IMPLICATIONS FOR RESEARCH PROGRAMS OF AGRICULTURAL EXPERIMENT STATIONS

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Information presented in the background papers emphasizes that major changes have already occurred in the economy of the commercial farm firm and that changes will continue to occur in the future, probably at an even more rapid rate than in the past.

My assignment is to consider the implication of past and future change in the commercial farm firm on research program of the agricultural experiment stations.

Need for Change

The agricultural experiment stations grew out of a recognition that farmers could not do their own research.

In the earlier years of a young and growing nation, the urgent need was to produce food and fiber for the people.

The state stations and the U.S. Department of Agriculture responded to this need by directing most of the research effort to programs concerned with increasing agriculture's capacity to produce.

New technology, a product of research and development, has and will continue to play a vital role in the economic growth of our country.

It also has made possible a modern, efficient agriculture that stands as a tremendous resource to support hot or cold wars, to meet foreign policy needs, to assure continued economic growth, to supply our expanding population with more and better goods and services, and to keep us competitive in today's common markets and in tomorrow's free world markets.

New technology gives rise to complex economic and social problems because increased output is not immediately "digested" into the national economy.

Burdensome surpluses have been cited as evidence that we have too much research, too much technology, and too much efficiency.

Some say we should slow down or stop production research and limit the use of new technology. But how much is really enough?

Research can't be turned off and on like a water faucet.

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Many problems of safe, efficient use of our natural resources are yet unsolved and won't be solved unless we continue and expand production research.

Reversion to a "policy of inefficiency" will stagnate economic growth and create more economic and social problems than it will solve. To kill the "goose that laid the golden egg" is not the right answer.

But we do need to expand research to help rural and urban communities adjust to the complex economic and social problems that arise from cost-reducing and output-increasing production research.

We need more research to provide information that will help:

1. Facilitate the "digestion" of potential gains into general economic or industrial systems.

2. Lessen the short-run problems created by increased output.

3. Bring returns in agriculture to par with other industries.

4. Insure that gains from technology are realized quickly and fully by farm and city people.

5. Bring about a more efficient allocation of resources between the agricultural and non-agricultural sector of the economy.

6. Result in more efficient use of natural and human resources of agriculture in relation to demands for products.

7. Lead directly to policies or developments which improve the short-run income position of agriculture and restore long-run structural balance, in terms of resource returns, between agriculture and other industries.

8. Promote directly the improvement of marketing systems and the economic and social mechanisms through which farmers exchange products and resources.

9. Allow specification of more efficient or desired uses of natural or human resources of agriculture relative to the present demand for products and prospective needs of a progressive economy.

10. Result in increased demand for farm products through chemical, physical, biological, economic or other research which:

   a. Improves the quality, form or grade of agricultural products.
   b. Creates improved strains or kinds of products.
   c. Creates new industrial or other uses of existing products, or
   d. Expands outlets for existing or new agricultural products in foreign markets or through improved human nutrition and greater capital consumption in this country.
Obviously, this is not a complete list of all the needs. Assuming that these are significant problems, what is the total job that needs to be done? And what are the steps that should be taken to assure that the state stations and the Department of Agriculture, individually and collectively, develop programs commensurate with the needs.

As we focus the spotlight on the stations' program and consider the changes needed to best serve the people, we need to be conscious of the stations' "public image" and the suggestions that are made to improve its usefulness.

Debate goes on as to whether the research programs of the state agricultural experiment stations are sensitive and respond adequately to current and future needs.

Are the stations really concerned and doing research that best serves the needs of the people?

Have we identified and obtained agreement on the most significant problems which should demand our efforts?

Are research resources being focused on areas of top priority?

Some say the research engine is "running wild" churning out technical progress faster than is needed or can be absorbed by the agricultural industry, the rural population, the nation, and the world.

Should less emphasis be placed on "production research" and more emphasis on research to develop new ways and means of adapting or adjusting rural America economically and socially to technological progress and economic development?

Should we be devoting more time to national and world affairs in which we find ourselves?

Others believe that America is fast using up its fund of knowledge on how to expand production, that we must redouble our efforts to prevent food shortages in 10 years, or to provide reserves and the potential to produce in the event of world crisis or prolonged drought.

These conflicting viewpoints are partly semantic, partly factual, and partly honest differences in evaluation.

