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PRESIDENTIAL ADDRESS

Animal research — time for a new approach

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I wish to comment on some aspects of animal research in New Zealand. My reason for doing this is that I contend that over the last 5 or so years the animal production industries in this country have been pushed into a new era. The reasons are both economic and technological.

The economic aspects are rather obvious: the changing market situation (e.g., the state of the world butter market and our dependence on the Middle East lamb market), the world economic situation and New Zealand's internal economic problems as evidenced by the supplementary minimum price scheme and the worsening cost-price squeeze for the farmer.

However, the technological aspects, such as the developments in genetic technology have received little attention. Therefore I consider it opportune to look at our approach to research in the animal production industries.

Developments in Animal Production

Developments in the animal production industries over the past few decades have been well supported by a strong and relevant research base. However, the traditional approach to animal research has been 'safe'. The objectives for animal production and hence research were clear:

- to increase total animal production and hence exports
- to improve the efficiency of animal production and hence the well-being of the farmer

through improvements in pasture production, land development and use and then by increasing the efficiency of utilisation of the pasture by the grazing ruminant animals through improvements in management, disease control and breeding.

The results of this approach have been spectacular. Examples include:

improved pasture species; aerial topdressing; genetic improvement through the intensive use of highly selected bulls by artificial insemination in the dairy industry, group breeding schemes, objective selection programmes and the development of new breeds in the sheep industry; definition of the role that fencing, stock densities and rotational grazing can play in the efficient utilisation of pasture; and the development of irrigation.

As well as these examples of technologies which have had a major impact on animal production in New Zealand, there have been considerable increases in output per labour unit.

However, this is all in the past; I consider the belief that the approach to research which generated these developments will continue to serve us well in the next few decades is erroneous. The old approach was safe — new improved production systems were developed with the approach resting on the philosophy that if we could produce more of the same, this would increase our export income and therefore increase our well-being.

More recently in the 1960s and 1970s the objectives were also clear: increase animal production and diversify. The farmers responded — we now have more sheep, more cattle, higher production of lamb, mutton and beef, more wool and dairy products, a new deer industry, a goat industry and large horticultural and other cropping enterprises which have developed on land previously used for animal production. However, even so the produce from sheep and cattle still earn about 70% of our total export income.

Objectives

What should be the objectives for the animal production industries as we head towards the 21st century? Do we really just want more of the same — more lambs, more wool, more butter and cheese, more beef or do we want something else? I cannot answer this question although I cannot help feeling a little nervous about the pressures on our traditional products in many of our markets, although some studies have pointed to a strong demand for New Zealand's food products in the world economy over the next 20 years.[19] The following group of objectives is not particularly novel but it is useful to list them in the context of this address.

Important objectives are:

1. Becoming more aware of the requirements of our markets, e.g., in terms of the demand for lean meat; however, just being aware is not sufficient — we must be able to translate awareness into effective action and devise imaginative strategies to meet these market requirements.
2. Increasing the efficiency of our animals —

especially in terms of reproduction and growth in sheep. With an average carcass weight of lambs at 6 to 7 months of age of about 13.0 to 13.5 kg and a national lambing percentage of less than 100% there is substantial room for improvement. However increasing reproductive efficiency does not necessarily mean having ewes producing litters of 3 or more lambs.

3. Becoming more efficient at utilising the basic resources — land, water, energy, fertiliser and of course the pasture which is grown.
4. Minimising wastage of animals from diseases and increasing productive lifespan.
5. Decreasing the cost of production.
6. Being imaginative and keeping an open mind in terms of the potential for new developments.

It is this final point of imagination that I want to develop further in this address, but firstly it is necessary to consider some aspects of research in this country.

The Scale of Research

A vitally important aspect of the whole research scene in New Zealand is its scale. New Zealand is a small country — we have only 0.08% of the world's population although on a *per capita* basis our gross domestic product (GDP) is moderately high. However, as a proportion of GDP our investment in research in very low — only 0.8% of GDP compared with the average of the OECD countries of 2.3%.[21] A low input into research need not necessarily mean a low output but a small involvement means:

