Agronomy research: the need for change

Closing Address to Final Session

O.T. Kingma

Bureau of Agricultural Economics

My task is to evaluate this conference : to assess how well current research in Agronomy, as revealed at this Conference, fulfils the needs identified in the opening session. There is a danger in assessing agronomy research in this way since the assumption is made that the distribution of conference papers is representative of the overall research effort. However, this forum is one of the few outlets for agronomy research and the call for conference papers was advertised widely. Therefore, while the above assumption must be borne in mind, I do not regard it as invalidating the following comments.

In my judgement the Conference has been a success. Some 200 papers have been presented and over 300 delegates have contributed to identification of issues and research problems in the large field of Agronomy. Topics have ranged from frontier research in the most specialised of scientific areas to broad developments in international markets for our plant products. To evaluate all this over a three-day period has not been an easy task, and in this context I would like to acknowledge gratefully the help of my Bureau colleagues Dr Tony Ockwell and Dr Mike Blyth. Without interaction with them, I doubt whether I could have achieved the required perspective.

In making the following statements, I would not like to be seen as dogmatically making unqualified statements about the agronomy research program. The subject reviews indicate that excellent work has been and still is being undertaken. Therefore, if I generalise too much at times please bear with me. Generalisation is the essence of the review process.

Sub-division of the Conference into nine specialist sessions, coupled with statements from experts on the issues confronting Australian agriculture, has been a good strategy. It has provided room for the necessary technical interchange as well as the machinery for periodically bringing the Conference back to the broader perspective. The use of posters on individual research papers as a back-up to communications has also been successful.

The distribution of papers within and between the nine specialist sessions has, however, not been balanced. I regard this as indicative of imbalance in the present agronomy research program, which, I think, is still too introspective and narrow to cope with the emerging problems of agriculture in the eighties. For example, over 21% of contributed papers have been in the area of plant nutrition, a traditional area of specialisation and one relatively straightforward from an experimentation/write-up viewpoint. Over 50% of papers within this category are still rather narrowly concerned with the effects of phosphate on crops and pastures. While the longer-term nature of some of these experiments, as well as the changing nature of the phosphate problem (acid soils), is acknowledged, one wonders whether the requirements of the profession for volume of publications has led to this continuing emphasis. It is a disappointment to me that there is little comprehensiveness or unity in this fairly large research effort, and disappointing to hear of badly-designed experiments in this area, involving confounding of results due to inadequate standardisation of soils on which the experiments are conducted.

Despite initial feedback to the Conference program committee through the preconference questionnaire, that research on cultivation and new techniques for sowing and growing crops was important, the Conference attracted less than 9% of papers in this area. Considerable interest has, however, been shown in this area at the Conference. I therefore feel confident that it will attract the required research resources in future.

If, as I think it should be, agronomy is defined in the broad as all areas of research leading to productivity improvement in crop production, then in addition to research into the plant itself and its protection and

management, agronomy research should also encompass research into farming systems - the way in which plant production fits into the farm unit and into the agricultural sector as a whole.

In this context, while it is encouraging to see some 10% of papers in this area of agricultural systems, I would argue that considerable scope exists for research beyond the crop/crop and crop/livestock interaction concept of systems. I fear we are all thinking of 'systems' in different ways. Most of you pay lip-service to the term, one of the exceptions being Dr Davidson, who has made very conscious attempts to slip the traditional mould. By systems I mean Dr Wood's holistic concept, encompassing all components of crop production, namely the environmental, technical, social and economic factors influencing on- and off-farm operations in the entire production chain from cultivation to retail.

This does not mean that agronomists should all suddenly become multidisciplinary researchers. But the messages you should be receiving loud and clear are that:

- much of your research is too narrow and piecemeal.
- unless results from your research are used by the agricultural community, then the research can be regarded as having been of doubtful significance.
- agronomists must increasingly be prepared to work with economists/ soil scientists/pathologists/entomologists/ecologists and engineers to improve the relevance of their research. (This was enlarged upon by Dr Wood in his paper on new crops.)
- agronomy does not finish at yield measurement. As stated strongly by Mr Johnston and Professor Freebairn, it is consumers who will finally choose to accept or reject the products scientists are helping to produce. Lack of attention to the transportation, processing, presentation and retailing requirements of the market can mean years of wasted research effort, as well as financial difficulties for farmers who have possibly been encouraged to adopt research results that have not been thought through.

In this category of systems perhaps scientists should, at the minimum, take stock of the relevant farm management questions and tailor their experimentation to answering these questions in close association with the community they are trying to serve.

