S. Afr. J. Anim. Sci. 1, 159–164 (1971)

RESEARCH IN SOUTH AFRICA – QUO VADIS?

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In choosing this subject, I was of course very much aware of the title of the introductory paper "Animal Production – Quo Vadis? " by Dr. McDonald. Hence also the "quo vadis". What I did not realize is that I had to address you immediately after a man with such great insights, as became evident when Dr. McDonald spoke to us here this morning. He has in fact touched on many things which I had intended speaking on; so in a sense he has stolen my thunder!

I would like to introduce this subject by very briefly referring to the research pattern as I see it developing in the world. The advanced industrial countries apply a significant part of their gross national product to research and to advanced training of the more gifted portion of their manpower. On the other end of the scale, those countries which are euphemistically called the developing countries, though really undeveloped, have no national economies of the kind from which they can draw funds to devote to research; in their populations they have no significant component of highly trained people able to do research. Their economies are correspondingly poorly developed, for there is, in general, a very close link between the economic development of a country and the amount of research done there.

Between these extremes are the countries in which an industrial economy is taking shape in the hands of indigenously trained manpower, supported in part by local research effort. South Africa is one of these countries. Its development, however, unlike that of countries in Western Europe and the United States, is not based mainly on its own research; it is based mainly on technologies and on information imported from other countries. In other words we are using other people's research to develop our economy. And that is true for most of the countries in the intermediate scale of research dependance.

We have been particularly favoured in this country because we have one major industry, the gold industry, which gave us the means to build up an economy, an impressive economy, far faster than could otherwise have been the case. If we had not discovered the Witwatersrand gold fields, this country would still have had a subsistence economy. So we have been particularly favoured. And as a result of developing a healthy economy we have been able to establish powerful research institutions in this country and to do much impressive research.

As a result of our economic development we are inclined to think of ourselves on an equal footing with countries in Western Europe. It is true that we are the most developed country economically on the continent of Africa: although we only have 4% of the area and 7% of the population of Africa, we have about 60% of the railways, 70 % of the telephones and more industrial development than the rest of Africa. Our per capita income is also the highest of any country in Africa, namely about R500 per capita per annum. But it is still very low compared to those of the advanced industrial countries. The United Kingdom for instance, has a per capita income three times ours. Australia's is also three times higher than ours. Canada is nearly four times and the U.S.A. six times higher. From that point of view we are very much in an intermediate stage of development.

Another relevant feature about South Africa which has a bearing on our research planning, is that we have, on the one hand, sophisticated industries both primary and secondary, that play a large role in our economy. But we also have very large areas in this country where the people have a precarious subsistence economy. So in a sense South Africa is representative of the world – on the one side it has economic features quite like those in the U.S.A.; on the other side it has areas quite like those, for instance, of undeveloped countries in Africa. I think we should take this into account in planning for research for the future.

We have established powerful, effective research organizations in the fields of agriculture and animal husbandry, minerals and metallurgy, general science and industry, atomic energy, in education, in sociology, in medicine and several others. These are impressive insituations, centres of research and of learning, where work of world renown has been and is being done.

Yet we are still only spending about 0,5% of our gross national product on research in this country, compared to 1,7% in France, 2,3% in the U.K., 3,6% in the United States, to give just a few examples. So percentagewise, we are only spending one third to perhaps one seventh of what the advanced industrial countries are spending on research.

Nevertheless, the research institutions which have been so successful have grown impressively and of course their budgets are growing correspondingly. Dr. McDonald has referred to the growth of research budgets and the dangers inherent in that. I would like to stress that too. In fact there are dangers for research institutions in success.

When research institutions achieve success there is a danger that they will coast along on that success. The greater the success the greater the risk that that institution will coast along for some years, even for some decades.