1. That the research should be of very high quality.
2. That an important component must be the application of this research in industry; therefore, the methodology for its application itself can become an important research objective. If little research is being carried out it is especially important that much of it is usefully applied.
3. There is a temptation with very limited resources to choose the safe and predictable research; that is the desire to avoid the risk of wasting money means that research which is regarded as a high risk proposition by funding organisations is neglected even though should the research be successful the potential returns are very high indeed.
4. That we are very dependent on overseas developments. Important changes in agricultural research, especially in regard to genetic technology mean that the products of research will be more readily marketable and saleable than has been the case previously — this has already happened to some extent in the plant field[14] but its impact will inevitably increase with the application of genetic technology in the world of animal science. This is likely to have a considerable impact on the New Zealand farmer: his big gains in the future will be likely to come less from improvements in management but more from the use of purchasable commodities, be it embryos, or bottles of antigen, antisera or the like (e.g., immunisation against endogenous hormones to increase fecundity[30] and growth[31]).

Type of Research

Therefore, I consider that the time has now come for the injection of considerably more imagination into animal research in New Zealand and then in the application of this research in the animal industries. However, before expanding on this it will be helpful to look at the set-up of animal research in New Zealand. While recognising that there is considerable overlap it is useful to describe research as existing at 4 levels:

1. Industry development research: examples are deer farming and protein extraction from pasture. In both cases research had exercised an essential and leading role in the development of these industries.
2. Predictable pay-off research: much of the research carried out within MAF would fall in this category and an example is the study of the genetics of carcass composition in sheep which will result in development of selection procedures to produce lambs with leaner carcasses. This type of research tends to be important at a national level. However, on occasions such research is rather unsuccessful — 2 examples where I would contend little progress has been made are the influence of nutrition on fecundity, and of nutrition on carcass composition, the reason being that we know too little of the basis of the processes of the control of ovulation on one hand and growth on the other.
3. Local research: this type of research is of regional rather than national importance and frequently has profound consequences for farming practice in the local area. An example would be the grass-wintering system in Otago/Southland, which incidentally is a concept not understood by some North Islanders, thus emphasising its local importance. Often this type of work would be more fairly classified as demonstration rather than research.
4. Fundamental research: this is high risk research for funding organisations, i.e., the potential returns are very high but the investment is long term and dramatic break-throughs do not happen overnight. This type of research does not thrive in isolation — its most important components are imagination and a diversity of approach, albeit with a common goal. Ironically the peer review system which in many cases determines the allocation of funds to research of this type has been accused of stifling imaginative approaches

and supporting traditional 'more of the same' type research.[1, 12] New Zealand supports a disproportionally low amount of fundamental research. Much of what could be relevant to progress in the animal sciences is carried out in the various science and medical faculties of our universities. In the current economic climate and as a result of Government policies, their research budgets are diminishing — without doubt to our cost in the long term.

Our research effort in the animal sciences in New Zealand has been very heavily concentrated on predictable, safe research and local, frequently demonstration-type, research. This may have served us well in the past but in order to make further progress in animal production it is absolutely critical that we invest far more resources in fundamental research. This will enable us to further understand and therefore to manipulate some of the physiological processes which in the long term determine the production from our farm animals. I contend that we can no longer afford the neglect of fundamental research and therefore will outline some of those areas where such a research approach could be expected to be rewarding.

Essential Characteristics

Before discussing some specific areas of research it is appropriate to mention 3 characteristics which I consider should be evident in this research effort:

1. A considerable breadth of co-operation, co-ordination and communication between researchers is vital because even if we were to increase immediately our expenditure on research and development to the OECD average, we do not have the total resources to be able to afford a fragmented approach. I am not advocating a centralised control of research, or a uniformity of approach but I am advocating the identification of important fields of work and a national approach to research in these particular fields. Consequently it means much more co-operation is required between the various research organisations (e.g., MAF, DSIR, MIRINZ, DRI, WRONZ, the Universities) and there are encouraging signs in this respect.
2. We must acknowledge that we cannot afford the current imbalance in research with its emphasis on the safe and predictable. The 'more of the same' philosophy is likely to produce a great deal of 'i dotting and t crossing' with little or no real progress.
3. We need considerably more imagination and imaginative approaches to our research.

Targeted Fundamental Research

I contend that we must place considerably more resources and money into fundamental research — that is targeted fundamental research with high potential returns. Here are some examples in the animal research field where I consider the potential returns are high recognising at the same time that the returns are likely to be in the long rather than in the short term. They are:

1. Genetic technology.
2. Immunological technology.
3. Computer modelling of the digestive system and of animal/plant/soil complexes.
4. Reproductive physiology.
5. Physiology of growth.
6. Physiology of lactation.