Despite major upheavals in recent years in the horticultural industries, requiring rapid farm adjustment and on-farm productivity improvement, only 4% of papers were devoted to horticulture. Judging from the range of papers presented there is an obvious lack of balance in addressing problems confronting the various horticultural industries. The research program seems highly selective in terms of crops, and there is no balance in research on the sequence of operations on horticultural farms ranging from variety choice to the specialised problems of growing and harvesting horticultural crops. I temper these comments by referring to the balanced and detailed review of horticulture given by Dr Possingham. Despite this, there would seem to be distinct opportunities for high-payoff research, which are not being addressed.

Related to the above, the growing of horticultural crops in Australia is inextricably linked to irrigation and our water resources. It is now recognised that Australia is a maturing water economy, in which an expansion phase characterised by increased use of irrigation water is being replaced by a phase characterised by water scarcity and problems of damage to soils and crops arising from years of application of possibly excess water at inappropriate prices. Water problems are likely to take Australia by surprise in the next years. It is, therefore, of concern that less than 10% of papers were directed towards the broad issues of water use, irrigation systems and water/soil interactions.

Agronomists are responding too much to bygone issues here. In the key irrigation areas farmers, in general, are now well and truly up against a water use constraint. As stated by Dr Possingham and Dr Smith, it is a matter of time before competition for water in its widest sense becomes so intense that institutional changes in pricing and administration will be enforced. Under these circumstances, it is unacceptable to hear detailed reporting of results from different irrigation treatments in which the volumes of water applied were not even measured. Further, it is irresponsible to encourage farmers into massive irrigation investments such as land-forming, establishment of large bays, etc., without adequate soil and agronomic information on the outcome of such investment.

Given the strong likelihood of fairly drastic structural change on the irrigation scene, as anticipated in Dr Smith's paper, forward-looking agronomy research should be aimed at obtaining quality crop/livestock output from irrigation while minimising water use and other inputs, rather than at ways of getting rid of excess water, at water-logging effects on soil quality, or at ways of making short term additional revenue from increased water application without considering the detrimental effects on the broader farm unit and all elements in the costs/returns equation.

Finally, I would like to comment on the way in which much of the technical agronomy research is being undertaken. Two developments suggest the need for rapid adjustment in the way scientists conduct their research.

First, the days of large research grants and guarantees of finance for extensive field experimentation are no longer with us. Capital fixed in long-term specialised field research equipment is increasingly less easy to defend to industries and governments allocating funds for research, especially if this equipment is used to support ad hoc experimentation. Furthermore, applications for research funds to conduct experiments which are seen as 'one-off' by research managers and providers of funds are going to be increasingly difficult to justify.

The second development is that at the same time as research funds are drying up computer technology has never been more versatile, with prices of both computer hard and software declining in real terms. Given this, scientists must therefore much more seriously think about combining cost-saving methods of experimentation with imaginative use of computer technology.

There is a need for well-thought-out replication and sequences of experimentation which cover essential points on a particular response surface rather than the traditional section of the response surface derived from factorial-type designs, whatever their complicated improvements. There is an obvious cost saving associated with careful and planned selection of points on the entire response surface associated with any research problem rather than piecemeal repetition of the same types of experiments. This does not happen to any great extent at present and, frankly, I am disappointed with the lack of cohesion in the overall agronomy research program because of this.

In addition, much of the 'one-off' research on the present agenda, while leading to easily documentable results, is pretty useless to the person who wants to put together a picture of responses under different combinations of inputs and with reference to the broader management system. This situation obtains even at the regional level. It is most frustrating to researchers in related fields, such as economics, advisers and agricultural system modellers, who search in vain through reams of technical research papers only to find that piecemeal results from these papers simply cannot be linked to give the required response picture. Resort in these situations is often to hand constructed graphs verified simply by exposure to agronomists and extension officers. Much of the present agronomy research is therefore never used by investors and decision-makers in agriculture.

Scientists must put more effort into exploring the dimensions of the problems they have decided to research: i.e., the farming community's requirements in terms of aspects of the cropping system, on which these people are required to make investment and managerial decisions.

Scientists must become much more aware of the final use to which their research is to be put. This requires a more 'goals-orientated' research agenda, 'research' not being confined to a documentation of the experiment. An example is the farm experiments and surveys conducted by some of the fertilizer companies. Another example is the interesting forms of obtaining experimental results being developed by Professor White and his team at Lincoln College. Addressing the question of "What do investors and decision-makers in agriculture require from me as an agronomist?" would assist greatly in improving pay-off of the overall agronomy research program.

