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# PROCEEDINGS OF THE TWELFTH INTERNATIONAL CONFERENCE OF AGRICULTURAL ECONOMISTS

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## Disparities in the Pace and Form of Agricultural and Rural Development

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# CO-ORDINATING ECONOMIC RESEARCH AND TECHNICAL RESEARCH IN AGRICULTURE

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THROUGHOUT a varied career as school-teacher, professor, scientist, farmer, and now banker on the international development front, I have been in many strange and difficult places. None has been more strange or difficult than this, my present role amongst economists of world stature. I am not an economist, so that on this programme I feel very much alone. No longer am I engaged in agricultural research so that on this count too, I could be disqualified as unsuited to handle the topic now before this conference. Yet I am proud to be here and deeply conscious of the honour accorded me by your invitation. I might add that none has been more surprised than my colleagues at the World Bank to see my name on your programme. The Bank prides itself on its collection of high-powered economists. It certainly does not count me amongst their number. In all these circumstances, I can but try to justify the confidence your programme committee has placed in me in respect to the important assignment with which I am entrusted. My only qualification for attempting the task is that I am supposed to have achieved some practical success in co-ordinating economic and technical research in agriculture. Apparently, I am credited with actually doing this job over the last twenty-five years when I led fairly large groups of research workers in my native land. With this background, perhaps the best way of leading into the subject is to outline my personal philosophy on the issues involved, and thereafter describe the methodology and results which have been the inevitable outcome. From such an outline, the prerequisites and conditions for a more widespread application of co-ordination may emerge.

Throughout my research life I have always believed firmly that the prime responsibility of the agricultural scientist is to serve the industry of which he is a part. In any developing country, this defines very clearly the work to be done. In practice it means applied rather than basic research. It leaves basic research to the more wealthy, developed countries which can afford the luxury of the pursuit of

knowledge for the sake of knowledge. It means that the agricultural technologist takes established principles and promising theories and develops from them techniques that can be fitted into a pattern and programme of production. Acceptance of this view does not imply disinterest or non-participation in basic research that turns up new knowledge, evolves new theories, or modifies old ones. Soundly designed and executed applied research often does these three things as by-products of the main objective. It is fortunate that this is so, because it is this that recruits, and retains within the field of applied agriculture, those bright creative minds that are essential to it. The key point is that, in contrast to the attitude of the purist who avoids as a plague any taint of economic justification for his existence, the agricultural research worker is proudly dedicated to the underlying motive of usefulness.

Acceptance of this attitude points directly to methodology. I do not need to remind this audience that farming is a business and as such is controlled by economic forces; that these forces are basic and can have effects upon agricultural performance of individuals and countries even more potent than the forces of technical efficiency. The agricultural technologist cannot afford to ignore this at any time. He needs the active co-operation of the economist at many stages of his work. He needs this in defining, high-lighting and pinpointing the problems requiring research for their solution if his programme is to be soundly based on foundations of usefulness. He needs the economist in interpreting application potentials of his results. He needs him in evaluating the consequence of his contributions upon the economic health of the industry. Clearly, this co-operation must be on a continuing basis. Not only should economic attitudes lie behind any applied technical research programme, but changes in programmes which time must bring should be similarly based.

Acceptance of these views is not without its impacts upon the work of the agricultural economist. To the degree needed in any given circumstances, his research priorities must lie in the direction of appreciating his direct responsibility to assist the technologist. He must provide appropriate analyses of economic forces capable of modification by technical attack. He must provide adequate measurement of the impact upon efficiency of definable technological weaknesses. He must be able to delineate the areas of economic weakness as guidelines to the technologist who has to decide just where his scientific effort may best be concentrated. The economist must do all these things with a keen sense of priorities. Perhaps his greatest

function is to pin-point needs. In other words, co-ordination of effort of the type envisaged in the title of this paper implies that the agricultural economist also should be motivated by the concept of usefulness. In this he must appreciate his dependence on technology, for his attitude to be creative and forward-looking, rather than merely descriptive or historical. Without mutual appreciation of the need for and the potentials of conscious partnership along common lines, each group runs the risk of working in a vacuum. It is a tragedy that this risk is a reality in so many countries.