Reconciliation of these differences and the development of a comprehensive cooperative, and coordinated program for agricultural research is a first order of business.
There is general agreement that the station’s research programs have changed in the past and that they must continue to change in the future if the stations are to serve the role for which they were founded.

What are some of the factors that limit, impede, or facilitate the changes that are needed.

Traditionally, great autonomy is reserved to the individual departments and to the individual workers.

There is no question but that there is competition for resources between departments and between individual staff members within a department.

This is good. But when urgent problems arise it is difficult to re-allocate resources among departments or between individuals. There is also a tendency to continue low priority work for fear that support may be lost.

The departmental structure system tends to discourage an interdisciplinary team approach so essential for effective solution of many of today's complex problems. But this really may not be serious, because experience has shown that most staff members recognize and are convinced of the needs and merits of teaming up with scientists from other disciplines for effective solution of many of today's complex problems.

Generally, the most effective interdisciplinary work is done on a voluntary, informal basis.

If staff members want to work together they will, and if they don't nothing much can be done.

Administrators can do much to make this important activity more meaningful and productive with simple fiscal and reporting procedures.

If a staff with the necessary training and qualification is not available to undertake work of the type identified as urgent, change must wait until resignation makes replacement possible unless additional resources are available. Staff tenure must be respected.

But normal turnover in staff permits recruitment of personnel qualified to do a particular job.

Resistance may be encountered if the changes require moving resources from one department to another or even between individual staff members within a department. But such moves must be made if needed changes are to be made, and they become routine action if provided for in the goals and objectives of a long-time research plan.
Where sizeable investments have already gone into ongoing work and there is tangible evidence of a potential pay-off, the work probably should be completed before initiating new work of higher priority. This type of delay need be only temporary and not a serious deterrent to reaching long-time goals, unless the time is extended beyond the designated closing date. There is a tendency for this to happen, and it takes firm action to prevent it.

Resistance by pressure groups too often delays or prevents change. This can't always be avoided, but will be less of a problem if steps are taken to keep groups informed about the goals and objectives of the total program.

A Comprehensive Plan

Planning the development and coordination of research programs has been an important activity of the state agricultural experiment stations and the Department of Agriculture from the very beginning of their existence, more than 100 years ago.

It has been done many ways, many times, at many places, at many organization levels, by many groups, and for many purposes.

In scope it has involved parts or all of a station department, areas of work or the entire station program, and areas of work on a regional or national basis.

It has been done by individual states and by divisions of the Department of Agriculture working independently. It has been a state-federal cooperative effort.

It has been prompted by the need for program evaluation and development, for information to support requests to legislative groups for funds, administrators at various levels, and for many other reasons.

In total, considerable effort has gone into program planning, development, and coordination and has played an important role in helping to make the agricultural research program the success it is.

But much of the planning has been haphazard and on a piecemeal basis. It is not surprising that planning has been this way since the stations and the Department of Agriculture are legal entities unto themselves, with specific responsibilities and some limitation on use of funds and methods of budgeting.

The size and complexity of the agricultural research program has grown and today there is an urgent need for a comprehensive plan for agricultural research. It is a difficult task, but it can and must be done if we are to assure that available state, federal, and industry resources are used most effectively in providing information for the solution of urgent problems now and in the future.
Probably the greatest weakness in the past has been failure to

1. Identify the most urgent problems
2. Establish goals and priorities
3. Establish responsibilities for different phases of the program
4. Provide and organize arrangements and mechanisms for comprehensive and coordinated planning
5. Establish administrative and fiscal arrangements to implement action to achieve the goals

Each state station needs to develop a comprehensive plan to assure that it is meeting the needs of the people within its own state. The Department of Agriculture needs to do the same thing.

Separate plans cannot be made on a realistic basis without full knowledge of what is being done and what is planned in other state, federal, and industry programs. There must be provision for over-all comprehensive planning, for cooperation on programs, and for coordination.

Sound planning must be based on the best possible estimates of trends and population growth, economic activity, technology, yield, imports and exports, and the requirement of all the various uses competing for these resources.

More and more research is being done by private industry. Such effort is directed primarily to producing and selling a particular product at a profit. Some of the research that experiment stations have done in the past will more and more be done by private industry.