Such successes usually lead to requests for more funds in order to capitalize on the success, a very necessary action. But such successes are generally achieved not by the organization as a whole, but by one or two particularly creative individuals in it. Dr. McDonald referred to the fact that if a scientist does one outstanding piece of work in his professional life as a researcher, he is doing well. I agree. I do not think there are many who achieve more; only some of the really outstanding people achieve multiple breaks-through. But if a man achieves one important breakthrough the whole institution benefits. But in capitalizing on it more staff must be appointed; but as a result of the lack of creative researchers, mediocre staff may be appointed. This dilution of the research staff with people who may have a good science degree but have no research calibre is a further danger. Then research costs go up, equipment demands go up, space requirements go up, but there probably will be no more outstanding successes.

This leads to another danger – disenchantment on the part of the government authorities that have to approve the funds. They observe that more and more money is going into this "bottomless pit" and ask what is coming out of it? Of course the authorities who provide the funds often can have only a limited appreciation of the significance of a research finding. A prime example concerns this fantastic U.S. space programme which has fired the imagination not only of the people of the United States but of the whole world, and has led to a fantastic pouring out of funds into ever widening programmes. Man now wants to explore the planets beyond Mars, we want to land on Mars, and we talk about space exploration as though we have opened the frontiers to the stars. But the objectives of this programme as far as Congress is concerned have no longer the political expediency on which the programme was launched, and now this bubble has collapsed. These tremendous organizations of scientists and engineers that have been built up with truly fantastic achievements, have begun the process of dissolution. That is an extreme example but it is one which we must keep in mind in a country like South Africa with its limited means and also its limited capacity to appreciate what science and technology can and in fact will have to do for its development.

As the demand for research funds grows so also will the tendency increase for those in government service, who are not concerned with doing research but with its financing, to exert control over how it is spent. The result is that you get a civil service type of control imposed on research organizations which are not amenable to that kind of control.

Furthermore, you get civil service type of conditions of service imposed on scientists, which does not promote scientific creativity. In the case of administrative functions of the State, one deals with routine functions, administrative and regulative functions involving the very great responsibility of carrying out the legislative work of Parliament and the decisions of the Executive, very important functions indeed.

In carrying it out, the administrative staff have to conform to certain rules, with little or no discretion. In general the need is for the kind of people who accept the existing, the laid down ways of doing things, and who are not looking for new ways of doing things.

This is in marked contrast to what is expected of researchers. There are no set ways and the only rules are scientific integrity and intellectual honesty. A research man is one who challenges what we know, how we see things and how we do them. The very word "research" is a guide. People have searched before but now you research - you look at it again and you challenge what is accepted as known, and establish if it is in fact so, or whether there are new approaches, better ways, greater insights. You go from the sphere of existing knowledge to beyond the horizon. This requires not only a highly trained person but one who has a flexible and creative mind.

These kind of people cannot be fitted effectively into the civil service type of conditions of service – you need far greater flexibility – different kinds of incentives and you have to take into account that scientists are people who are not only in great demand within the country but also internationally, particularly those that establish a name for themselves. This factor plays no role in civil service conditions. We in South Africa are suffering from the imposition of civil service type conditions in our large research establishments.

There is another danger in the growth of research establishments, viz., that more and more research staff get involved in non-research matters. This is particularly so if administration dominates research instead of being subservient to it.

Dr. Vanevar Bush, one of America's great scientists, once said that when there is a choice between doing research and doing something else, the tendency is very strong to do something else. When a research worker has the choice of sitting in his office and working on his in-basket or going into the laboratory and doing research work, the temptation is to look at the papers; they are the pressing though frequently unimportant things; the research is important but may well not be pressing. The result is no progress with the research.

Now, what have we done about research and research establishments in this country. We have done much to bring specific scientific disciplines together under one roof. In other cases we have brought together scientists to work in a particular defined field. So we have devoted much time to the organizational structure of research establishments in South Africa and much of it was necessary.

However, we have not done enough to promote inter-disciplinary co-operation either on a routine or on an ad hoc basis. Yet this is where a great part of the strength of modern science lies.