Pursuit of cost-saving methods inevitably implies that alternative methods must be developed to achieve comprehensiveness in the research program. One answer to this problem is to use computer simulation methods to: (a) build representations of the system which are then used as a basis for 'experimentation',

and (b) begin to identify key bottlenecks in the system which may have high research pay-offs. Classically these "bottlenecks" can be identified through sensitivity analysis in such models. Research attention can then be diverted to clarifying or exploring the implications of change in these bottlenecks.

Building of systems models must become more purposeful rather than the somewhat indulgent modelling that has occurred to date. Modelling has tended to take place as an end in itself with virtual discarding of the model after building and minimal experimentation. There must be a commitment to better writing up, distribution and use of models. The ability of these models to address relevant problems must be improved and quick updating facilities must be included.

In summary, an ordered system of experimentation involving revised exploration of response surfaces coupled with new ways of using computers would, I predict, stand the agronomy profession in good stead in years to come as competition for the scarce research dollar increases.

In order to give direction to the agronomy research program for the years ahead, it is important to provide some indication of whether change is likely to occur in Australia's rural industries. In this respect your program committee has scored highly again by ensuring that the necessary information is available to the Conference via the opening papers. I will attempt to summarise likely developments identified for agriculture.

The past two decades have seen major expansion in world agricultural production. Key factors have been: (a) expansion in resources, particularly arable land devoted to agriculture, and (b) increased productivity due to improved farming systems and adoption of new technology.

As identified by the opening speakers, long term export prospects for Australia's wheat, beef, sheepmeats and live sheep, wool and sugar appear to be sound, reflecting our international competitiveness in these areas. Fluctuating world economic activity and attendant instability and protectionism in world agricultural commodity markets are key considerations. The fortunes of our producers depend crucially on the domestic policies of our trading partners. Export prospects for other industries are not good. However, most horticultural industries, the dairy industry and intensive livestock will undoubtedly continue to have a sound future in supplying the domestic market.

Current growth rates in production appear to be sufficient to meet the level of demand to the mid-1980's. As the decade progresses this situation does not appear to be as clear-cut. Increasing resource limitations as arable land is used up, and energy-based inputs such as fertilizers, chemicals, irrigation and machinery become more costly, will mean that future expansion in food production will have to come from ever higher levels of productivity, particularly through the development of new technology. Such technology will have to be increasingly sensitive to economic and environmental constraints.

There are indications from this Conference that the required productivity increases are likely to be forthcoming. However, constraints on production will be encountered which will make marginal increases in agricultural production, within the present mode of production, ever more costly to obtain. The reason is that increased intensity of competition for ever-scarcer resources, and the costs to society of ever more intensive use of resources for food production, will have to be forcibly addressed, with possible repercussions for property rights in agriculture. In this regard the interesting papers from the State Departments of Agriculture and the summary by Dr Claxton have forcibly pointed to rising concern for the way in which our natural resources are being used.

A key element in the survival of the rural industries will therefore be maintenance of our competitive position in trade and within the economy. The importance of continuing competitiveness in world trade need not be elaborated upon again. Agriculture is also likely to come under increasing adjustment pressure in the 1980's arising from structural changes in the economy. Key issues in this regard are energy, finance, intersectoral competition for resources and associated expansion of the mineral sector, changes in the balance of payments and exchange rate management, industry protection and change in other macro-economic policies. Emphasis in agricultural policy will continue to be *away* from trying to offset cost increases towards creating an environment which facilitates change.

The 'bottom line' from all this is that farmers will continue to have to undertake productivity improvement to maintain their levels of real income. They will do this by increasing production from existing resources through technological change, as well as by altering the size of their unit to match their attendant packages of inputs, including labour.

This type of statement is commonly translated into an "ever bigger, ever better" philosophy. I want to stress that this need not be the case. In some instances farmers will be able, and willing, to exploit economies of size with attendant and continuing substitution of capital for labour. This has been the trend to date. However, there is a limit to the degree to which we can work against nature using increasingly 'artificial' forms of agriculture.

Increasingly, and given the abovementioned constraints, we are likely to see a significant number of our farmers make conscious decisions to remain with the more natural forms of agriculture on which much of Australia's rural industry was founded. This type of agriculture, based on family farming, will now, however, increasingly benefit from sophisticated small technology and will likely be characterised by reduced use of expensive purchased inputs, smaller machinery and plant, and improved management through scientifically-based conservation practices. Agronomists can assist greatly in this area by turning their minds to development of sophisticated technology suitable for small-scale agriculture. There is a big challenge here. Minimum tillage systems with their emphasis on concern for the land, selectivity in use of chemicals, and attention to 'management' of the crop in its true sense, are I believe, the take-off point for this development. The 1980's will, therefore, likely see the emergence of a duality in agricultural production systems in Australia, the implication being that productivity increase should not be thought of dogmatically in terms of ever-bigger farms and the displacement of labour.