When I began research some twenty-five years ago, my thinking was strongly influenced by a monograph on the New Zealand dairy industry published in 1926 by the late E. J. Fawcett, a Cambridge-trained agricultural economist. Fawcett had spent several years in the extension field and later became Director General of Agriculture. He was the first to make a critical appraisal of the dairy industry in terms of inputs and outputs and of the relative significance of factors affecting efficiency. Based on the survey approach the study, which has largely dominated research thinking ever since, high-lighted five main points: (1) It focused attention on the overwhelming importance of pasture, rather than forage crops and concentrates, to the efficiency of the New Zealand dairy industry. (2) It high-lighted the key importance of output per labour unit within the New Zealand economic scene. (3) It raised doubts on the importance of high yield per cow—elsewhere a major criterion of efficiency—under the peculiar economic and production conditions operating in a grassland environment. (4) It stressed the major contribution of animals per unit area rather than output per animal, to output per acre, and hinted at possible important interactions between them. (5) It advanced the concept of output per acre as the most significant single criterion of efficiency within the then and likely future economic structure of the industry, and advanced production targets in such terms.

Since Fawcett's original contribution, others have carried on the work and covered the whole range of farm production in New Zealand. The New Zealand Dairy Board set up a special group to make continuing studies of a similar type, supplemented by efforts to isolate technical causes of inefficiency. Encouraged by this example and the many dividends that quickly accrued to dairying through technical research stimulated by and aligned with economic findings or suggestions, the Meat and Wool Producers Boards followed suit and established an economic group with comparable objectives. Other farming bodies along with the agricultural universities likewise contributed. By and large, the coverage has been such that, for

many years, no technical researcher has been under any illusion as to the precise economic niche into which his or her particular contributions might fall. Equally, the economic researcher has benefited by the two-lane bridge thereby created. It was in this general climate that I began to develop the Ruakura Animal Research Station twenty-one years' ago. The Station's role was to study the whole field of animal production—New Zealand's greatest industry—by welding into patterns of production all the manifold contributions of specialist science. I was never in doubt as to what the research programme of the station should be. With such a strong personal economic bias, I had likewise no doubts how the programme should be implemented. Lastly, I soon became aware of the organizational implications.

Taking implementation first, the 2,000 acres of Ruakura quickly became divided into forty or fifty farmlets each carrying sufficient stock to permit not only sound scientific interpretation, but reasonable economic appraisal of the effect on production efficiency of the particular factor or factors under study. This experimental farm approach as opposed to the normal field-plot technique, involved the annual use of some 750 head of beef cattle, 750 head of dairy cattle, 6,000 sheep, and 1,000 pigs. This farm-scale operation accelerated the application of results to the industry as a whole. On the programme side, the objectives called for studies on feeding, breeding, and management in a grassland environment. Nutritional teams were charged with the responsibility of studying pasture as a food for ruminants. They were specially interested in efficiency of pasture utilization, the problems of pasture production having already been mainly solved. Their main yardstick was output per acre. The interactions of this with output per animal and stocking rate became a question of great fundamental and practical economic importance. These teams were also intrigued with problems of pasture quality in relation to animal performance as providing the key to better production on problem lands and pastures. The contributions of the groups concerned have become internationally recognized. They have affected the thinking and the approach of grassland workers throughout the world, resulting also in much rewriting of standard texts on animal nutrition—a science developed mainly from studies of the stall-fed, concentrate-fed animal rather than the free-grazing ruminant which characterizes most of the sheep and cattle world.

Animal breeding teams faced the very real challenge that high output per animal might not necessarily be the most desirable goal. They concentrated on inheritance/environment studies designed to

elucidate the key interaction problems. They brought animal size into their calculations as a determinant of efficiency in the special conditions of a grazing environment. They were able to demonstrate the superior contribution of animals of small size and high genetic merit to output per acre, even in poor environments where stock is subjected to considerable stress. They developed breeding methods to provide such stock. In other words, they too adopted essentially an economic attitude to their work.

On the management side, a whole host of studies were aimed at plugging demonstrated leaks in the efficiency picture. Output per labour unit as well as per acre were the underlying targets. Thus, the daily task of milking cows, the greatest single labour demand of the dairy farmer, was tackled by a milking-machine team. Its contributions of non-stripping, machine stripping, the herring-bone milking shed, circulation-machine cleaning, and improved milking routines based on time-and-motion studies and physiological experimentation soon led to major improvements in milking management. They stepped up the practical level of cows milked per man from fifty to ninety head. These contributions alone today permit the national herd of two million to be milked by 8,000 fewer milkers than were needed fifteen years ago, a wage saving of more than £6 million per annum. On the animal-health side, effort was concentrated on management procedures designed to reduce the impact of the most serious causes of herd and flock wastage. Industry surveys pin-pointed these with accuracy so that, one by one, major scourges have been eliminated from flocks and herds, and the average length of productive life of livestock raised to a level higher than in any other major livestock country.