We have done very little about defining long term national objectives for science. It is to this I think that we should devote energetic attention. We should worry less about organization and, particularly, avoid making arbitrary changes in organization or in the location of research facilities as this will lead to disillusionment and frustration on the part of the research staff. In research, clearly defined objectives for the individual research worker and for the institution, freedom from unnecessary administrative and organizational rigidities, and, above all, inspired leadership, are basic requirements. We should define long term and medium term national targets for research. Short-term research has its place, particularly in industry. But in national research establishments it should be limited;we should encourage industry or other interested parties to do this work.

We have many problems in this country, and many fields in which research can play a role. Research in the hands of able men is a powerful tool, the most powerful tool known to modern society for solving some problems. It can even be a useful adjunct for helping to solve non-technical problems.

In our research establishments the emphasis should be on calibre of research staff; do not appoint a mediocre man, but rather wait till the right man comes along. We must exert strong internal discipline in relation to the demands for funds as the growth in research expenditure must in time relate to the growth in the national economy. Dr. McDonald mentioned that if we extrapolate the rate of growth of expenditure on research and on science in the western world to the year 2000, the gross national income in many countries would be completely absorbed by research. Some time there must be a levelling off. If the pruning is done externally it can be so much more painful and frustrating.

It is easy to start a research project, but it is very difficult to bring it to an end. The great danger is that research is done, something worthwhile is achieved but the work is not rounded off; instead it carries on, more assistants, more equipment, more money, but it could have been predicted that little more of value is likely to come out of it; the law of diminishing returns comes into play. The decision to stop the work at the right stage is one of the most difficult and one of the most important decisions of research management.

Now where should we set our horizons? What kind of problems can we look at? What problems should we set as targets? I don't know this for your field because I don't know much about it. You have many brilliant and competent people to define your long term objectives. However it does seem to me that when we take into account agriculture and animal husbandry by our less trained Bantu people in large areas with a subsistence-economy, there is scope for much research, application and development. When your President, Dr. Bonsma, and I attended the United Nations Conference on the development of developing (sic) countries, the most striking single fact that became evident, was that the information is often available (Dr. McDonald also referred to this), but there are not the people in these countries trained to absorb, understand and apply it. This is therefore one of the objectives that we could set ourselves. It was not my intention to try to define long-term objectives for research, but rather to point to the need for such definition.

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We have powerful research establishments that can have a profound impact on this country's development, as has been demonstrated impressively in a number of fields. Their tasks would be made easier and their relative costs would be lower if they were given the autonomy they need and deserve as responsible institutions. There is scope for greater and freer co-operation between different research institutions. We should also create conditions in these research establishments that will lead to inspiration of research people, and will allow selection of the best personnel, not only from this country, but from overseas.

There is need for a greater awareness in government circles at all levels, in industry and with the public, of the important role research can play in our future socio-economic development, in maintaining a healthy balance of payments, in security and in defence.

Discussion

The lack of co-ordination and co-operation are probably our main factors in modern agricultural research. What are Dr. Stutterheim's views on the feasibility of a National Agricultural Research Centre or Organization or alternatively a National Agricultural Research Council, primarily responsible for co-ordinating research and giving directives on research to be undertaken?

It might be quite a good idea, but I don't really know enough about the agricultural field to express a definite opinion. However, I believe that when we talk about co-operation in research, the most effective means of getting it is to get the research people themselves together, and not to try and organize it from above. By all means create conditions at the top which will in no sense interfere, but rather promote direct personal contact between research workers in different research establishments to get together and to discuss their problems. One finds then that there is enthusiasm for co-operation, but if it is forced from above there is no enthusiasm. The most effective way of ensuring co-operation in research, is to open all channels, to allow research workers, even at the lowest level, the most junior, to go and talk to other people whom they think may be able to help.

Who is going to dictate to the junior scientist when he must stop carrying on with research? You have emphasized this need for long-term research. Should there be a body organizing this research, because if the senior man does it they call him a dictator, and where do you draw the line?

