted sense and refers only to the investigation of problems arising from the exploitation of animals by man for his, not their, benefit. Although to biologists this may appear a very arbitrary limitation, it is sanctioned by the use in a similarly restricted sense of the words "Animal Production" in the name of our Society.

Any attempt to review past animal research and to catalogue current investigations would result in something between an abstracting journal and an annual report - both valuable but singularly unexciting forms of literature. But some day, someone should write the history of animal research in New Zealand, and if the investigations now in progress prove successful it should be an inspiring story. Let us then consider some of the ways in which we are most likely to provide the future historian with worthwhile material.

The success of animal research seems likely to be conditioned by four major factors - direction, personnel, equipment and organisation.

In our very flexible English language, direction does, of course, mean administration, but it also means aiming or guiding, and it is in these latter senses, perhaps more than the former, that animal research needs direction. It must never be forgotten that the aim of animal research is to increase the economic efficiency of animal production. Not even a drastic revision of our system of monetary values would invalidate this fundamental principle, for it will always be necessary to economise in such real things as time, soil, water, fertiliser, food and labour. If animal production becomes uneconomic it will become obsolete, and animal research workers will have to seek another sphere.

A mere general recognition of the target is not sufficient, there must be conscious and deliberate direction, for aimless research is no more likely to succeed than unaimed rifle fire.

Unfortunately, there still persists in some circles a form of intellectual snobbery which refuses to recognise as true research any investigation which has an economic purpose. This attitude is justified by the allegation that fundamental research results in the discovery of new knowledge, while applied research merely seeks methods for utilising knowledge which is already available. If this were true, animal research would be rather like a cross word puzzle. The research worker would merely have to choose from abstracting journals the answers that best fitted his clues and piece them together. Only very occasionally does this happen, and even then, great ingenuity is required to adapt available knowledge to conditions which may differ widely from those in which it was discovered. More generally it is found that there are very real gaps in existing knowledge, and these have to be filled before the problem can be solved. In this way applied research has made many contributions to fundamental knowledge, contributions moreover which have been of immediate practical importance and which, in some cases at least, would probably not have been made in any other way. One of many concrete examples which could be quoted will serve as an illustration. The fact that cobalt is necessary for the normal metabolism of at least some animals was discovered in searching for the cause of an enzootic disease of ruminants. It is difficult to see how else it could have been discovered, as at that time no chemical method was available which was capable of measuring cobalt in the concentration in which it occurs in pastures or animal tissues. After field experiments had shown that affected sheep could be cured by the addition of one part of cobalt to every 10 million parts of dry matter in deficient pasture, chemists quickly evolved a method of measuring cobalt in parts per 100 million, a degree of accuracy which there was no reason to attempt prior to the animal experiments.

Occasionally one still hears the term "pure research". It would appear to have about as much meaning as the Australian advertisement which described a popular brand of beer as 99% pure.

Any attempt to divert research activities into economic channels inevitably encounters the classic story of Benjamin Franklin. You will all remember how an enquirer who asked what was the use of a new invention, was crushed with the retort, "What is the use of a new born baby?" But economic pressure has today resulted in the practice of birth control in every civilised community. So must every country with limited resources practice mental contraception in its Research Institutions. The rate at which biological problems multiply, in the stimulating environment of the laboratory, is matched only by the most prolific members of the animal kingdom. If unchecked this mental fecundity must result in a plague of embryonic ideas, few of which have any chance of survival in the environments in which they are conceived.

It must not be thought, however, that the problems of animal industry can be solved by a few simple experiments designed on empirical lines. Dr. Hammond in his report to the New Zealand Government said –

> "After the commercial needs of industry for research have been considered and the problems defined, the scientific workers should be allowed to find out the scientific principals involved in their solution. If an attempt is made to find an immediate remedy without knowing the scientific principles involved the results will only be of use in local conditions in one area and will not have a wide general application. This will result in the work having to be repeated time and again under different circumstances instead of standing for all time and under all conditions."