On the organizational side, it cannot be too greatly stressed that this approach virtually forced the adoption of a special type of research organization. It insisted on a project basis to research in which the requisite specialists, including economists, were drawn together as a team with the common objective of attacking the problem decided upon. This is in marked contrast to the more usual type of organization where scientists are grouped in relatively independent specialist departments and where, in consequence, co-ordination and particularly economic co-ordination is most difficult to obtain. The project method tends to keep the feet of the scientist firmly on the ground. Specialist departmentalism so easily leads to his occupying a zone so high in the air that he is rarely conscious that the farmer exists as an economic being.

This approach has had marked effects upon industry output and

efficiency in New Zealand as even a casual examination of my country's export data indicates, and economic studies support. More important to economic development, technical research of this type has extended greatly the horizons of production potentials. Fawcett's target of 200 lb. of butterfat per acre for 1926 had to be lifted, first, to 300 lb. in the 40's and to 400 by the 50's. Before I left the station in the early 60's, the 500 lb.,[1] barrier, like the four-minute mile, had been broken. Similar expanding targets for meat per acre have also been set on the basis of proven performance. To justify my earlier contention that even *ad hoc* work of this type is capable of turning up new knowledge of basic importance, it is not without significance that the relatively small Ruakura professional staff earned, over the period, five doctorates in science and nine doctorates in philosophy, all from universities of high international standing. Lest it be thought that the units under my control alone have been responsible for progress, or alone have embraced co-ordination with economic thinking, let me stress that this is not so. Most New Zealand technical research in agriculture has been and is economically orientated. All the major advances have so originated. The scope for further progress along the same lines is great.

Thus, the continued concentration on phosphates in association with clovers as the key to high-level production of animal food from permanent grassland has been due to continued demonstration and appraisal of the economic soundness of this approach. The unique pioneering development of aerial application of phosphates in association with legume seed to over ten million acres of hill and mountain lands, with almost incredible benefits to productivity, arose not merely from the economic necessity to use such areas as a matter of top priority, but from a full appreciation of the continuing need for low labour costs and high outputs per labour unit. The spectacular transformation of millions of acres of New Zealand lands by application of the trace elements, cobalt, copper, molybdenum, or selinium did not arise from the inquisitiveness of backroom scientists on the role of individual chemicals upon plant and animal health. It sprang from the determined and sustained effort of scientists dedicated to the conversion of useless lands into usable ones.

So much for the philosophy, the methodology and the consequences. Of greater significance to this meeting should be ways and means of bringing about greater co-ordination between economic and technical research than exists at present. There is a special need

[1] For European workers accustomed to thinking in terms of milk, this is equivalent to 1,200 gallons of 4 per cent. F.O.M. per acre from pasture alone.

for this in the under-developed areas of the world. Deliberate organization toward co-ordination is not easy to envisage or to accomplish. So much depends upon the attitude to research—the attitude of scientists, of farmers, of politicians, and of the general population. This attitude springs from the complex of forces that mould and develop the kind of people of a country. It is derived from the kind and quality of their education at all levels. It is geared to their standards of social and economic development. It stems from the people's needs and the extent to which these are consciously recognized by the majority.

Obviously, the strong association between economics and farming technology in New Zealand cannot be attributed solely to Fawcett's simple monograph. This merely focused attention on problems and indicated ways and means and in particular indicated priorities. The reasons why I and my associates have gone about the business of technical research in the way we have is because our environment forced this approach upon us. We were conscious of the tremendous and pressing problems of a raw and undeveloped land. We belonged to a young nation in a hurry to grow up. We were interested in a high standard of living for our people and ourselves. There was no time to adopt the leisurely approach of the academician. Our immediate needs were pressing and clear cut. In this general climate, we had no alternative.

One is tempted to draw an analogy with Denmark, where the economic crisis at the turn of the century through the opening up of lands of the new world, forced a new way of life on the people and, perhaps, was responsible for the economic bias of technical research for which Denmark has been renowned ever since. One is equally tempted to look at the converse in the United Kingdom which so long relied upon the farm lands of her dominions and colonies to the detriment of her own agriculture, a situation largely responsible, perhaps, for the preoccupation of British agricultural research workers with the pure rather than the applied, and the lack of conscious planned attack on production problems of economic importance. When I reviewed the organization and programmes of the major production research stations of Britain in 1958 on behalf of the Agricultural Research Council, it was noteworthy that none employed an agricultural economist, and that so few projects were based or justified on economic grounds. It was staggering to find scientists so rarely exposed to the thinking of farmers and with such little knowledge of, or interest in, the practical problems of the industry they were selected to serve. In making such comments I am not

unconscious or unappreciative of the substantial contributions to knowledge that have come from the British system. It is not without significance, however, that countries other than Britain herself have capitalized on this knowledge more rapidly and efficiently.