We need some dictators in research work! However, my remarks, about starting something and not knowing when to stop, apply not so much to basic research. In basic research, there is almost no limit to the programme. Assuming that something fruitful, in the sense that new information is coming out of the basic research, then there need be no stopping. But when we come to applied research and development, this is a field where we in South Africa are not going far enough. While discussing Dr. McDonald's paper it was mentioned that basic research is expensive, it is. Applied research is much more expensive and development even more so. The ratios are something like 1:10:100 in cost and therefore we tend to say, allright we will do basic research. Basic research I may add, is easier than applied research because it does not have the additional lack of freedom, which economy dictates. Therefore, you may find that when you start doing applied research, you may get on a very beautiful line but if you have that economist, as Dr. McDonald so rightly pointed out in the team, he may say "no". Technically something may be achieved, but it will not pay, so you have to stop. Hence there is a need for looking and re-examining what we do in the applied field to ensure that what we are trying to achieve does not in fact bring far lower benefits than the costs of the work that we are doing. This is a great danger. One can do research very readily costing hundreds, even thousands of rand on a problem which, if solved, will bring you in ten-thousand. This research is not economical. It is not the research workers themselves, but the other people who examine it critically, who will see this, which is a very necessary thing.

An important aspect has been raised, namely, long-term research and who should do the directing? I think this should be done by senior men, and is generally team work. It is so important, that I would like to mention one field in which I feel we should work, and that is in establishing ways of promoting exports from this country. Now you may say, what's that got to do with research? Recently an analysis was done of the American economy which they divided into nineteen sectors. They analysed what role is played internally and in exports, and they also looked at the research pattern. Of the nineteen sectors, five were responsible for 89% of the exports and they were responsible for 82% of research – there was a connection. We in South Africa are living in a sense, in a false paradise because we have got gold, we have had gold in the past, we have gold now, it is paying for our imports. However, gold is a vanishing asset. By the year 2 000 it will not contribute 2% to our foreign exchange although at present it contributes 40%. What is going to take its place? This is an expanding economy and I am discussing it because our economic planners are saying that our economy must double or rather that our standard of living must double by the year 2 000. According to the latest census, our population will be double what it is now and will be 40 million by the year 2 000. So a doubled standard of living for twice the number of people means an economy that has grown four times. You can imagine what our import bill will be! Moreover, there will be no gold to pay for that import bill. What then is going to pay for it? There must be more exports of primary and secondary products, other than gold, which means that the agricultural industry, and secondary industry will have to provide the exports.

What can we in South Africa produce that we can export? This is a very big question and it depends on research, as America has proved. Because for products to have a market value in the year 2 000, they will have to be new products, for the greater part unknown today. If we don't produce new products, then nobody is going to buy our products, because, other people will be producing new products, based on research. So our research in this country, in the agricultural field, animal husbandry field, and the secondary industry field, outside agriculture, will have to devote a tremendous amount of effort, now, *soon,* in order to be able to produce products which we can sell so that we can earn foreign exchange to pay for the goods that we want to import. These are the things which I think our research establishments should devote time to.

What are the pros and cons on research for and in the underdeveloped areas?

The problem of doing research in under-developed areas, whether they are within South Africa's borders or outside them, is immaterial, but not the question who is going to pay for it, and who is going to do it? They cannot pay for it because they haven't the means, so somebody from outside has to provide the means. They also have not got the kind of trained people who can carry out research. So research done in underdeveloped countries, indigenously financed and indigenously manned is impossible. There is no such thing! It cannot be done! Doing it for them is to my mind a socio-economic responsibility for the world as a whole, on the advanced nations and, within our borders, on us. That is, on those that have the means, both financially and intellectually at their disposal to devote attention to their problems. We should provide more of this aid. We are not now doing very much in the way of assisting them, but we are doing a lot to assist ourselves. We are not devoting much attention to the problems of our own under-developed areas, although we should. The problems need to be defined in terms of where factual information can help us to find solutions. This I don't think has yet been done to any great extent. There are a lot of opinions expressed about what we should do for them. Many of them are of a political character, and that I don't want to go into, but we do require facts and this is the aspect that is often resisted. Again, Dr. McDonald, referred to the resistance to research. People object to research, which means they object to facts. They object to hearing what is in fact the case. This reminds me a bit of the very devout Hindu who was a vegetarian – he had to be. When people spoke to him he said: "I cannot take life – hence animal life is absolutely taboo". Then one day somebody showed him the water that came out of the stream that he drank from under a microscope, and he saw little micro-organisms swimming around. What was his reaction? He smashed the microscope! Now this is the kind of reaction which I think one gets. They don't want to hear the facts.