This should not be interpreted as meaning that a remedy should not be sought for a disease until its etiology is fully understood. Early investigators of "bush sickness" rightly deduced that it was a mineral deficiency - "they had found out the first scientific principle involved in its solution". This lead inevitably to the discovery of cobalt deficiency, but even today we know little or nothing of the functions of cobalt in animal metabolism.

It is, of course, very much easier to emphasise that animal research should be directed towards the goal of productive efficiency than to indicate how this can be done. Just as the explorer in an unknown country is constantly diverted from his path by unexpected obstacles so will the research worker seldom find it possible to proceed directly to the solution of any problem. But an explorer who allows himself to be constantly lured from his path by interesting discoveries will neither reach his prescribed destination nor live to report the discoveries made in his meanderings. To be successful, each research worker must have a clear conception of the problem he is seeking to solve, and he must proceed towards its solution with a great tenacity of purpose. He may have to pause frequently to perfect his technique; he may have to make many fruitless excursions down promising side tracks before he finds the correct approach; he may have to wait for others to fill gaps in his knowledge, or to build essential equipment; he may have to admit defeat; but if he is conscientious in his attempts and records them faithfully he will inevitably do something to assist in the final solution of the problem by those who follow.

The choosing of problems is the responsibility of those who direct research. In general, they should choose the problems of greatest economic importance, for whose solution the resources at their disposal appear to be most suited. The general adoption of this principle would do much to eliminate friction and jealousy between Research Institutions. Once a choice has been made research administrators should exhibit the same continuity of endeavour which they have a right to demand from the investigators whom they direct. This may involve the refusal to attack problems which clamour for attention, if the initiation of new projects means the diversion of research workers from unsolved problems which have been allocated to them. While such a policy would reduce the number of problems being investigated simultaneously it might well increase the total effectiveness of animal research in New Zealand.

One of the most perplexing problems for those who direct animal research concerns the termination of projects. Biological interactions are so complex that the final solution of any major animal problem almost inevitably entails a complete understanding of all biological principles, and so animal research will go on as long as man retains a healthy curiosity. The lure of a line of research in which one has achieved some success must be experienced to be appreciated. But in every animal research project there comes a time when economic considerations call a halt. In fixing that end point there are three main considerations: A project should be continued as long as there is any chance of finding a solution to the economic problem. When an economic solution has been discovered, further research is justified if it seems likely to produce a more effective solution or to assist in the solution of kindred problems, provided there is no other Research Institution very much better equipped for such further research. In cases where preliminary investigations show that adequate facilities are not available, and cannot be provided, for the solution of the problem, it is futile to continue indefinitely, but no project should be terminated until some definite conclusion has been reached and recorded.

No one, of course, would have the temerity to seek to limit the field of research workers at Universities, or other institutions, not specifically designed to solve economic problems. It is, therefore, difficult to understand why some such institutions

themselves exclude from their spheres any problem which has the faintest economic taint. I hope the more adventurous leaders in our University Colleges will pardon my presumption in directing their attention to the exceedingly fertile fields for research which lie just outside the forbidden economic zone. Here are to be found many problems to challenge the crusading spirits of the most intrepid knights of the academy. One might mention - the biological reactions of the flora and fauna which are found in the environment of farm animals; the physiology of ruminants with special reference to digestion, amino acid and vitamin synthesis, and the endocrine control of reproduction and lactation; the exact mechanisms by which trace elements control animal and plant metabolism. The answers to these and many other similar questions will sooner or later be required by research workers in the economic sphere. If our academic colleagues can provide them they will save us many headaches. In return we can promise that the centrifugal force of economic research will continue to throw into peri-economic space countless new worlds for them to conquer. We will often meet them in our visits to these outposts of civilisation; we would gladly have their company on our return journeys.