During my last two-and-a-half years of service with the World Bank in a wide range of undeveloped countries, one outstanding impression has been forced upon me. This is, that while organized research in the field of agriculture is largely noticeable by its absence, what does exist rarely involves a partnership between economics and technology of the type under discussion. Most research in practice in these countries is based on the thinking and approach of the sophisticated highly developed Western World. It is seldom orientated in the direction of the countries' development needs. Research trainees have tended to go back home from overseas training without any conscious appreciation of the dignity of useful work. Their training has often been too specialized and their subsequent research designed far more to provide the prestige of publication in a scientific journal than to bear upon pressing local problems. A wide range of examples of this could be quoted if time permitted. In practice, this widespread situation constitutes one of the great barriers to investment lending in agriculture. Far too frequently, our attempts in the World Bank to finance agricultural development projects capable of contributing to the economy of the individual and the country, are handicapped or frustrated by quite inadequate local data of methods, possibilities, and potentials which an economically oriented research programme could have provided. Too often the Bank is thrown back on educated guesses in order to participate at all. The danger of such guesses needs little emphasis. There is a tremendous need for pre-investment studies which are soundly based in terms of both economics and technology. This international gathering, at this present stage in history, could perhaps do no better job than to use every effort to draw the attention of emerging nations to these deficiencies and to encourage and assist any movement toward remedying them. In this connexion it is worth stressing that the chances of effecting economically oriented agricultural research should be far greater in countries where research organization is virtually absent, or only in the process of evolution, than in countries where the vested interest of long established organizations tend to work against any change in outlook or approach.

In summarizing, I would suggest six prerequisites for co-ordination of economic and technical research in agriculture. These are: (1) Both economic and technical research workers in agriculture

should be motivated mainly by the ideal of 'usefulness'. (2) Both groups should be trained to appreciate their interdependence and that the full benefits of their respective contributions depend upon a continuing conscious partnership. (3) Research administrators should be selected who appreciate the desirability and need for this co-ordination, so that their policy in leadership, in the search for funds, in the formation of priority programmes, in organization, and in the selection of staff is determined accordingly. (4) Research organization should be on a project, rather than on a departmentalized, basis so that the targets and responsibilities of each worker are unmistakably delineated. (5) Politicians and governments, particularly in undeveloped countries, should be urged to concentrate much less on the prestige of academic research and much more on research geared to the pressing developmental needs. They should be helped and encouraged to this end. (6) Training of scientists in the western world for service in undeveloped lands should be re-examined and reorganized in the light of the high priority of economically oriented research.

J. F. VAN RIEMSDIJK, *Bennekom, the Netherlands*

I wish to thank Dr. McMeekan for his paper and to give him my congratulations for developing the Ruakura Animal Research Station where the conditions for programming and executing agricultural research work are ideal. Still more important, they seem to be used to the best advantage in creating team-work which, as he says, is welding into patterns of production all the manifold contributions of specialist science, all kinds of technical ones and of economics.

New Zealand farmers, at least those engaged in animal husbandry, are lucky to be served with the findings of such excellent team-work. The more so, when taking into account a statement of Dr. McMeekan in his book *Grass to milk*. This reads that the extension service in New Zealand has an enviable record of achievement in the way of carrying the results of research to the field, of testing new ideas on a pilot scale, of guiding the intermarriage of research and practice in the complex business of farming, and of bringing back to the researcher ideas and problems that keep science on its toes. To avoid misunderstanding by picking out these quotations, I hasten to add another statement of Dr. McMeekan, that much of the progress which changed the picture of New Zealand agriculture dramatically in thirty years was due to the farmers themselves. By trial and error they have done much of the job without organized aid. Yet it is true, he says, that most of the changes have been based on research.

*Summa summarum*, according to all this we might conclude that

the problem of co-ordination would be solved simply by copying the New Zealand system in other parts of the world and in other fields of production. However, it is not claimed that New Zealand farmers are satisfied with the set-up as developed by Dr. McMeekan. And, even if they are, the question still remains whether they should be. I should like to restrict my comment to this point.

