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LETTERS

Objectives of Agricultural Research

The state of social science research in the U.S. Department of Agriculture (USDA) and the state agricultural experiment stations is indeed disappointing, as the Hightower report (1), the Pound report (2), and Nicholas Wade (News and Comment, 18 May, p. 719) point out. Even more disappointing is that there is little evidence of improvement. The lack of increase in financial and executive support for social science research in the agricultural research system is difficult to understand when compared with repeated pronouncements of interest in social problems, including rural community development.

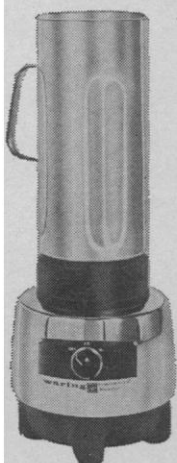
Wade places primary blame on Congress, especially on Congressman Jamie L. Whitten (D-Miss.) for the paucity of support for social science research in the "agricultural establishment." While some blame rests there, it is too easy to blame Congress for actions or inactions that disappoint us. Part of the blame must rest with decision-makers in USDA and the Office of Management and Budget. For many years they either failed to see the need for a stronger research program in the social sciences or feared to go to appropriations committees with meaningful requests.

Part of the blame also rests with social science researchers and their program administrators. We have not convinced budget-makers in the Executive Branch or members of appropriations committees of the value of our work. I know of no way to *prove* the value of social science research. Yet, if we cannot convince people of its usefulness, how can we expect them to support it?

While Hightower's *Hard Tomatoes, Hard Times* and the Pound report deplore the state of social science research in the agricultural establishment, administrators in USDA and state experiment stations have not been callously blind to social and economic problems of farmers and other rural people. Their agencies and institutions are supported by the public, so they do—generally very well—what the public pays them to do. If the public appropriates money to build chicken houses, administrators would be stupid indeed to use such money to build homes.

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research on boll weevils, I fear it will continue to be "oppressed and poverty stricken."

M. L. UPCHURCH

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References

1. J. Hightower, *Hard Tomatoes, Hard Times* (Schenkman, Cambridge, Mass., in press).
2. *Report of the Committee on Research Advisory to the U.S. Department of Agriculture* [PE 21338 (main report), PE 21339 (appendices), National Technical Information Service, Springfield, Va., 1973].

It appears that the *Report of the Committee on Research Advisory to the U.S. Department of Agriculture* (1) was incomplete. One of the four questions which the committee was supposed to answer (1, p. 2)—"the extent to which the scientists in the basic disciplines relate their research to agriculture"—does not seem to have been answered. My own answer to the question is that, with very few exceptions, the scientists in the basic disciplines have *not* related their research to agriculture.

I am a practical geneticist, a corn-breeder. I routinely scan journals like *Plant Physiology*, *Genetics*, *Heredity*, *American Naturalist*, and the *American Journal of Botany* for research reports of practical consequence to those whose business it is to breed and grow better crop plants. I have waited 20 years for a molecular biologist to suggest ways in which the Watson-Crick hypothesis could alter our plant-breeding techniques. I have been looking for 15 years for suggestions from researchers in plant physiology about how the Krebs cycle could be manipulated, genetically perhaps, in ways to increase crop yields or to improve other attributes. I have even thought that the tremendous advances in cytology brought about by electron microscopy could point out key organelles in cells that could be monitored while the corn plant—or another agriculturally important plant—was constructively altered.

But I have been disappointed. The leading figures in biological research have not pointed out how their results might be used agriculturally. Or if they have, they did so in meetings I don't attend, or in journals I don't read, or in a language I don't understand. The general impression I get is that, so long as their funds are sufficient, the leading biological scientists have no

concern at all with agriculture and whether or not it can use their findings.

This is not to say that these basic researchers *ought* to have communicated with agricultural scientists or have been interested in the agricultural implications of their research results. The conventional view is that the proper end of basic research is the furtherance of its own investigations and that practical applications are incidental to this end. Nevertheless, because the top-flight basic researchers make little or no effort to communicate with agricultural scientists, they have failed to stimulate the buildup of programs in agricultural research in those areas with the most promise of spin-off to key agricultural needs.

