

AN ESSAY ON FEDERAL-STATE RESEARCH PROGRAMS
IN AGRICULTURAL ECONOMICS: NEEDS AND PROSPECTS
FOR THE FUTURE IN AGRICULTURAL MARKETING

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A very cursory review of our agricultural economics literature shows a history of concern in our profession about the effectiveness of our research efforts. And, concern and criticism follow us to the present day. Certainly, there are no simple answers to the questions that face us. The question for discussion cannot be elaborated on in an authoritative and summary fashion. Though more modest in scope, the question before us is part of a much broader question of where are we and what should we be doing in agricultural research. This broad question was posed by the Senate Committee on Appropriations in 1965. We recall that the level of concern was such that USDA and the Association of State Universities and Land Grant Colleges arranged for a special 12 member USDA-SAES task force to study and report on the situation. Their comprehensive report, entitled "A National Program of Research for Agriculture" (Report of a study sponsored jointly by an Association of State Universities and Land Grant Colleges and USDA, Oct. 1966) is testimony to the monumental nature of the assignment.

We know that the matter to be decided in our profession is important. If it is not, then, why would its nagging presence cause such endless professional soul searching and, sometimes, even bewilderment? Why the frequent uneasiness about the real merit of research programs? Is it because we feel that too often the question of justification has been faced down or circumvented, rather than faced up to? Could it be that our customary approaches to the assessment of research needs are part of the reason why questions continue to be raised?

These are bothersome thoughts, but not so much in relation to research goals. Goals are generally in the nature of articles of faith and usually, by consensus,

are placed beyond the reach of debate. An excellent example is the Agricultural Marketing Act of 1946. This piece of legislation had a tremendous impact on the agricultural economics profession. It has provided the basis for much of our past and current research on the economics of agricultural marketing. The goals of the Agricultural Marketing Act of 1946 are presented in Section 202. I cannot present these goals to you in a better way than by reading to you directly from the Act which states, "...that marketing methods and facilities may be improved, that distribution costs may be reduced and the price spread between the farmer and consumer may be narrowed, that dietary and nutritional standards may be improved, that new and wider markets for American agricultural products may be developed, both in the United States and in other countries, with a view of making it possible for the full production of American farms to be disposed of usefully, economically, profitably, and in an orderly manner." While the entire legislation is somewhat more specific than the portion selected, it, nevertheless, serves to illustrate the point that we work with rather broad goals and we generally find it difficult to disagree with them.

The real source of worry and disagreement is the vagueness and lack of coordination in recognizing, identifying, and defining research problems. Problems exist, along with implied needs, whether goals are specified or not. Goals help keep things on track. After the fact of problem identification, they are useful mostly in rounding out our decisions about which research projects are desirable, just as resources and other constraints impose realistic limits on what is possible.

It is important to remember to keep this after the fact function of goals. Failure to do so has frustrated

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objective problem identification since the beginning of organized research. It is extremely difficult to search out and formulate meaningful problems for a research program when we are equipped with nothing more than a well intended set of goals. What is needed is some timely information on the how, where, when, and what of problem identification.

The art of identification and diagnosis in the empirical world of economics is important in its own right just as it is in the natural fields of science. In the natural fields, the pursuit of problem identification depends upon tedious classifications of elemental observations. But, we do not go about things in this way. To me, this is a major soft spot in our research philosophy. We depend on abstract principles and solution strategies and encourage cultivation of goal appreciation to the neglect of basic problem orientation. Then, we are disappointed when we instruct research staffs to go forth and apply their talents, and are invariably asked by these same people if we have any particular problems in mind that need exploring. Then, we must undergo the embarrassment of having to scout around for research problems for people who we think should have some real live ideas about needed research. Who is at fault? Certainly not the researcher. We really cannot expect any different response from people whose training and conditioning does not stress a fundamental acquaintance with real world problems.