Dr. McMeekan's book, *Grass to milk*, gives some evidence of farmers not being satisfied. He quotes part of a letter he received from a young farmer complaining about what this farmer termed 'the all-too-frequent error of agricultural scientists arriving at the wrong conclusion from their own experiments and in consequence recommending methods that few farmers have any hope of applying'. I do not believe that the charges of this young farmer were justified by the reasons he gave. However, one of his observations is worth mentioning, namely: 'The problem of what to do is kids' play compared with the problem of how to do it.' This observation has much to do with our problem of co-ordination. A failure to co-ordinate technical and economic research up to the farm level may lie at the root of it. We cannot deny that year after year big differences in farming results are to be found which cannot be due to any factor other than the very complex one of management. In the Netherlands, for example, within groups of arable farms which, apart from the management factor, seem to be potentially quite homogeneous, some 50 per cent. of the differences in balance of costs and returns per hectare are due to differences in physical yields per hectare. However, the specific factors which are responsible for the differences are unknown. We can only assume that a substantial part of the unknown factors is controlled in a favourable pattern by some farmers of the group and either not at all, or insufficiently, by others. Obviously, if this is true the question can only be answered by technical research. Applying technical research in this area would bring technical researchers together with farm management workers in direct contact with a range of farms and farmers, resulting in a wider field of co-ordination than that developed in the sphere of experimental farms in Ruakura. In my opinion such an attack, which should be in addition to, and not in place of, the work already done, could bring results as useful as those of the Ruakura system.

S. R. Wragg, *University of Bristol, U.K.*

I have been actively engaged during recent years in a number of projects, initiated by O.E.C.D., which are concerned with the co-ordination of economic and technical research in agriculture.

My early enthusiasm for this subject came to me partly as a result of reading about the work being carried out by Dr. McMeekan and his colleagues at Ruakura. In particular, I was attracted by their simple yet highly effective system of experimentation from which there appeared to flow a steady stream of directly applicable quantitative solutions to problems of production and resource use. Moreover, both the problems and their solutions were conceptualized in terms for the most part consistent with conventional profit maximizing models. More recently I have come under the influence of an equally seductive spell cast by Professor Heady, with whom I am currently associated in O.E.C.D. projects relating to this particular field of study. Using a combination of simple arithmetic, and perhaps as many as 150 cows per experiment, Dr. McMeekan produces sometimes three, sometimes four, simple input–output coefficients. In contrast, using only one quarter the number of cows but treating their results to a vastly more sophisticated form of mathematical analysis, Professor Heady presents us with a production function containing eight variables and twenty-seven coefficients together with many other parameters derived from the basic function. Although I am no less convinced of the imperative need to co-ordinate economic and technical research, this illustration should make it perfectly clear why I have a number of unresolved doubts concerning the scope, form, and methodology most appropriate to this end.

I would not dissent at all from any of the criticisms which Dr. McMeekan makes about scientific research and research workers in the United Kingdom. I would merely say that the situation appears to be little if any better in most other countries in Europe. The real reason for the agricultural scientists' preoccupation with pure research, or with applied research of no apparent practical value, is that in most European countries there are far too many agricultural scientists employed in relation to the quantities of non-human resources which are available, but which are essential if really effective applied research is to be carried out. This situation arises from one very simple fact. It is cheaper to employ a scientist who asks for nothing more than a desk, a microscope and perhaps a cage of rodents, than one who, for example, insists on having a herd of cows—perhaps he needs a large herd—together with the supporting area of farm land, buildings, and equipment. There is a corollary to this. It follows that for a given sum of money it is possible to employ more of the former class of scientists than of the latter. If, in addition, we recognize the universal habit of equating size with status we can recognize immediately the expansion path leading to maximum prestige at minimum cost.

The most common criticism made by economists in Europe and the U.S.A. about the research activities of agricultural scientists is that their experimental designs do not include sufficient treatments to enable estimates to be made of the whole of the relevant sector of the input–output function. The reason often given for this is that resources are insufficient. But Dr. McMeekan, with 2,000 acres, 1,500 cattle, and 8,000 sheep at his disposal could, and I believe on occasions did, assign more land and stock to one single treatment than is available to some research institutes in Europe to meet the requirements of the whole of their research programmes. Yet, we find that most of Dr. McMeekan's input–output type experiments are limited to three treatment levels. I would like to ask him whether his work has ever been criticized on this point by New Zealand economists and whether he would agree to having failed, on occasions, to include in his experimental design the whole of the relevant sector of the input–output function.