This frees public funds for research in areas of importance to society but which do not attract support from the private sector.

For example, support for research on problems such as plant-soil-water relationships, land use, conservation, water utilization, and economic and social problems depends on public funds.

A comprehensive plan must recognize the state, regional, and national aspects of the problem and provide for a cooperative, coordinated approach that will assure the most effective use of manpower and facilities.

We need to evaluate the competitive position in particular areas for producing crop and livestock products to determine the comparative advantages among regions.

We need to appraise, evaluate, and develop ways to open up the markets throughout the world. Such a development offers a partial solution to surplus problems at home and also help to relieve hunger and poverty in underdeveloped countries abroad.

We need to appraise our staff, our facilities, and the needs, and decide on the areas in which we should build strength.
We need to develop a research environment that will bring together the most competent, scientific talent, financial resources, and research equipment on a problem or a program basis.

Agricultural experiment stations are confronted today with problems of broad regional and national scope that require talents and facilities beyond the means of any one station.

In what areas of research should stations attempt to build strength?

A team approach that includes scientists from many disciplines is becoming increasingly important for the effective solution of today's complex problems.

To help assure that the best minds are focused on the problems and to avoid unnecessary duplication of effort and facilities, the establishment of "technical research committees" is needed to review, project, and plan programs necessary to solve the major problems of the future.

The quality and quantity of a research program are influenced in a large measure by the characteristics of the scientist, the problems on which he bases his project, and the environment in which he works.

A research worker must be willing to acknowledge his basic obligation to put the public interest ahead of partisan or personal interest.

A research worker must be alert to new developments and new needs. He will see a problem in his specialized area before it becomes obvious to the general public and make an appraisal of its potential importance.

The ability to choose wisely the area of investigation which offers largest promise of solving important problem is one of the most valuable attributes a research worker can possess.

What type of scientists and how many will we need? Well trained, imaginative and visionary scientists will be required to cope with the complex problems of the future.

Have our graduate training programs been updated to help do this?

New knowledge is being developed so fast that someone has said the "half-life" of a scientist is less than 10 years. Positive steps need to be taken that will provide greater opportunity and encouragement for scientists to "re-educate" themselves to keep abreast of the changing times.

There is a continuing need to develop better ways to measure the amount and kind of support available in terms of the quality of research achieved.
Each research project should be evaluated as to its potential contribution to the solution of present and future problems.

We need to estimate the nature of the agricultural industry during the next 10 to 20 years.

We need to estimate what agricultural production will be in various sections of the state and the country.

What will be the crucial problems? What priorities must be established to assure new information most likely to be required in the future?

We must determine the long-range objectives as the means of defining needed change and for building strong research programs.

Program evaluation serves as a basis for identifying the nature of programs and determining the needs and opportunities for improvement.

In recent years important steps have been taken to provide for over-all, long-range planning and coordination. At best, progress will be slow, but we need to keep at it.

No longer can we afford the luxury of not getting the greatest mileage possible from manpower and funds expended for research on the most urgent problems.

**Research Classification**

In recent years the state experiment stations have been sharply criticized for too much emphasis on "production research" and too little emphasis on "utilization, marketing, and economic and social research."

The term "production research" is firmly associated with departments whose work in earlier years was primarily oriented to improving productivity of land, plants, and animals.

Spurred on by consumer demand for a wider variety of better quality, wholesome, convenience foods, research in "production" departments has shifted more and more to studies on quality improvement of products, processing, and utilization.

For example, one of the major objectives of a North Central regional project is to learn how to influence, change, and improve the quality of beef carcasses. It is expected that results from these studies will establish basic genetic principles that beef cattle breeders and feeders can use to make changes that may be required as consumer demands change.
The dramatic change within the last five years from "fatty" to 'meatty' type marketed swine carcasses was possible largely because information was available from years of swine breeding and nutrition research.

To continue to classify studies such as these under "production research" as is commonly done, is not descriptive of what really is being done. This practice leads to public misunderstanding, and results in unjustified criticism and inadequate financial support for research to furnish information essential for continued economic growth.

This situation serves to emphasize the need for research classifications that are logical, descriptive, and in terms that are readily understood by everyone. Grouping of research activity into a few broad areas of work or programs generally has not been done. There is no single, best classification to serve all needs, but there is a growing recognition that this needs to be done.

