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Resource Economists at Land Grant
Universities: An Applied Perspective

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The objective of this paper is to argue that the primary research role of land grant university resource economists is applied, problem-oriented research.[1] This is accomplished through a set of seven loosely related premises justifying applied research. In addition, some obstacles to applied, problem-oriented research are identified.

Semantics

A common language is a necessary prerequisite to a meaningful debate.[2] Much apparent disagreement among professional economists regarding their appropriate research role evaporates when each party clearly understands their opposition's vernacular.

Research is a process carried out to create new information, identify new relationships, develop new concepts, or verify existing concept. Johnson (1984) identified three types of agricultural research. His first two types--problem solving (PS)

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[1]Nelson (1959) provides a succinct argument supporting basic scientific research. I agree with his argument wholeheartedly and would strongly argue that the bailiwick of many scientists, including many at Land Grant Universities, is basic research, but that cadre of scientists does not include agricultural economists.

[2]This paper was commissioned to serve as a point of discussion regarding the resource economist's role. A companion paper by Biere was commissioned to develop the counterargument, thus the debate context.

and subject matter (SM)--are applied; PS relates to activities designed to solve a particular problem, and SM relates to activities designed to solve a broad range of subject-matter problems. Premises supporting applied research will be based on Johnson's PS and SM categories throughout this paper; that is, applied research is that undertaken specifically for the purpose of obtaining information to help resolve a particular problem, either problem specific or discipline related. Johnson's third type of research is disciplinary (DISC) research, which improves one of the traditional disciplines by improving its theory, its fundamental measurements, and its techniques. Basic or disciplinary research, then, will be defined as that research carried out to expand knowledge, especially in a traditional discipline, without regard to contemporary problems or felt needs.

This apparently clear distinction between applied and basic research is actually quite blurred; and not at all discreetly disjoint, it is rather a spectrum or continuum. For example, almost all research becomes applied in the long-run because it is used to solve problems. And contrarily, almost all research becomes basic in the long-run because the knowledge remains after the problem is solved, to be used for other problems or for its own sake.

Premises

The following premises supporting the argument are in no particular order; they are neither claimed nor believed to be mutually exclusive nor all inclusive. Yet this is not to say that the premises do not represent prevailing thought among agricultural economists. Most support the applied, problem-oriented research role on its own merits yet not at the expense of basic research.

Premise 1

Our clientele and mandate in land grant universities call for applied, practical research. Agricultural producers, commodity groups, agribusiness interests, and legislators look to land grant institutions for practical solutions to their problems. They seek answers to today's problems today. Consultation boards and experiment station directors favor concentrating on applied problems to please the legislature and the business community (Norton 1973). These groups favor those researchers whose work has a clear and easily traceable benefit to their interests (Lipman-Blumen and Schram 1984).

Russell (1962) argues that as citizens, scientists have a public duty to see, as far as they can, that their skill is utilized in accordance with the public interest and not simply to supply knowledge for its own sake. Our clientele seek facts to guide decisions; such facts do no good if buried in scientific journals. The real strength of our profession has come from researchers' ability to solve practical problems in agriculture and to identify with farmers and farm groups (Wiegmann 1980).

Castle (1981, p. 276) argues that "there is no problem more important to the administrator of agricultural education and research programs than the reconciliation of the interests of multiple clientele." He further argues that to maintain or enhance credibility, research needs must be established and documented in a professionally defensible way in order to distinguish between real social need and researchers' wish-list requests. As Graduate Dean, Castle saw agricultural departments shift from addressing farmers' problems to basic research as limits to the application of knowledge were reached. This has led to claims that the public agricultural research establishment is not serving the clientele it was established to serve; in other words, agricultural colleges created to serve the common people have abandoned them (Hadwiger 1982). Others have observed that users' needs and the world of agriculture are being upstaged by the world of science (Lipman-Blumen and Schram 1984). Castle (1981) acknowledges this is an inevitable conflict between disciplinary needs and user-group demands.

Twenty years ago Kelso (1965) argued that agricultural economics was moving away from answers for real world decision makers toward useless models of a hypothetical, simplified world. The land grant system was established to answer real world questions; our clientele are waiting.