Finally, I would conclude with one or two observations about training and research in the under-developed countries. Dr. McMeekan has referred to the dangers to which young scientists from the under-developed countries are exposed when they visit the richer and more highly developed countries for graduate or post-graduate training. I feel bound to say that the dangers are no less acute for students of economics coming to study in Europe or the U.S.A. This applies particularly to those who develop a strong taste for theory, model building, and mathematics. For, as Professor Heady himself pointed out in discussion last week, the econometrician's limiting resource is neither concepts nor computing facilities but hard facts. There is a great deal going on in the econometric field which is just sheer luxury made possible by affluence. New hypotheses are set up, not because the old ones have been, or even can be disproved, but because it is dull work merely contemplating a set of equations. If, in the under-developed countries the criterion for judging research is its relevance to the solution of urgent problems—as assuredly must be the case—then precisely the same degree of realism and simplicity is called for in the approach to economics as Dr. McMeekan demands for agricultural science. This is absolutely essential if economics and science are to be successfully integrated.

M. Shafi Niaz, *Planning Commission, Karachi, Pakistan*

No one would agree more than I do, as an agricultural planner, with Dr. McMeekan when he pleads the idea of close co-ordination between economic and technical research in agriculture. In a

developing country where resources, especially in terms of trained personnel and even of funds, are scarce, such a co-ordination assumes even more importance. It reminds me of Parkinson's statement that in a country where resources are unlimited, the only economy that is done is in thinking; the reverse should be true in a country whose resources are limited. However, the agricultural economist, on a sound analysis of the economic situation of his country and of others concerned, can guide the technical research workers to the problems on which they should focus their attention, those which are 'useful' from the national point of view. For example, he can indicate what shifts in cropping patterns are warranted in the light of the changing economy of his and other countries. Such an indication will point out to the technical research workers what type of work is needed to meet the needs of the changing economy. That is to say, it can indicate whether new high-yielding, drought-, salinity-, and disease-resistant and otherwise better varieties of crops are needed, or whether the efforts of technical research workers should aim at anti-salinity and anti-water-logging measures. It would also help in determining the priorities within problems.

I need not emphasize that the programme of research should be formulated after very careful thinking and that priorities should be allotted to the various problems needing solution. This is necessary if the scarcest resources are to produce the most useful results. There is a tendency in some of the developing countries to set up new research institutions and research stations without considering whether there is really a need for them in the years to come and whether they can really help to solve the felt needs of the economy as recognized by the economists. This situation leads to the dispersal of the efforts of the few technically trained personnel available. The other day Mr. Sinha of the U.K. remarked that in India there was a tendency to construct palatial buildings for research but that they find it difficult to staff these institutions with appropriately qualified and trained personnel. This leads to the wastage of financial resources and to the thinning out of the available technical personnel, to the detriment of useful research work. This Indian experience, if it is true, should be taken as a warning. I feel strongly that the failure to achieve close co-ordination between economic and technical research in agriculture is due to a lack of proper communication and understanding between these two groups of workers. I cannot refrain from saying that one of the reasons for lack of co-ordination is the absence of understanding on the part of some of the foreign technical advisers who work in the developing countries under various aid programmes.

Some of these advisers, in order to show some spectacular contribution, want to exhibit their brilliant ideas in the form of new research institutions. Others suggest drastic reorganization of institutions without taking account of local conditions which will determine whether their ideas can fit in, and be useful, to the country concerned. As the words of these advisers are well heeded by the administrators in the under-developed countries, their recommendations are often accepted. This tends to go against the expected co-ordination which otherwise perhaps would have been easily achieved. I should not be misunderstood to mean that foreign advisers are wholly responsible for such situations. In many cases, their contribution has been very useful, and I do not under-rate it.

Another factor which contributes to the lack of co-ordination between economists and technical research workers in certain cases is the disagreement among the economists on the type and priority of the problems needing to be solved. This makes it difficult for the technicians to decide on the correct line of action.

The problem of better and more effective co-ordination remains. Dr. McMeekan has himself pointed out the need for it, especially in the under-developed areas of the world. A solution may lie, perhaps, in giving both economic and technical research workers a proper training in the ideal of 'usefulness'. Similarly, the administrators who are at the helm of affairs should be given proper orientation towards rural development problems and programmes. Such an effort has been tried in Pakistan through the setting up of Village Development Academies where administrators of all types and ranks, subject-matter specialists, and others are given short-term training in problems of rural development and allied matters. I can say with confidence that this programme is working very satisfactorily. Dr. McMeekan stated that research administrators need to be selected who appreciate the need for co-ordination. He also stated that politicians and governments should be urged to concentrate less on the prestige of academic research. These are very laudible suggestions, but I wonder at what stage and to what extent they can be really put into practice. Perhaps he will provide an answer.