At first sight these may appear to be very broad topics, but within each of these fields there are approaches which will have a high chance of being extremely profitable. In order to realise this potential it is the type of approach to the work which is the most vital ingredient. Therefore in order to develop successful lines of fundamental research within each of these fields there is a very strong case for the establishment of 'centres of excellence'. Already in New Zealand there are some imaginative and innovative researchers involved in these fields. Usually there are 2 features which limit the returns from their work:

1. They are working in isolation and the approaches are fragmented because of the lack of resources.
2. The scientist is expected to combine the roles of researcher, technician, administrator and farm advisor.

A fragmented approach to many of the fields listed above is worse than useless — it promises much and produces little. In my opinion the only way to approach work in these fields is to set up centres of excellence with groups of researchers developing team approaches to the questions. The fundamental concepts of the centre of excellence are:

1. That the approach be multidisciplinary.
2. An active group of researchers attacking one problem; clearly in this respect personalities are of vital importance as essential attributes of team members must be mutual co-operation, trust and respect.
3. There must be a co-ordinated but broad approach to the problem but at the same time a fragmented approach must be avoided.
4. The essence of originality and innovation in research is the diversity in patterns of thinking and in the expertise of the different members of the research team.

The overall key is an integrated diversity of approach to the problem in question.

The 6 topics I mentioned are all very appropriate fields for targeted fundamental research using a multidisciplinary team approach. However time would not permit an adequate discussion on all — as there are other appropriate areas for consideration

— for this reason I will expand on only 1 area in any detail, namely genetic technology.

Genetic Technology

New Zealand workers have been at the forefront of the practical development of 2 examples of genetic technology, namely artificial insemination[8, 26, 27] and embryo transfer[32] in cattle. While AI will certainly continue to be of major importance in genetic improvement programmes with farm animals, some of the more recent developments are likely to have considerable impact on animal breeding. I refer here to procedures such as the sexing and cloning of embryos, nuclear transfer and gene transfer.[2, 18, 24, 28]

Identification of the sex of embryos for embryo transfer could have considerable application especially in dairy cattle improvement programmes. For example, male progeny are required from only the top cows whereas an increased number of female calves would enable more intensive selection and also facilitate the export of embryos. In this respect a recent report indicates that a patent application for an embryo sexing technique has been filed in the United States.[28] It is also likely that in the near future it will be possible to treat semen to selectively eliminate the Y male sperm; this could have considerable impact in artificial insemination programmes.

Identical twins (or triplets) may be produced by the splitting of a fertilised egg.[36, 37] Identical twins are clones — a perusal of the Proceedings of this Society over the past 40 years will illustrate some of the impact that the use of identical twins has had on dairy research in New Zealand.[5, 23] The potential impact of the use of large numbers of identical animals in research is phenomenal as for example in studies of the influence of different nutritional environments on growth, protein and fat deposition, ovulation rate, etc. Some would consider that the potential impact in the commercial world is even greater than that in research in that identical embryos could be stored while another member of the clone is evaluated in an artificial breeding programme. However in this respect the implications with regard to the breadth of genetic diversity in the population would need serious consideration.

Nuclear transfer is really an extension of cloning — instead of splitting the embryos, cells are collected from the trophoblast.[13] The nuclei are recovered and each transferred to an unfertilised egg from which the original nucleus has been removed. Those cells that develop are therefore identical.

Gene transfer — specifically the introduction of genes into animal cells through the use of selected purified DNA — also has considerable potential.[11] For example, growth hormone stimulates milk production in cows[4, 22] and consequently if the gene or DNA specifying growth hormone could be introduced into the fertilised bovine egg, then conceivably the resulting cow could have more growth hormone and a higher milk production. In this respect a recent article in Nature[20] reported the successful transfer of the gene for growth hormone from rats to mice. Some of the mice that developed with the additional copies of the growth hormone gene were almost twice the size of their normal litter-mates. This technique also opens up the possibility of 'genetic farming' — that is the use of animals to produce large amounts of certain proteins such as hormones and antisera for commercial purposes. To take a specifically local example, if those salivary proteins protecting a cow against bloat could be identified and either the gene(s) isolated or the DNA synthesised and then transferred into the developing embryo that animal would then develop with the gene(s). There is evidence from work with mice that this injected 'foreign' gene could then be passed on to the next generation.[10, 20, 34] The potential value of a high quality bull carrying the gene for an antibloating protein is clear.