What is the right atmosphere for research? We hear of a lot of talented people leaving South Africa, but of very few talented young scientists coming here. What are Dr. Stutterheim's views on this, can he perhaps suggest a method of attracting people of talent and recognizing talent at our research institutions and promoting that talent?

This is a very, very important question. I think that that there are two things that attract able scientists from one place to another. The most important is inspired leadership. If we have that, then I think that we will also attract people. But when we start thinking of attracting people from overseas we run into another problem. We of course, do attract a certain number of people from overseas but we have this other problem, that when you start equating the income of the scientists here in terms of overseas salary scales, even in their government organizations, they come to the conclusion that our scientists are very poorly paid. We all agree. They say therefore, although I would like to come and it appeals to me, I have a wife and children to think of. However, I do believe that our standard of living, in terms of incomes here is somewhat different, when it comes down to the point of the buying power of our money and the buying power of other people's money. If we for instance compared the income of an American scientist and the income of a South African scientist in dollars and rands in terms of buying power, you would then find that what the Europeans say is true. In other words, that the dollar is heavily over-valued, and that you could get away with a much lower dollar equivalent in South Africa than you could in America. To put it very plainly, I think that a salary in South Africa of R10,000 would be the equivalent in America of about R15,000. These are all factors that play a role but we could draw more people if we tried to recruit them. We are not doing this. Here and there it is done but actively we are not trying to recruit very many scientists overseas. If we did we may find it easier than we imagine particularly under the conditions as they are now where there is a certain measure of disillusionment in overseas government circles about the tremendous amount of money they have to spend on research. We have not yet reached that stage, although we are beginning to reach danger level.

Overseas countries have a big advantage over us because they have a frequent interchange of students at their universities, to and from other countries. For example, take the United States. If you have an outstanding student who makes wonderful progress at postgraduate level, they are certainly going to corner him and keep him in that country. Now if we could have a greater interchange of students from other countries at our Universities we could also retain the best material. What are the ways and means of selecting a good scientist?

Once a research worker has performed, once he has been working, and you see the calibre of the man, the quality of product in his publications, his reports and so on, then one can assess a scientist. However, if he is straight from the University then you have no particular way. Some people say that you must only pick those that do best at University, that is those who obtain the highest marks. I personally don't agree with that. I do not think that academic achievement in itself is a measure of research capability. There are some students who achieve fantastic results at university but who are no use in research at all. There are some people who battled through university who are exceptionally good. And here we deal with this rare quality of creativity, which is not related to intelect. People who are creative are generally difficult people. They look at things in a different way to other people, that is why they are creative. Very often research workers are creative people, and those at school are the ones that annoy the teacher and so they don't do well. They are very often exceptionally good research workers. But you are dealing here with the upper-levels of the intellect, which are the most difficult to assess and this can only really be done afterwards, which is a problem.

What are Dr. Stutterheim's views on specialization in training? I get the impression that we might be rather overstressing the training of analysts, people able to pull apart and analyze, perhaps rather too much at the expense of the man able to put together, to synthesize, and to see the horizons, who will in later years have the necessary perspective to do the planning. It seems to me that we stand at the moment in our training in a great danger of suffering from a lack of people able to see disciplines in relation to each other which is then reflected in our obvious lack of team-work at the moment and perhaps the narrowness of the specialist who dominates the research scene.