I. N. Romanenko, *Kiev, U.S.S.R.*

The problem raised by the last speaker is of great interest to my country too, where a large amount of money is being spent on the development of science. We have to think how to use this money in the most effective manner. The scientists and economists of the

Ukrainian Republic, to the ranks of whom I belong, have had valuable experience in this field. In our Republic the scientific work is carried on in more than one hundred research and educational establishments; among them twenty research institutes, three research stations, twenty-two regional stations and about fifty chairs of economics in different educational, agricultural, and other institutes. To ensure the scientific management of agriculture it is essential to co-ordinate the studies not only of economists but also those of biologists and technicians.

All scientific studies are co-ordinated by a State Committee at the Republic's Council of Ministers. To give an example, economists, soil specialists, biologists, and people of many other qualifications are working together now on the problem of regionalization and specialization in agriculture. This kind of team-work enables economists to make their suggestions on the rational distribution of agricultural production, on the basis of the different natural conditions existing in the different regions of the Republic, and in each particular collective or state farm. The suggestions made by scientists are discussed at conferences of agricultural workers at different levels and, if accepted, are introduced into practice. Plans for scientific studies of the most urgent problems are co-ordinated by the institutes at the All-Union level. This, in its turn, helps the scientists and the State to achieve faster and more satisfactory solutions to the problems.

S. J. J. DE SWARDT, *Department of Agricultural Economics and Marketing, Pretoria, South Africa*

I want to draw attention to one point. It is that scientists are individualistic. A good scientist has to be so, and it is only human for him to crave for recognition. The very wide field of agriculture is becoming more and more scientific every day. There is more and more specialization. There are these specialists with their human frailities and they have to be welded into teams. This is one of the big problems of agricultural administration. I had the good fortune to be in the U.S.A. recently, when the successful shot to the moon was recorded on television. I thought they made a wonderful job of that. There you had all those specialists together in a huge scientific project in which the individual was just lost. And when it came to the successful outcome of the undertaking, what was stressed was the teamwork. About eight or nine of the top leaders were brought before the cameras and each was given the credit for his section. Although individuals, the big emphasis was on the success of the team as a

whole. It set a wonderful example to all of us in the handling of a situation like that. The problem was solved of recognizing team-work but also of giving recognition to everyone to whom it is due.

In South Africa, as in other countries, we have been struggling with this problem, and during the last ten years we have been trying to give it special attention. There are four principles on which we have tried to work. (1) Co-ordination at the top in policy committees, on which the three disciplines are represented: the purely scientific people, the agricultural economists, and the extension workers. This is a good beginning but it must be carried further. (2) At the project level it is also necessary to bring in individuals with different approaches and different disciplines. Wherever appropriate the economist and the extension worker are brought in, so that the scientific worker can know that in the end his results must be tested by their usefulness. (3) The question of recognition has to be borne in mind all the time. One has to cultivate the spirit of team-work but at the same time to give recognition to everybody to whom it is due. (4) The importance of the right selection of team leaders and administrators of the research. It is necessary that these principles be implemented in directing and inspiring the work if it is to yield the right results.

M. M. MALYA, *University of Aberdeen, U.K.*

It is generally not true to say that the recommendations of the Western experts, who visit and study the problems of underdeveloped countries, are wholly unrealistic or inapplicable. In the Indian context, it can be said with confidence that this has not been the case. A great deal of study, observation, and thought has been expended. Sometimes these recommendations have been made with caution, but we in our enthusiasm have often forgotten to heed the caution and have gone ahead too rashly. On the other hand, too often the recommendations are not implemented at all. I refer to the reports of Ashby in 1949, of Black and Stewart in 1954, and of Heady and Roenberg in 1959, in the field of agricultural economics. Reviewing the rural surveys in India during the past decade, it is seen that many of these expert recommendations are yet to be implemented.

H. PILHOFER, *Marketing Research and Economic Planning, Massey-Ferguson Ltd., Toronto, Canada*

I note with a sense of regret and urgency that technical and economic research co-ordination seems to be conceived as a co-ordination

only between academic and government people. There is another sector, however, the industrial sector, which in a free society at least is basically responsible for the innovating processes bringing about economic and social change. I would suggest that in the recommendations of Dr. McMeekan somehow the point be made that we try to find ways and means to co-ordinate the academic, institutional, governmental, and *industrial* research on the technical as well as on the economic side. Today in a free society, all industrial activity is geared basically to satisfying consumers' demands both quantitatively and qualitatively; and these consumer preferences lead to the broad outlines for technical research. The innovating processes come through the industrial research co-ordination between technicians and economists working in industry, guided by mature and well-educated business men. The gain from this co-ordination is primarily that of time. Through a co-ordinated research programme we are sometimes in a position to advance the introduction of products into the market plan by five, six, and sometimes ten years. May I therefore plead that this process of co-ordination be further extended?