Premise 2

Returns to applied research are greater than those to basic research because a higher percentage of applied research is useful--and useful today. Some basic research may never be used. Much of what we now have is not being used. We have a surplus of basic research. Our models are well ahead of our data and our ability to use them for real-world problem solving. Miernyk (1976) has found this to be the case in socioeconomic impact assessment where it still appears to be true that our capacity to design highly sophisticated regional models has far outrun our ability to implement them, given the primitive nature of available data in data-gathering techniques.

It is inefficient to continue to add to our basic knowledge until we know how to use more of what we have. There are plenty of problems around today that need solving; in Frank's (1949, p. 21) words "we ought not shirk the present aspects of today's problems in order to indulge in too much tinkering with tomorrow's." Much of what we have learned already can be used to solve today's problems with only a little understanding of the problems and application of the tools by concerned scientists. A very successful, North Dakota farmer and businessman summed up the essence of this premise by noting that he had only a third-grade education, but he had not used all of that up yet!
Lindblom and Cohen (1979, p. 88) argue that

> Projects are often justified on the grounds that all knowledge is valuable. Since sooner or later we need to know everything we can, any well-designed project is thought to be worth doing. It is a measure of amateurishness in project choice that those. . . who so argue do not take the next step in their logic of project justification. For if the value of (all research) is in fact so high as to promise returns on almost any kind of investigation then (research) ought to be considered a resource so precious as not to be squandered. It is assumed high value does not justify a relatively indiscriminate endorsement of any project, but calls instead for careful allocation.

That careful allocation is an important economic decision, with heavy weights placed on current real-world problems to be solved through applied research.

Premise 3

Applied, problem-oriented research make us and our results visible to policymakers (Norton 1969). Policymakers will only ask us for our input if we can relate what we know to the issues they face. The publicly distinguished agricultural economists have made their mark by being pragmatic as well as, or rather than, esoteric. This assumes we want to influence policy, and since no scientist is completely free from values, not even physicists (Frank 1949), we cannot argue that we should avoid delving into policy matters.

We have become so enamored with creating academic novelties and basic research that Castle (1980, p. 102) has observed "economists have something important to say; it is just that no one important is paying any attention." In the words of yet another sage, ". . . the once great concern with social institutions has been pushed into the background during recent decades in favor of quantitative optimizing models" (Ciriacy-Wantrup 1971, p. 40).

At a recent workshop on the 1985 Farm Bill sponsored by the Office of Technology Assessment,[3] no small concern was articulated for the lack of knowledge regarding private provision of public goods! The problem is not a lack of knowledge but the inability of economists to take that knowledge from the academic journals and sophisticated computer models to the policymaker (Leistritz and Murdock 1981).

Premise 4

Agricultural and resource economists are, by definition, applied scientists. The fundamental discipline of economics, on

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[3]"Technologies to Benefit Agriculture and Wildlife" workshop, Washington, D.C., October 29-30, 1984.

the other hand, is "what economists do." Agricultural and other applied economists use the tools (theory, laws, models) developed by the traditional social scientists. We are the logical connection between theorists and economic problems. Kelso (1965) however, argues that there is no such thing as agricultural economics, only economics applied to agriculture.

Even the prestigious American Economics Association was organized in the late 1800s to encourage research into the "actual conditions of industrial life" (Norton 1969, p. 10). The "classical economists were largely problem-oriented. . . .one would hope that the man of powerful intellect is attracted by both the problems and the apparatus used to solve them" (Norton 1973, p. 19).

While universities have economics departments to do basic research into man's behavior, they also have departments of applied economics (i.e. agricultural and resource economics) to apply that basic knowledge to contemporary, real-world problems.

Premise 5

Applied research makes us appreciate the interdisciplinary nature of today's problems. It forces us to think like problem solvers, not economists with an answer looking for a problem. Just as teaching sharpens our theory, so does applied research sharpen our understanding of the discipline to the point of testing our theory. Economics for economics' sake leads to knowing more and more about less and less or "being wrong in a more elegant manner" (Kelso 1965, p. 11). It is like rearranging the deck chairs on a sinking ship or navigating its correct course (Castle 1980).