Genetic markers — the identification of genetic markers for desirable production traits would greatly assist in the selection of superior animals without the necessity for tedious progeny testing or even long term performance testing. Accurate identification of superior animals early in life would clearly have a major impact. This approach is being used to identify cows with a low susceptibility to bloat on the basis of the production of certain salivary proteins when the cows are exposed to 'bloating' pastures.[7] Another example would be a genetic marker for the Booroola gene; here a serum protein or a blood cell antigen would seem to offer the best possibility. While the Booroola's high fecundity is apparently due to one gene and lends itself to such an approach, there are also strong possibilities for the location of genetic markers for traits inherited in a multigenic fashion. In this respect the markers may not be absolute indicators and the mathematical approach to the analysis of the data would form a vital component of a multidisciplinary research effort. The last few years have seen great advances in these fields in the human.[3, 29] Various physiological or biochemical criteria can also be considered in the search for genetic markers.[16, 17]

I have presented here a glimpse of the possibilities in this one area of genetic technology. A team approach to research on aspects of these questions could be expected to prove very rewarding.

Immunological Technology

This has received some attention recently in the agricultural press as a result of the work on immunisation to improve fecundity in sheep,[25, 30] immunisation against somatostatin to increase growth[31] and immunisation against luteinising hormone releasing hormone (LHRH) as an alternative to castration.[15] This technology has enormous potential but much fundamental

work is still to be done before much of this work can find general application in farm animals. We simply cannot expect to always reap the benefits of overseas research with little input of our own. Also the effect of patents and the close links which are developing between the research scientist and the commercial organisations developing these technologies, mean that their cost and availability to the New Zealand farmer may develop as a very important issue over the next few years.

Computer Modelling

The interaction of the animal with the pasture and the process of ruminant digestion are complex. Traditional experimental methods have not yielded the type of information required to enable us to really understand the pasture/animal system or to understand the processes of ruminant digestion. Therefore, we are unable to predict the animal production from a system or to define the important factors to select for in the development of new pasture plant cultivars. In this respect with small inputs, computer modelling approaches have already yielded valuable information and have identified important areas where further experimentation is required.[9, 33]

Physiology

I now want to comment briefly on the other 3 areas I mentioned: the physiology of growth, reproduction and lactation. The necessity for fundamental work in these fields is obvious when one considers the lack of progress over the last 10 to 30 years. The real objective of our research is that it leads to improvements in practice on the farm. As an example I ask 2 questions:

1. Are we any closer to understanding the influence of nutrition on fecundity in sheep than we were after the seminal work of Wallace[35] and Coop[6] 20 to 30 years ago?
2. Can we say under what conditions the farmer can expect compensatory growth in his animals or can we say what the effects of nutrition on carcass fatness are even likely to be?

The answers to these questions must be in the negative. In relation to the effects of nutrition on fecundity we do not even know what good nutrition is. The difference between our research in 1983 and that in 1953 is that we have defined feeding levels more accurately and now do laparoscopies and record ovulation rates instead of counting lambs at birth. Our advice to the farmer is still much the same — 'feed your ewes well and you should get more lambs, but if you do not we cannot tell you why you don't'. With respect to the influence of nutrition on carcass fat and lean deposition we are not much better off — we now know that about 60 to 70% of the variation in carcass fatness between animals is due to environmental factors of which presumably nutrition is an important one. However, if we cannot predict the effects of nutrition then this is of little value.

In respect of these questions, the need is for us to develop lines of research in an attempt to provide some fundamental understanding of the factors controlling ovulation rate in sheep and of the factors regulating the allocation of nutrients to the various body processes whether they be protein synthesis, fat synthesis, milk synthesis, wool production, etc., so that at least we might develop some ideas as to how various factors such as the level of nutrition, whatever that is, might effect these processes.

Summary

The approach to animal research which has concentrated on predictable pay-off research and local research has served us well in the past. However, I contend that as a nation we can no longer afford the neglect of fundamental research. There are a number of areas where a major input into fundamental research could be expected to produce large dividends. The field of genetic technology is one such area. In order to develop lines of work in such areas a strong case can be made for the establishment of 'centres of excellence' where a number of researchers attack different aspects of the same problem as part of a multidisciplinary team.

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