D. G. R. Belshaw, *Makerere University College, Kampala, Uganda*

This excellent paper unfortunately applies only too accurately to the situation on the eastern side of Africa, but it seems to me that two important aspects have not been brought out sufficiently in the discussion. The first is the importance of examining the rather inflexible design of much experimental work in agriculture. We notice from research papers, for example, an undue emphasis on output per acre without specifying which is the scarce resource. We notice the failure to measure complementary inputs; for example, crop experiments where there was not a weed to be seen, whilst no data are presented about the labour or machinery inputs required to achieve that situation. We notice an undue emphasis on mean results with no mention of the variation around the mean. We notice a failure to consider the timing of investment, an aspect which is particularly important in low-income countries where a high discount rate applies. Thus, my first point, as Dr. Bergmann has put it, is that we need more research concerning research.[1] My second point is that I believe the answer to these problems in the long run must lie in the universities, not so much through their research work, as through their training and educational functions. Far too often in under-developed countries we have agricultural chemists, agricultural biologists, and

[1] In *Economie Rurale*, numéro spéciale, 1964, p. 11.

others, who have had no training in farm management, and sometimes not even training in agronomy, crop husbandry or animal husbandry. It is very difficult for these pure scientists to conduct applied research which will be relevant at the farm level. It is here in the long run that we look to universities, especially those in the advanced countries, to increase the role of economics, particularly in farm management studies, and to ensure that this becomes an essential part of the training of any agricultural scientist.

C. P. McMeekan (*in reply*)

May I thank you for treating me relatively leniently? If I had given this address in Australia, I would have been pulled limb from limb. As far as the question of Mr. Riemsdijk is concerned, I agree that much could come from on-farm research. Only time made it impossible to describe how, in our own activities, one of the standard practices has been to ensure that any new development is adequately tested on an on-farm basis before the technique concerned is given the hall-mark of approval by the station.

Dr. Belshaw has drawn attention to the rather simple experimental approach that we have adopted, and has asked the question whether we have not been criticized for this by economists. We have been criticized quite severely at times by both economists and biometricians. We are not without experience however, and have not been happy with results from a multifactorial approach to farm and animal experimental design. Too frequently we have become so bogged down in trying to ask the computers the right questions, that we have not known where we have got after we have finished. Being a very simple fellow imbued with the idea of trying to ask simple questions in a simple way, I have tended to restrict design to two or three or at the most four variables. This has had the advantage of producing results that can be understood and evaluated by farmers, who are not readily prepared to take science merely on trust but who are willing to try when they do understand. The method has taken us a long way along the road of increasing efficiency. Judging by results, it has been far from costly. The time may come, however, when a more sophisticated approach may be desirable. It will be interesting to watch the output of Ruakura over the next twenty years, because under its new leadership, my successor, Dr. Wallace, is giving emphasis to more complex experimental designs in an attempt to extract maximum information from every experiment. He and I have a wager, as to who will have made the greatest net contribution to farm

production over the two twenty-year periods. I hope I live long enough to see the outcome.

I agree with both Mr. Niaz and Miss Malya, as to the general comments on foreign advisers. They are both right. I would suggest, however, that if foreign advisers have failed to orientate themselves to the local scene it is because they have been too specialized in their own upbringing. One of our great lacks today is what I would call, for want of a better name, the 'general practitioner' in agricultural science. Too many of us are specialists who are selected on our reputation as specialists, and who find it very difficult to begin to think of the whole complex of agricultural production as an integrated balanced operation. We have never had to do this in our own environments. Yet we must do it if we are to come up with a workable development programme for the country concerned.

Mr. de Swart emphasized the need for recognition of the individuals in the research game and the difficulty of providing recognition in a scientific sense, once one is tied to a project approach. This is true, but I do not think it is an insuperable difficulty, as my paper indicated. One young man, who received a doctorate from his own university for his Ruakura studies, was a South African sent to us for experience and training. Another example is that one of my greatest problems has been to retain staff in New Zealand. Once they have published their work, either England, the U.S.A., or Australia have sought and usually obtained their services. This is surely recognition of a tangible kind. I agree with Mr. Pilhofer with respect to my omission of the part which can be played by commercial interests. However, I was limited to three thousand words and felt that such firms do not need to be convinced of the necessity for useful objectives in their research. Co-ordination is implicit in their need to make profits. I also agree with him that much work in overseas countries is badly designed. This is surely a matter of training, which I have tried to stress as a factor to which we must give major attention in future.