Confusion will develop if many different groupings are used. Therefore, there should be a cooperative and coordinated effort to develop a uniform system that will be used by all groups - research, teaching, and extension.

Grouping research into areas of work or programs does not eliminate the need for individual project outlines, regional projects or line projects in the Department of Agriculture. They serve well the purpose for which they were intended and should be continued. There is wide difference of opinion on how projects should be organized. Some believe it best to have a few "Mother Hubbard" projects that allow leeway to go in any direction desired by the scientist or as suggested by the research results obtained. Others feel that projects should be narrow and specific. Some place between these extremes should be the goal. In general, there now are too many "fragmented" and "piece-meal" projects. Fewer, well-defined, well-organized, and adequately-financed projects will help make research more efficient and meaningful.

Research Mix

There is wide difference of opinion and no general agreement on "what should be the research mix among the various sciences and how should they be related."

The large number of areas requiring research makes it necessary to establish priorities and limit the use of scientific talent and resources to only as many programs as can be adequately supported with available resources.

Can we identify the most important problems using methods now available?

If present methods are inadequate can we develop new methods or guidelines that will help to determine the research mix that will produce the greatest benefits?
In recent years the social scientists have developed sophisticated techniques, methodology and procedures now used extensively by industry and others as an aid in decision making.

Modern computer science has opened up many possibilities not feasible previously.

In the physical sciences, and to a less extent in the biological sciences, response to a given set of conditions can be controlled and results predicted with reasonable accuracy.

Economic and social reactions depend on many uncontrolled factors making predictable results difficult and less accurate, given our present state of knowledge.

The problem of identifying the most significant problems is a difficult one, but I see no reason to believe that is isn't possible to improve on the "hit and miss" methods now being used. Some research has been done on this problem with encouraging results. More needs to be done.

To argue the merits of basic and applied research and the proper balance between them is academic and serves no useful purpose.

Both basic and applied research are required for the effective solution of most problems.

How much of each is needed can't be predetermined because it depends on the problem to be solved.

In general, we need to expand and strengthen basic research in order to build up a backlog of knowledge, to improve future technology, to retain highly competent staff, and to assure high quality training of future scientists and engineers.

Pressure for Research

Suggestions, requests, and pressure for research to solve a wide variety of problems come to the stations from many sources. Occasionally the pressure is for work on an entirely new problem, but more often it is for more effort on a particular problem, on an area of work, or for a shift in emphasis in the type of research being done.

Experiment stations have responded to pressures in the past, and they must continue to respond to pressures in the future if they are to serve the purpose for which they were established and if they expect public support for work that they are uniquely qualified to do.
The important consideration is: How can the stations make best use of the suggestions regardless of source to strengthen the present program to serve the people better?

Too often the experiment station responds to pressures for research on an individual basis. This is more likely to happen when an articulated long-range program plan is not available.

The net effect of responding to individual requests has been that limited resources have been spread over so many areas of work that there aren't enough resources available to adequately support work on the most significant problems.

The stations should not simply be reacting late to overripe needs with stop-gap information. They should be diagnosing the symptoms not yet even understood by the public and starting processes in motion to provide basic and applied information for solution.

Scientists should have insights into the needs well before the public recognizes or articulates them.

There never has been and probably never will be resources to support research to work on all of the problems. It isn't necessary or even desirable for a particular state to spread its limited resources over too wide a range of problems since there are other state, federal, and industry groups to help share the load.

Furthermore, the amount of pressure generated for research on a particular problem is not necessarily related to the needs for research that would bring the greatest benefit to all of society, or to even an important part of it.

How then do we avoid the pitfall of committing limited resources to work on lower priority problems?

I believe there is no sure way of even coming close to allocating the resources necessary to solve the most important problems until there is a well-planned, nationwide program.

This will mean putting low priority problems on the shelf, at least temporarily, a decrease in the effort on some work, and actually phasing out of work on the low priority problems. This is not easy to do, but it must be done to free resources for work on as many of the important problems as possible.

Unless and until we develop a comprehensive, long-time plan for developing research programs to meet the needs, it will not be possible to gain public understanding, acceptance, and public support.

Research is time consuming and expensive. It can't be turned on and off like a water faucet without serious loss of time and money.
Many of today's complex problems can be solved best by a team of highly trained specialists, working together in adequate facilities equipped with special types of equipment that often are quite expensive.