Whatever the ultimate structure of production, output from Australian agriculture will expand. To give some idea of magnitudes, preliminary results from simulation studies in the Bureau indicate increases in wheat and other broad-acre crops of 25-30% by 1990 and increases in livestock of around 10%. These figures are supported in some of the State Department of Agriculture papers. Key features of agricultural systems in each State as a result of this expansion are also outlined in the State Department papers.

An important development in this area appears to be the prospect of double cropping. I am very much less concerned by the prospect of two crops per year or three crops per two years, subject to the correct management and conservation principles discussed by Dr Phillips, than I am about development of cropping systems on marginal lands. The examples shown at this Conference of land degradation and runoff problems on lower country as a result of large scale clearing of forest on marginal lands are sobering.

Given all this, many areas of likely high research payoff can be identified from the present agronomy research agenda. However, there are also areas of serious under-representation of research effort. A broad summary of major areas of research interest and a judgment on allocation of research resources to each would be useful:

As stated by Dr Ozanne and Dr Scowcroft, it would seem that it is a matter of time before genetic engineering techniques are sufficiently refined for use in producing new crops and plants with greater yield potential and better ability to resist stress arising from disease, pests and climatic variability.

Work appears to be advancing on development of new plant systems to improve biological nitrogen fixation and uptake of nitrogen, phosphate and other nutrients from the soil. Furthermore, there is potential for improvement in efficiency of fertilizer use; the decline in clover and medic pastures is alarming and deserving of high-priority research; and research on plant nutrition should be integrated with research on plant systems, and not conducted in isolation.

Successful research is being undertaken in the area of reduced tillage techniques which, together with selective use of chemicals, is helping to reduce reliance on purchased fuels and to maintain soil quality.

New developments in farm machinery to improve cultivation operations from land preparation to harvest have been introduced. Development of further new technology in this area is likely to have a potentially high payoff. There would appear to be openings here for machinery manufacturers which are not being taken up.

Work is progressing on development of integrated approaches to control pests and diseases, involving combinations of biological, genetic and chemical control.

As indicated by Dr Lovett and his colleagues, there is evidence that new uses of agricultural waste products such as stubbles can contribute to soil conservation practice as well as to improved productivity.

Work concerned with the conservation and management of our natural resources - water, soil, biomass and air - in the production of crops is of highest priority. Particular problems relate to combating soil erosion and salinity problems in irrigation, and to cropping systems that promote soil quality. There is urgency in tackling these problems: they are under-researched. It is this whole area which worries me most in the present climate of expanding agriculture.

Exploration of organic farming techniques, much less reliant on purchased inputs such as chemicals and artificial fertilizers, is under-represented and is a challenging area for research.

The impact of new, larger machinery systems on soil quality and production requires further research if irreversible damage to soils is to be avoided. In this respect some evidence is beginning to emerge on the possible problems that could accompany large-scale mechanisation (1).

Technical improvements beyond the farm gate, particularly in the areas of storage, processing, presentation and product characteristics of crops, are important if future bottlenecks in operations beyond the farm gate and problems at the retail level are to be avoided.

Greater attention to the broad range of crops rather than the present selective research is to be encouraged. There is imbalance in the research effort associated with individual crops.

Economic analyses of crop production in order to assess the cost-reducing impact of agronomy research on the farming system is crucial if technical results are to be useful to decision-makers. Little attempt is made in the present agronomy research program to draw out the economic implications of technical research. However, this would seem to be a key avenue by which scientists can 'extend' their research results to users. Economics is not just a matter of gross margins and partial budgets. It is concerned with estimation of the opportunity cost or value of resources and the assessment of private and social benefits and costs of alternative ventures. Incorrect pricing and assessment of net benefits by advisers can lead to incorrect investment decisions by farmers. In general, scientists have concentrated on technical areas within a particular farming system to the detriment of the farm as a unit. There is considerable scope for closer interaction between physical scientists and economists in: (a) setting up experiments for best use of results by agricultural operators, and (b) drawing out the wider implications of technical results.

Finally, the whole issue of formulating relevant research programs and the setting of priorities between programs and projects is going to loom large in the next years. Scientists should react quickly to this challenge. As mentioned earlier, the way in which scientists conduct their experiments and use computer technology is a major area where cost economies can be effected in research programs.

1. Wilson, B. 1982. Farms : Too Big for Their Boots. World Agricultural Report, May.