Having determined the objects of animal research, what do we require to make their realisation possible? The first and paramount consideration must always be an adequate supply of competent research workers. A recent press report suggested that in future British civil servants would be selected by a Committee which should contain psychologists but not psychiatrists. Perhaps some day a similar Committee will be appointed to select research workers in New Zealand. For their guidance, I should like to propose the following specifications to which all candidates for research positions should be expected to conform. A research worker must have the insatiable curiosity of all young things. The persistance of this beyond adolescence is probably a special form or arrested development, but it is so essential, that when it fades the research worker should be promoted to an administrative position. He must have a critical intelligence which will make him very suspicious of anything which has not been experimentally verified. "The scientist has learnt to believe in justification, not by faith, but by verification." He must have a tenacity of purpose that will allow no obstacles to deter him in his pursuit of knowledge. He must have an uncompromising honesty, which will impel him to acknowledge his own errors as soon as they become apparent. Finally, it is desirable that he should have a philosophical outlook that will enable him to meet many disappointments without becoming as difficult to work with as a wolf with hydrophobia. It will at once be realised that the simultaneous possession of all of the above attributes would render a person so unusual as to be almost abnormal. They would certainly disqualify him completely as a lawyer, a clergyman, a journalist, a diplomat or a politician - all very honourable professions. No one need, therefore, feel the least shame if he does not meet these specifications, but he should realise that research is not his vocation.

It is almost certain that some one has said that research workers are born not made. They are certainly not born ready-made. The training of research workers is a controversial subject and there is probably no stereotyped plan which will fit all cases. The university degree is only the end of the first phase and in all cases some form of post graduate training is required. In order to plan this intelligently it would appear necessary to decide early in what type of research work the graduate is to be employed. Obviously he cannot make this decision himself, and it would not be fair to ask him to undertake specialised training without any guarantee of employment in which it will be of value. I am of the opinion that Research Institutions should interest themselves far more in the training of their future staff. I should like to see a system instituted whereby research recruits were chosen during their degree course and appointed as soon as they have received their Bachelor's de-

degree. The Research Institution would then assume the financial responsibility for further training, and assist in specifying the nature of such training. In some cases this would consist of further study in carefully chosen subjects leading to an Honours degree at a New Zealand University College, at a New Zealand Research Institution or overseas. In quite a number of cases I believe it would be wise for the new recruit, immediately after graduation, to serve an apprenticeship in a Research Institution to enable him to acquaint himself with the problem on which he is to be engaged. If necessary, he could then undertake further study with a better comprehension of what is needed to complete his mental equipment. Such a system would obviously require very close collaboration between the University Colleges and the Research Institutions, both in the choice of research recruits and in planning their training; but this should not present difficulties any greater than those being overcome at San Francisco.

We have determined the objects of animal research, we have chosen and trained our research workers, and now we must find them a home. No Research Institution is complete without a well designed, well equipped laboratory - or is it? Hans Zinsser in "As I Remember Him" refers to the era in which American millionaires discovered science as an outlet for their surplus wealth and says, "Opportunity was made available faster than the brains needed to take advantage of it could be mobilised. There was at first much mere re-discovery of what was already well known in Europe and in many places half trained people in magnificent laboratories were sitting on sterile ideas like hens on boiled eggs." We have advanced a long way since then. Both eggs and ideas (including sterile ones) are now hatched in very expensive incubators.

Yes, laboratories and equipment are necessary, but I would urge that their planning and selection be guided a little more by the purpose for which they are to be used, and a little less by the latest architectural fashions, and the most recent editions of manufacturers catalogues.

An accomplished angler experiences great aesthetic satisfaction in capturing a wily trout with a well cast fly, but a boy with a worm will catch more fish than an unskilled fly fisherman. No investigator will accomplish much unless he finds some pleasure in his work, but research is something more than recreation, which is engaged in mainly for pleasure. It is perhaps worth mentioning that much modern apparatus is not foolproof and research workers should be extremely criticial of results obtained with apparatus with which they are not thoroughly familiar.