In spite of these difficulties, problems must eventually be rationalized in terms of logical principles to see how they fit into the general scheme of things. But, this does not negate the need for having the sharp eye of a troubleshooter. And, whether we care to admit it, we are being called upon more and more to be economic troubleshooters. The winds of change tell us this is the way it must be.

Quick response to change demands instant communication about what is going on. For this to happen, some way must be found to use relevant current impressions about emerging problems and to distribute them immediately throughout the research community. We need to find some way to continually broaden our collective professional perspectives. When we allow ourselves to retire behind the walls of our respective specialities, we lose touch. And, although our individual research achievements may be good, they would likely be much improved by the benefit of contact with and knowledge of a broad perspective of potential and emerging problems.

As set up, the apparatus of our professional organizations will not do the job. The journals and other devices of communication are excellent for individual idea development and encouraging general professional rapport, but they are not tuned in very well to the

pressing everyday needs of research activity. Nor are arrangements of contacts among research departments and agencies sufficiently formalized to implement the timely circulation of informative developments. There is a great tendency for the isolation of activities and interest among our fellow research agencies. We do not have a convenient clearing system of ideas and problem identification facilitating the performance of the total research community.

Neither are USDA Research Advisory Committees presently equipped to handle the task. Without a year-round committee staff to receive and process research leads from the agribusiness community, they cannot be expected to function as full-time information centers. Besides, they are formally organized to address their attention primarily to the program needs of USDA, not the Federal-State system at large. But the thought of formulating such a system suggests interesting possibilities. Of course, the idea is not new. Every important trade association and action group recognizes the importance of stimulating an inflow and outflow of information. This is the best way to promote involvement which is essential to organizational viability. Many corporations also use newsy house organs as tools in implementing personnel policies. Even government agencies do. The question is, can these principles be adapted to the workaday needs of the agricultural economist? To follow through on this question requires some justification about why and some notion about how.

It is time, I think, to acknowledge that we are well into an era that is reshaping the role of agricultural economists. No longer can we realistically expect the researcher to function most productively as a complete diversified package. The slow pace of the past allowed him to be purchasing agent, production engineer and sales agent, all in one, for a research activity. As fast as things move now, it is a misuse of his time and energy to require that he have to scrounge around by his own devices for problem leads to maintain an orderly flow of useful research activities. Some way must be found to provide direct access to research leads, and at the same time, enhance initiative.

A current digest of problem related events in agriculture and agribusiness would be an invaluable working tool for streamlining the task of finding appropriate problems. Moreover, it would stimulate a better appreciation of the whole family of changing problems in the economics of agriculture and interacting industries. If properly conceived, the digest could preserve subject matter identity for reference purposes without destroying its usefulness as a chronicle of the overall course of pertinent events.

What is suggested here is systematization of the principle of "extension in reverse." The Federal-State

research and extension system has an elaborate apparatus with abundant touch points for transmitting research and advisory outputs. Would it be possible to use the same channels of communication to achieve a better coordination of the backflow of inquiries and problems? It is not uncommon to discover quite by accident that Economic Research Service and certain State research staffs have been called upon separately to furnish quick solutions to an identical problem. Apart from the question of jurisdiction, better communication could often prevent fruitless duplication of effort. What's more, an awareness of what is going on and an exchange of views most of the time will help in packaging a better answer.

If we think of research as an input-output transformation for which problem shaping ideas are inputs and solutions are outputs, we see the importance and need of the suggested "extension in reverse" machinery. Conceptually, every person in the research and extension system and their "outside" contacts would be potential reporters for their respective agencies or stations. Ideally, they would feed bits and pieces of information to a special agency staff charged with editing and collating these inflow items into appropriate problem area patterns. These units, in turn, would forward their summaries of current events to a central office staffed and equipped to publish reports from all areas in, say, "The Research Times" or "The Researcher's Journal" for distribution to the profession. Energetic units might also want to glean signals of pertinent developments from newspapers, trade journals, and other media for inclusion in the professional digest. No one would be required to make free gifts of personally conceived research strategies, so the possibility of someone being "scooped" on a pet project would be nil. In fact, by being able to keep abreast of the times, even the most sophisticated performers could do a better job of sharpening up their research product.