Research on which the results have regional or national application should be limited to a few well-supported locations, thus freeing resources for other important work.

Depending upon the nature of the problem to be studied, some research centers will be small while others will be large. Essential to their success is joint planning, beginning at the initial stages and including both state and federal representation by both scientists and administrators.

To assure that the information from these research centers will be put to use as soon as possible in the solution of applied problems, close working relations must be maintained with workers in the areas where the results are to be used.

Too often in the past, research centers became islands unto themselves mainly because there was not enough time and effort put into developing plans that provided for all phases of work necessary to assure maximum benefits.

Beyond the initial planning and development stages there must be joint responsibility by the cooperating groups at the technical and administrative level for maximum results.

For effective solution of many types of problems satellite locations will be required. These should be provided for in the initial comprehensive plan.

Programs that include several persons at different locations should have a research coordinator responsible for coordinating and integrating all technical work in the program and for liaison with administrators to assure that budgetary and similar needs are considered.

Pressures for research will continue to exceed resources available. We can best meet the important needs and in doing so return the greatest benefits if a comprehensive plan is available to guide us.

Pay-off for Research

The answer to the question, "What is the relative pay-off for various research activities?" is very important in evaluating current programs and determining future programs.

Different methods have been used to estimate the pay-off from research. A common one is the cost-benefit analysis using yield increases, greater efficiency, quality of products, or similar values to measure the benefits.
Evaluation using results from past experience can be done with reasonable accuracy when there are tangible benefits that can be measured.

But for most research it is not possible to project the end results or the benefits, with any degree of accuracy, at the time the research is initiated. If it could be done, in most cases there would be little reason for doing the research.

Sometimes benefits far exceed expectations. Other times the results have little or no immediate application. In still other cases the results are negative.

For example, negative results may be as valuable or more valuable than positive results if they show us things that won't work or suggest additional research which provides information necessary to solve the problem. Can the benefits of results of this type be evaluated?

An example of a problem in Wisconsin illustrates how impossible it is to visualize the potential benefits from research.

Several years ago cows grazing sweet clover pastures were dying from internal bleeding. Scientists at the university extracted and identified coumarin, the compound responsible for losses. Plant breeders developed a coumarin-free sweet clover that could be eaten without harm to cattle.

But by this time alfalfa had replaced sweet clover in the forage mixture. An evaluation of the research at this time and point would have shown that the problem had been solved, not directly by the results of the original research, but indirectly by technologies that grew out of a related research effort.

The conclusion might have been that there were no benefits from the original research. A more reasonable and likely conclusion was that a definite indirect benefit resulted from the research as originally proposed.

But this is only part of the story. The anticoagulating properties of coumarin became the essential ingredient in a product effective in controlling rats. The Wisconsin Research Foundation used the income from this development to support other research projects and graduate assistants. How would you calculate the benefits at this stage?

The biggest pay-off of all was the successful treatment of heart patients with a compound made possible by the original research, which demonstrated the anticoagulating properties of coumarin.

This illustrates that it is extremely difficult, if not impossible, to project benefits that may accrue from research before or at the time the work is initiated.

I know of no systematic, conscientious effort to develop methods to help quantify benefits from research or to set guidelines for what can be done or what can't be done.
The methods may not be as good as we would like them to be, but we must be willing to evaluate programs, establish priorities, develop comprehensive plans, and implement them.

**Conclusions**

Past and future changes in the commercial farm firm have important implications on the stations' research programs. There is no cut and dried formula to chart our course. The signs on the road are not very clear.

I hope there won't be too many detours, but there will and should be some, because many great discoveries come from unknown and unchartered paths. There will be stop signs, go signs and warning signs. We need to observe and profit by them.

And most important of all, we will need to build some new roads. But before deciding on the best route, engineers often explore and investigate a dozen or more possibilities.

We need to do this too, and we need to be serious about it. But once the route is clear and the blueprints are drawn, let's get the machinery rolling.

There probably won't be new money for new machines. But there is a lot of service in old machinery that is not being used to capacity or being used inefficiently in maintaining roads with little traffic.

Let's identify these roads and fit them into the over-all system and move ahead with an aggressive program geared to the most important problems.