Gadgeteers are not an unmixed blessing. The man who can, and does, design a piece of apparatus when it is needed, is almost invaluable. The man whose main object in life is the production of extremely complicated apparatus is a pest in a laboratory, though he might be worth a fortune to a manufacturer.

But animal research in New Zealand is not all conducted in laboratories. A well designed set of sheep yards; a carefully sub-divided area of pasture; an efficient apparatus for collecting excreta from a grazing animal; apparatus for recording rate of milk extraction; an instrument for collecting liver samples from a living sheep; an apparatus for separating faeces from dry grass - such things as these are just as necessary as the most delicate apparatus ever listed in a catalogue. In the production of paddock, yard and shed apparatus, New Zealand workers can afford to give their imagination full play. Some of them have already made very valuable contributions.

Obviously the great expense involved in building and equipping laboratories necessitates a degree of centralisation, but there are some real dangers involved in such a policy. In research stations there is no correlation between size and ef-

ficiency unless it be a negative one. The director of a research station should always be a man with a very real interest in research and it is his duty, to direct the investigations towards the chosen objectives, to assist in their planning and to supervise their conduct. As soon as the research station gets so big that he can no longer take a personal interest in individual projects, efficiency will tend to decrease.

There is another subtle danger which must be guarded against in large animal research stations. In order to study the problems of animal breeding and animal feeding it is necessary to breed flocks and herds and to grow feed for them. It is only natural to endeavour to breed good animals and to grow good pastures and crops, and these can easily become main objectives requiring the attention of a large proportion of the staff and actively interfering with the planning of research projects. It may be quite justifiable to breed high grade stock and to feed them on first class pastures, but, if necessary, there must be an equal readiness, deliberately, to breed poor stock and to feed them on third grade pastures. An animal research station must not be allowed to degenerate into a model stud farm.

When decentralisation becomes necessary its planning should be governed by the geographical occurrence of specific problems rather than by the vested interest of districts, departments or institutions.

Now we have reached the stage where our trained research workers know their objectives and have been provided with adequate research facilities. How shall we plan their research? Most projects start with a preliminary survey in which facts are collected with a view to defining the problem. For this purpose organisations such as the Herd Recording Department of the Dairy Board, which collect data covering a large animal population and have staff who are expert in surveying such data, can render invaluable service. Where such organisations are not available, the greatest possible care must be observed in sifting data, and it must always be remembered that the evidence of untrained observers is frequently an expression of their opinions, rather than a faithful record of their observations. It is, therefore, important that at the earliest possible stage the problem should be isolated from distracting influences. This may mean setting aside an area in a research station, or even instituting a special research station, and detailing staff to observe, measure and record. This process is necessarily very tedious. It can be prevented from becoming boring by the frequent analysis of recorded data with a view to finding a point of attack.

Sooner or later the problem must be broken down into a series of definite questions, each capable of a direct answer. In the past there has been a tendency to seek to accomplish this by allotting to the problem a team of workers, each of whom is an expert in his own subject, and sending them into the attack simultaneously. This is probably a very wasteful method. Let me refer again to Hans Zinsser. He quotes the following statement from Bacon - "Neither is it possible to discuss the more remote and deeper parts of any science, if you stand but upon the level of the same science, and ascend not to a higher science." The author then goes on to say - "The ascending to a higher science is the task with which medicine has been occupied in the 20th century. But this 'ascending' which Dampier calls 'the resolution of physiology into biophysics and bio-chemistry' cannot be achieved without an immense preliminary labour of pure observation, by which problems amenable to the methods of exact science are isolated and simplified." Let me give you an example from animal research in New Zealand. When the concerted attack on facial eczema was planned in 1938, it was agreed by all concerned that chemists should seek to discover the toxic material in pasture, which causes the liver derangement characteristic of the disease. Accordingly, pasture was collected from paddocks from which fresh cases had been reported, and this was subjected to a

