IMPLICATIONS FOR RESEARCH:
A DISCUSSION
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My objective as discussant is one of drawing together from three very stimulating papers a few points from which to start group discussion. For this purpose it is not necessary for me to give a balanced and unbiased view of the papers since my objective is not one of presenting a review. The authors can reemphasize their most important points if I fail to mention them. I hope that what I leave unsaid may stimulate as much discussion as what I may say.

First, it is interesting to note that the authors of these papers could not discuss the implications of change in the economy of the commercial farm firm for research without also taking into account the other side of the coin. That is, research affects farm structure. Army and Smith incorporated the idea into the title of their paper, "Research and Development in Farm Related Firms--Its Impact on Agriculture." Early in his paper, Browning attributes new technology to research and also complex economic and social problems to new technology. Then he calls for research to solve a number of these problems that have arisen in agriculture. Lanpher and Finley likewise stress the role of colleges in developing, testing, and implementing management technology in agriculture. They also stress the impact that computers in the hands of progressive managers can make on changes in agricultural structure. The point is that we are discussing the impact of interaction of structural changes in the economy of the commercial farm firm with the organizational structure of our research and research services. This interaction is a dynamic process in which one change leads to another.

Browning calls for a comprehensive plan for development and coordination of research activity. By listing the failures of the past he infers that this plan should:

1. Identify problems
2. Establish goals
3. Establish responsibilities
4. Provide coordination and planning
5. Provide administration and financing

Browning, Army and Smith agree that private research will play a large role in the research of the future. The comprehensive plan is required to bring about the team work required among individuals and institutions to solve problems of a broad regional, national, and international scope which are beyond the talents and facilities of a single station or research organization.

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Likewise, Army and Smith emphasize the "systems approach" and cite the space program as a successful application. Although a comprehensive plan for research and the systems approach are not the same thing, both in this case are advanced with emphasis upon the broad nature of the problems, necessitating a team of specialists cutting across traditional departmental and subject matter lines. Likewise, one could infer that the make-up of the teams would change with the problems. Hence, a major requirement of any comprehensive plan of research and especially the formal organizational structure behind it must be one of flexibility.

Although I am in agreement that long range planning is needed to permit successful handling of large complex problems involving large outlays of resources in their solution, I wonder whether a framework in which the plans can be made and altered easily isn't more important than the plan. Perhaps some modifications in our present research organizations are in order to create a better environment for longer range projects, inter-disciplinary teams, inter-station teams and research. One should not lose sight of the fact that one of the strengths of our experiment stations has been their flexibility in permitting work on problems arising from the grass roots of both farming and science. Any long range planning or comprehensive planning that eliminates this situation could do more harm than good.

With respect to team approach, "if the staff wants to work together they will" and its converse are probably correct as Browning says. However, this leaves out a substantial group, those who have no strong feelings for or against such activity. This latter group may in fact be the most important group since its members may be more concerned with problems of research than the organizational structure of administration. For these people there are important factors which administrators influence to some extent. Each member of a team must find it easy to receive professional recognition in his own field or he will consider himself as a mere underpaid consultant. The administration can influence this situation to some extent in two ways. The individual departments must clearly recognize that the time spent by its members on such teams is an important claim. Part of such recognition is shown in the willingness of departments to contribute materially from their budgets to such joint projects. Second, the administrative organization of projects must be such that the members of a team can truly take joint responsibility. Where the handling of funds and the lines of authority in the organization and administering of a project become involved, researchers shy away. The world is full of interesting problems. Researchers generally have little trouble becoming interested. If team research is desired, organizational structure must encourage rather than just permit such research.

Although Browning calls for planning and establishment of priorities in research he admits that the payoff of research is unpredictable. Often the least impressive research at the planning stage turns out most productive. Successful solution of a recognized problem may be unimportant relative to the by-product discoveries made in solving an unimportant problem. While these facts do not negate the value of planning, they do raise questions about how tightly research plans should be structured with respect to use
of funds. It raises questions about the way in which priorities are set. In fact one can question whether there is any good overall criteria for setting such priorities.

Army and Smith and Browning alike emphasize long range planning and the former declare that the more or less blind Edisonian approach to problem solving is obsolete. However, the objective of industrial research is much clearer than that of experiment station research. Both papers point out that industrial research is conducted directly or indirectly for profit. Under this objective, systems analysis which carries current basic knowledge into merchandizable packages can be given priority. Longer term research or research simply to increase knowledge of a subject area would receive less attention. Army and Smith suggest the universities would still be a prime source of the latter "fundamental" research. Both papers suggest that the broader industry-wide problems, national and international, may be in the universities' area of research.

The suggestion that electronic data processing and communications use will control tomorrow's farm operations is intriguing. Management decision analysis service will be supplied by phone to every farmer. The technological information and directions he doesn't get by phone he can purchase at the shopping center as a crop systems kit. This may appear a bit far fetched as we look into the near future. Nevertheless, the idea of selling technical support and management guidance as a part of the package farmers will purchase from their agribusiness suppliers is a real possibility tomorrow even if one has to wait until the day after tomorrow to get the computer hook-up.

Will such trends have an effect on the structure of our extension service? Very likely they will. These trends could also increase the traffic of farmers trying to obtain an evaluation of the relative merits of various techno-management packages advertised for sale at the local farm service center. More likely, extension specialists will be called upon to give training in greater depth in a wide variety of subjects. If more specific information is furnished by suppliers, more background information will be required to evaluate this information.

Although Lanpher and Finley haven't been quite as optimistic about the role of electronic data processing as have Army and Smith, this may be one of planning horizon. The former have tried to be practical and I believe conservatively so. They recognize the trial and error and experimentation required to incorporate electronic data processing into record keeping and extension activities. I think these authors will also agree that use of computers requires more rather than less management. Someone has to furnish this, either the farmer, the extension worker, or the agribusiness agent. When you bring the EDP to bear on farm management problems you have about the same effect as doubling the machinery complement on a small farm. More land or an increase in farm size is generally required to make the latter arrangement profitable. More technical know-how and management ability are required in the former.
Reading between the lines of the Lanpher-Finley paper I think I detect the idea of process control as a potential use of EDP. The farmer's plan (program) and his records could be matched and quarterly or so he could have a summary and analysis of the differences from which to direct current decisions and evaluate progress. With a little imagination we can visualize weather data, changes in the market situation, and other factors incorporated into the analysis.

The question is whether such service can really be furnished on an individual basis. Perhaps it can because of noncontinuous requirements allowing one center to handle many different farm problems, one after another. In other industries there might be a more or less continuous analysis required.

In summary, these are points of agreement among the papers. There will be rapid changes. Research and research institutions will change. Long-range planning and provisions to handle large inter-disciplinary problems are essential. EDP will play an important part in the new research. The new research is, in turn, expected to further affect the structure of the commercial farm firm and its management.