I don't know whether I can cover that whole field. I think you have touched on a range of very important issues. I would like to comment particularly on the one about analysis and synthesis. I agree with you that our training in the scientific field tends very much towards the analytical. This is in a sense the easier part of scientific and technical work. Characteristically, the scientist is analytical. But there is another group of scientists, if you are prepared to accept them in the ranks of scientists and these are engineers. Engineers take, for instance, knowledge: what they can find and what they can't they add to by what is known as engineering intuition. In other words they bridge. They have got to do a job, but all the facts are not always available. This does not mean that they can say "well I have not got all the facts, I am not going to do the job, I am not going to build the bridge, I am not going to put up this refinery, I am not going to put up this power station, I have not got all the facts." An engineer cannot do this. He can say "well I have some of the facts and then my judgement, my intuition, my experience, call it what you will, is going to bridge the gap and I am going to put together all this and I am going to create this structure that we need". That kind of training is most important and it should really be given to all scientifically trained people. So that having taken something apart they can then start putting the pieces together again and get out of it something of greater use, greater social use. That is as far as I would react to your question.

The last was a very, very important question, which should be dealt with in a little further detail than it has been done, because it is so closely related to the major issue, which rose as a culmination out of the whole of Dr. McDonald's address. That is related again to this question of integration. It is also related to the question asked about the selection of scientists. Wouldn't Dr. Stutterheim say that the whole of our traditional type of school and university education, by its emphasis on examinations which depend on analytical ability, is tending to select the wrong kind of people for these integrated functions, and that is possibly an explanation why it's not the brilliant people that tend to succeed in industry if one's to go by Prof. Boloney's analysis?

The second point is perhaps a bit unfair, because it is really a question for Dr. McDonald: It has now come out quite clearly where the great deficiency lies: not in the analyses or the breakdown. Research is of the highest order but is it building these into working systems to make an agriculture for the future in this country? Dr. McDonald went so far as to say that what is required is technique and the science of agricultural management. The small farmer has had his day unless he can get vertically integrated. In order to produce the food we are going to require, we are going to need something, we are going to need the synthesis from the soil upwards to the product through something which is infinitely more difficult than any engineering problem, because when an engineer builds a bridge he knows that his foundations are solid or, if not, he makes them solid. And then he builds the column and when he puts the beam across, he knows it is likely to stay there. When you are dealing with lactating cows or young things which you are trying to encourage to breathe at an earlier age and you want them both to have a calf at foot and to continue to grow, and to get into calf again you are dealing with some of the inner mysteries of the female creature and this is a far more difficult thing than engineering. That is why I would like to ask whether you do not think that in regard to the future organization of agriculture it is a role that perhaps the Society should undertake since nowhere are these concepts added together to produce practical working systems on a large scale.

I think we are now entering a field of which I have limited knowledge and experience. So I am on dangerous ground. This is the field of education and training of the mind. I am inclined to agree that this is so, that our methods of teaching tend to stress the analytical and that therefore we are not necessarily creating the kind of citizens that we need. And this is a problem that educational research should look at. I do believe that once you come to the levels of higher education – and this is at the senior graduate and at the post-graduate level – that this becomes less true. At those levels there is scope for doing both the analytical and the synthetic. Both the breaking up into elements and understanding them, and then putting elements together again to create systems. So there is some provision for that in our educational system, but it may well be true that it is limited. On the second topic I am afraid I can't express any views. I think that the speaker has presented a very interesting question but I think there are other people in the audience who should answer it, and not me.

Is there some internal inconsistency in the arguments. The doubts arise from some remarks previously made which are very near to the heart of everyone here. It was that "no research can in the long run be successful unless it does not provide a large field of basic research, which has in fact no practical objectives at all. It is through basic research alone that new and unexpected results can be obtained." The question is really, if you are going to start setting objectives and directing research, isn't this in its nature inhibitory of the freedom of basic research?

The original author of that statement is not from my organization – he is in fact a chief scientist in Germany. He is the president of the Max Planck Gesellschaft. He said this when he opened one of our laboratories. What he was referring to is that if you start on applied research you may very soon find that you have run out of information of a basic character. And that applied research will therefore stimulate basic research. I think that was the main thing and I don't think it was an inconsistency. In fact I would say that this is a marvellous way of starting basic research. You very often tackle a project thinking that you will be able to see it through. The deeper you get into it, the more fundamental you tend to become. So applied research can be a very great stimulus for basic research. You may find that it is beyond your own capacity and that you must start calling other people in. So much the better, form a team!