very exhaustive chemical investigation. Fortunately the work has not been wasted, as it has resulted in the discovery of a number of interesting substances whose existence was previously unsuspected. But investigations have now shown that, by the time clinical symptoms of facial eczema appear, the toxicity of the pasture has almost invariably fallen to a very low level. The chemists were, therefore, set an impossible task. It has taken a very considerable amount of tedious work to perfect a technique whereby toxic pasture can be collected and preserved in such a way that it retains its toxicity. Today, it is possible to say to the chemist, here is a bag of dried pasture that we know contains the toxin, can you isolate it? Obviously the chemical search for the toxin should not have been started until that stage was reached.

Does this mean that the chemist (or other specialist) should stand aside until the problem has been reduced to a form in which it is "amenable to the methods of his science"? That will depend very largely on the specialist. If he can ally a broad knowledge of his science with a general understanding of animal husbandry, he should be able to give valuable advice on the best methods of "isolating and simplifying the problem." But in any case he has a duty to himself and to his colleagues, to see that he knows the full history of the material that goes into his test tube, and to ensure that it arrives there in a satisfactory condition. This entails a close collaboration between the laboratory workers and those who collect the materials that go into the laboratory, or test the products that come out of it. Administrators have a duty to see that this is made possible, but only the enthusiasm and sympathetic understanding of the individuals concerned can ensure its realisation.

So far much of what has been said has a general application to research, or at least to animal research, almost anywhere. Are there any conditions peculiar to New Zealand? Some two years ago after attending a stock sale in the Wairarapa district a farmer friend took me to the place where farmers foregather to discuss their bargains and introduced me to one of the district pioneers, a man of the old school. The old timer looked me up and down, grunted, and said, "I suppose you read a lot of bloody books, but there'd be a dozen things down in the paddock you wouldn't know anything about." There are probably a number of adequate retorts, but I must confess that they all eluded me at that time. I think I replied, "You may be right about that." With all due deference to this distinguished audience, I would like to suggest that he would still have been right had he addressed his remarks to any of you. New Zealand farm animals spend their lives in the paddock, and they subsist almost entirely on growing pasture, supplemented with hay and silage made from that pasture. There are very many gaps in our knowledge concerning the habits of the grazing animal, the environment in which it lives, and the interactions that occur between the animal and its environment. The filling of these gaps will not be easy. A paddock will not go into a test tube nor can a cow be examined under a microscope. The dry powder which the chemist analyses is not the same thing as the growing pasture from which it has been made, nor does a stained section of hepatic tissue behave in the same way as the liver of a jaundiced sheep. The organ of a cow killed at the abattoirs is probably not in the same condition as it was when the cow left the paddock several days before. We know that climatic conditions affect the incidence of diseases such as milk fever and grass staggers in grazing cows, but it may not be safe to assume that they do this by causing a change in the chemical composition of pasture at any particular time. The health and lactation of a cow on any given day are influenced by many things besides the food consumed during the previous twenty-four hours.

All this means that we must observe and record the reactions of our grazing animals. This will often present great

difficulties, but these can be overcome when research workers realise that it is their job to overcome them. Listen to this extract from an account of an experiment designed to observe the incidence of oestrus in 46 merino ewes in North Western Australia. "The ewes were run in two adjoining paddocks, one of 800 acres and the other of 700 acres . . . The ewes were mustered each morning. This was difficult and arduous work, particularly after the rains started. Not only were the paddocks large and the temperatures high, but after a few inches of rain had fallen the ground became too soft to use a horse for mustering. When the spinifex heads were about four feet high it was hard to find the sheep. After mustering, those ewes which had been marked were transferred to the other paddock." Daily observations were continued from the 7th October, 1941, to the 7th May, 1942, and were only stopped then because as the authors say, "Conditions on the Station arising from Japan's entry into the war became so difficult."