The basic researchers' ignorance of agriculture and its needs must be recognized as an important reason why funds for agricultural research are not now being used to support much topflight basic research.

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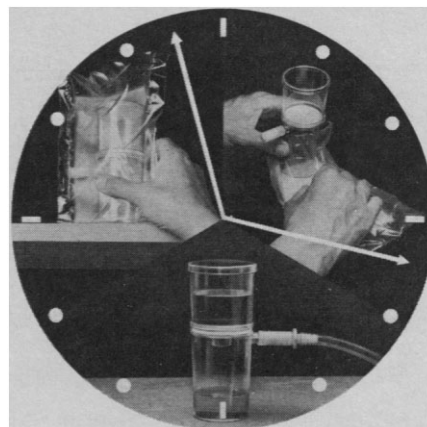
1. *Report of the Committee on Research Advisory to the U.S. Department of Agriculture* [PE 21338 (main report), National Technical Information Service, Springfield, Va., 1973].

Nicholas Wade's recent appraisal of agricultural research (News and Comment, 1 June, p. 932) is one more perspective on the elephantine set of circumstances that have contributed to both the successes and problems of this research effort. He obviously talked with a lot of people and collected many of the personal observations that have been expressed among agriculturists for several years. His commendable intentions in making these many-sided observations part of the public domain should be acknowledged.

However, the emphasis on negative, controversial, and inflammatory issues to the near exclusion of the positive, harmonious, and productive ones has resulted in overly restricted conclusions regarding the motivation of recent changes and the prognosis for future changes. When Wade states that the recent reorganization of the Agricultural Research Service (ARS) was "undertaken for political reasons," he is confusing means with objectives. As one who had considerable responsibility for that reorganization, I can say that the primary objective was to *increase* the capability of ARS to meet rapidly

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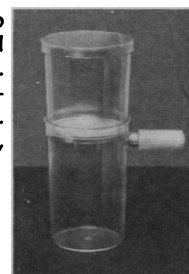
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changing problems that required agricultural research. A second objective was to *increase* the ability of ARS to work closely with the state agricultural experiment stations on mutual research responsibilities. That there were "political" and administrative activities related to achieving these objectives is to be expected but does not make those activities a substitute for the objectives. To leave the impression that this reorganization was solely "politically" motivated does a disservice to the many ARS employees who view the reorganization as an opportunity to make improvements in the system and who have responded with dedication in trying to do so.

Similarly, the conclusion that "Things will have to become a lot worse before they get any better" does not acknowledge a growing cognition among agricultural scientists, among both state and federal research administrators, and among members of the Congress of a need for changes in the system. That one could obtain differences of opinion on what those changes should be is

obvious because of the wide-ranging activities and disciplines of agricultural research. However, these differences and the sharpness with which they are expressed is, to many of us, real evidence of the interest in bringing about improvements.

Knowing the large number of institutions and people involved, the consequent potential variations in responsibilities, and the progress which has been made in identifying and moving cooperatively toward coordinating mutual responsibilities, I cannot help but be encouraged and optimistic about the present attitudes and efforts for changes in agricultural research. Such changes will ensure future successes comparable to the many achieved in the past.

NED BAYLEY

Office of the Secretary,
U.S. Department of Agriculture,
Washington, D.C. 20250

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The Blind Technique

At a recent meeting at the National Academy of Sciences entitled "Conference on carcinogenesis testing in the development of new drugs" (23 to 25 May), it was suggested by Robert Elashoff, a statistician, that pathology specimens be sent to the pathologist blind (unidentified). A spirited discussion ensued between proponents and opponents of this viewpoint. The response of the audience clearly indicated a polarization either for or against the viewpoint proposed. Any discussion of this important matter should take into account a number of philosophical considerations which are familiar to pathologists but may be less well known to other scientists.

1) Many statisticians consider the examination of pathology specimens and slides analogous to the examinations in a double-blind clinical study. There are, however, important differences. The placebo factor in treating patients who are easily influenced by psychological factors and expectations

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