We now return to where we began. For the most readily observable benefit from effective communication of ideas, generating information would show up in the program planning stage. Better equipped to identify problems and judge their relevance, researchers could come forward with proposals that could be appropriately meshed into agency goals with much less friction. Fewer projects would be rejected because of goal disorientation and researchers would be spared the agony of trying to second guess the system. Moreover, a flexible and more inclusive idea foundation would facilitate matching research needs with the talent and interest structure of the research staff. Among agencies, with everyone generally aware of the direction research needs and interests are drifting, research programs could be more efficiently coordinated for the achievement of communitywide goals.

In Marketing, as well as other Divisions of ERS, knowledgeable fast action response to an inflow of inquiries is a part of our way of life and an interesting and revealing one. Requests come from the general public, Congress, industry groups, Federal and State agencies, private research organizations, foreign sources and from about any other kind of inquirer one can imagine. Thinking negatively, these requests may be regarded as just another set of chores to be performed. Viewed from the positive side, they are not. Of course, sometimes it is necessary to think negatively, but not always.

Because these inquiries usually have their basis in real world problems and come from a broad spectrum of concerned parties, they form patterns of clues about research needs. Mental reviews of these indicators consciously and unconsciously influence our thinking about how we should shape our research program. Someday, as the standard excuse for postponement goes, it would be more than worthwhile to make a formal comprehensive analysis of the research implications of inquiries coming to ERS for the edification of the supervisory and research staff. This would amount to an in-house variation of the previously discussed concept of a research digest for economists.

ERS researchers are exposed to incoming inquiries because their expertise is required in providing answers. But, in practice, exposure is not universal. Naturally, since the burden must be shared and talent utilized as efficiently as possible, requests are channelled to researchers according to these guidelines. Still, the experience for them is enlightening because many requests exceed the boundaries of expertise for any one economist. Probably nothing creates a firsthand appreciation of the work of others among our economists more than being assigned to a broad question that must be answered promptly. Then, the one responsible for the assignment has no choice but to call upon others and through the experience of exchange becomes a better problem solver.

Quite frequently, requests come through that even overtax our collective resources. These are the things that lay bare the deficiencies in our research program and make us painfully aware that withdrawals are exceeding deposits in our research accounts. More specifically, they reveal imbalances in a program that must be concerned with both immediate and long-term solutions. In program development and implementation, these requirements are not independent of one another.

An important part of the responsibility of the long-term ongoing portion of the program is to continually bank reserves of "solution materials" for use in handling urgent problems that cannot be postponed. Failure to maintain a reserve of "solution

materials" means that some immediate problems will simply be solved by default. Having passed unattended, these problems may reappear as new problems more baffling than ever.

Some urgent problems do tolerate a certain amount of postponement – perhaps, measured in a few weeks or months. But, without program flexibility, it is useless to delay action for such short periods of time. A thoughtfully balanced program stands ready to meet these intermediate emergencies by including the machinery to marshal necessary resources and apply them quickly.

To view these intermediate projects as one-shot adventures in research is a mistake. Properly conceived, they are seen as means for generating two directional contributions to the total research effort.

These short-term investments also bear revenue for deposit in the general research reserve. But equal, if not more rewarding benefits, are the insights into similar broader problems that can be incorporated as assets in the general line of research inquiry.

The thoughts expressed here about research organization and planning have been viewed largely from the standpoint of the Economic Research Service. However, it should be self-evident that this is not solely a matter of a single research organization. Getting on with the job of meeting immediate, intermediate, and long-term needs is a collective responsibility for all of us. Bringing about the kind of Federal-State cooperation that would expeditiously meet this responsibility would indeed be a real achievement in research implementation.