Leontief has argued (1982):

> Year after year economic theorists continue to produce scores of math models and to explore in great detail their formal properties; and the econometricians fit algebraic functions of all possible shapes to essentially the same set of data without being able to advance in any perceptible way, a systematic understanding of the structure and operations of a real economic system.

He now prefers to work with engineers, psychologists, and scientists other than economists "because they know how the real world works."

An "extension background in an academic discipline makes it more difficult to determine a specific area for research, . . "(Ross 1974, p. 15). The "solutions to contemporary problems require research involving an increasing number of disciplinary specialties. . . ." (ESCOP 1984, p. 4)

In summary, applied research not only gives us a better understanding of our own discipline (Merton 1949) and its strengths and weaknesses, but also leads to solutions for today's multidisciplinary problems.

Premise 6

Applied research makes us better teachers. It demonstrates to our future policymakers--students--the relevancy of theory and how it can be applied to understand and solve real-world problems. Students in agricultural economics curriculums are interested in a discipline that can be applied to solve their problems or the problems of their employers. An instructor actively involved in a program of applied research can be much more effective than one not so involved. In this case the teacher both produces the material through research and retails it in the classroom.

Premise 7

Applied research attracts research dollars. With these dollars we may be able to identify and often accomplish our basic research. Applied research is easy to get funded--someone has a problem they want solved. Basic research, on the other hand, is not so readily funded. Shortcomings identified in application of our knowledge can be solved, at least in part, by bootlegging basic research within an applied project. These basic research needs then become applied basic, since the shortcoming needs to be overcome to resolve a current problem. This type of basic research--applied basic--avoids trivial solutions. The felt needs for basic research are real rather than conjured up by economists with little or no feeling for contemporary issues.

Obstacles to Applied Research

Although the foregoing seven premises might imply applied research is preferred by agricultural economists over basic, the contrary is the rule. Promotion and tenure rewards go to basic researchers writing for their peers and academic journals (Johnson 1984). A lack of empirical data or a poor understanding of the problem makes it hard to apply pure theory to real problems, so real problems get ignored.

Because applied research is problem-oriented, it carries a time element. Applied research is short-term and needs to be seen through to completion vis-a-vis basic which can be abandoned or forever delayed. Therefore, deadlines, a necessary evil of doing applied research, tend to encourage basic research without deadlines.

There is a stigma that applied research is done by those who fail at basic research. Applied researchers are escaping the rigors of their discipline. Thus, a cult or guild of scientists is created that discourages applied research and encourages created novelty. This cult is afraid to reveal to other disciplines and nonacademicians just how rudimentary their basic skills are, preferring to keep them masked in the cult's jargon and journals.

Applied research forces researchers into policy positions as experts on economic aspects of real world problems. Many professionals either do not like the heat or cannot relate their knowledge base to applied problems. Basic research is politically much safer.

Applied research very often calls for a multidisciplinary approach. Such teams of scientists are difficult to form at universities (Hadwiger 1982, Swanson 1979). Their success depends upon the ability of scientists from many disciplines to speak a common language, a language that has yet to overcome the cults' highbrowedness.

Finally, applied research is generally funded by outside sources that require proposal writing, progress reports, final reports, and other administrative red tape often absent with basic research. This administrative burden coupled with tedious financial and personnel accounting make applied research less attractive when compared to in-house funded basic research.

Conclusion

Seven premises were advanced to support the contention that agricultural and resource economists should direct their efforts toward conducting applied research. Subsequently identified were disciplinary shortcomings (i.e., basic research needs) for economists to ameliorate. The obstacles to applied research, namely strong incentives to do basic research and disincentives to doing applied, have moved the center of gravity of agricultural economics research well toward the basic end of the research spectrum. The land grant schools established to do applied research have reward systems that encourage basic--the cart is dragging the horse! Because of these obstacles we have an abundance of "theorists" and a shortage of "applicators". It is time to use some of our storehouse of economics to help solve real-world, applied problems; only then will the most efficient directions for continued basic research be identified.

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