Mere eye observations are not sufficient. Yard sticks must be devised which will enable us to measure with reasonable accuracy, and record, in terms of standard units, the fluctuations in the grazing animals' environment, and their reactions to that environment. Every effort must be made to convert these units into exact chemical and physical terms, but we must not make the mistake of substituting for these units, chemical and physical measurements which have no relationship to them. Let me illustrate. To say that pasture growing in a paddock on the 15th April, when cut and dried, and fed at the rate of 1,000 grams per day for 10 days produces certain specific liver lesions in each of 3 lambs weighing 60 lbs., does give some measure of the capacity of that pasture to produce facial eczema. When we can say that the pasture contained so many parts per million of a definite chemical compound, known to produce facial eczema, our measurement will be much more accurate. But a complete chemical analysis of the pasture for all present known constituents, none of which causes facial eczema, gives no measurement at all of its facial eczema producing properties.

But even the recording of paddock observations in well defined units of measurement is not sufficient. There must be deliberate experimentation in the paddock, and paddock experiments must be planned with the same intelligence, and conducted with the same skill, as laboratory experiments. The latter are designed to measure the effect of specific factors under carefully controlled conditions, in which they can be isolated from disturbing influences which occur in the paddock. Such laboratory experiments are, of course, quite indispensible, but on the other hand those who claim to be able to dispense with paddock experiments, make the colossal assumption that they understand, and are able to isolate, all the factors which are compounded to form that complex biological system, the grazing animal in the paddock.

In designing paddock experiments it is seldom possible to hold environmental factors, such as temperature, light, humidity, nutrition and exercise, constant throughout the whole period of the experiment, and it is, therefore, necessary to study the effect of adding, subtracting, or altering specific factors within a changing environment. This, however, may be an advantage, as certain reactions occur only in one phase of the very variable paddock environment, and such environmental interactions may give the necessary lead, and can be further studied in experiments designed for the purpose. It is, therefore, important to measure environmental variations, as completely as possible, during the course of paddock experiments. Laboratory and paddock experiments are complementary phases of animal research; they must be carefully integrated, and if not conducted by the same workers, those in one sphere should be familiar with the other, and there should be frequent interchange of reports and suggestions.

I do not think that it is possible to over-emphasise the need for paddock research in New Zealand, and I want our young New Zealand scientists to recognise the challenge of this almost unexplored field. There are research adventures to be undertaken here, as exciting as any to be found in the most famous laboratory. I firmly believe that some of our most important animal problems can be solved by paddock research, and cannot be solved without it. But they will not be solved by leaving paddock observation and collection of paddock specimens to shepherds and cow hands. We must, ourselves, go into the paddock, and we must go there not just as casual observers, but as trained research workers. Above all, we must learn to think in terms of paddocks and grazing animals. We cannot, of course, do without the laboratory, but we must get away from the idea that all the intellectual aristocrats reside in the laboratory, and that only mental plebeians are found in the paddock. We need a real democracy of animal research in which there are well worn paths connecting paddocks and laboratories, and a great many workers who are equally at home in both spheres of research.

This address has now traversed a course almost as tortuous as that of a major research project, but any of you who are actively engaged in animal research will know that I have left much ground unexplored. It will not, therefore, be necessary to emphasise that it has not been my purpose to present anything in the nature of a charter for animal research in New Zealand. Earlier, I suggested that a research project should never be terminated until some definite conclusion had been reached. Many of you may consider that in this address very few incontroventible conclusions have been reached. Nevertheless, it must be terminated. It has become almost a convention for the president of this Society to epitomise his address in the form of a question. In conforming with the precedent set by my illustrious predecessors, let me lead you back to the thought with which I started, so that after I have finished, you may draw your own conclusions. My question is - Should a research worker know what he is looking for?