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# SOME PROBLEMS OF RESEARCH ORGANIZATION IN AGRICULTURAL ECONOMICS

by

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Some Problems of Research Organization in

Agricultural Economics[1]

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Agricultural economics does not suffer from provincialism in the sense that individual research workers are unwilling to lift their heads from their individual investigations and show interest in the activities of their colleagues. On the contrary, the Journal of Farm Economics has traditionally been a sounding board for those members of the profession who have desired to make suggestions as to the direction that research in agricultural economics should take.[2] In recent years the belief has existed that a significant number of relevant problems have been overlooked because of the traditional specialization in agricultural economics.[3] Certain of these neglected areas

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[1] These remarks were originally given as a seminar to the Department of Agricultural Economics at Purdue University. They have been modified somewhat as a result of the discussion at that seminar.

[2] For example: Schultz, T. W., "Theory of the Firm and Farm Management Research", JFE, Volume XXI, No. 3, August 1939; James, H. B., "Agricultural Economics in the Years Ahead", JFE, Volume XXXVIII, No. 5, December 1956, and Brandt, Karl, "The Orientation of Agricultural Economics", JFE, Volume XXXVII, December 1955, No. 5.

[3] Brinegar, George K., Kenneth L. Bachman and Herman Southworth, "Reorientations in Agricultural Economics", JFE, Volume XLI, No. 3, August 1959.

have been identified and recognized scholars have contributed articles which have reviewed relevant research and specified neglected areas or problems.4/

This type of soul-searching is not to be disdained; a certain amount is necessary if we are to be progressive, although this type of introspection can become pathological. Individuals and even departments can spend so much time in self-evaluation that they fail to give their best effort to the work they are attempting to carry on.

Even so, it appears that there are a considerable number of problems that both the administrator and the individual research worker must face that do not appear to have ready solutions. Despite the rather substantial amount of literature on research methodology and elaborate administrative arrangements within the Experiment Stations and the USDA, the number of concrete guides that can be used in specific situations is quite sparse.

Specialization Within Agricultural Economics

It is well known that agricultural economists have developed rather highly refined areas of specialization. The traditional areas of specialization--Farm Management, Land Economics and Marketing were further sub-divided and some individuals become known as commodity specialists, farm record analysts, and tenure experts. Historically the man of wisdom and stature usually ended up being an expert on agricultural policy. It has been charged that investigation within these sub-disciplines tended to become standardized, rigid and

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4/ Nerlove, Marc, and Kenneth L. Bachman, "The Analysis of Changes in Agricultural Supply: Problems and Approaches", JFE, Volume XLII, No. 3, August 1960; Ruttan, Vernon, "Research on the Economics of Technological Change in American Agriculture". JFE, Volume XLII, No. 4, November 1960; Clodius, R. L., and Mueller, Willard F., "Market Structure as an Orientation for Research in Agricultural Economics". JFE, Volume XLIII, No. 3, August 1961; Fox, Karl, "The Study of Interactions, Between Agriculture and the Non-Farm Economy: Local, Regional and National", JFE, Volume XLIV, No. 1, February 1962.

unimaginative.[5/] As a consequence, studies were repeated in numerous states with the findings being highly predictable.

We are all familiar with the changes since World War II and there is no need to belabor them here. The growth of modern production economics encompassed farm management, land economics, agricultural finance and part of marketing. Today almost every Land Grant Institution that offers graduate work has at least one and usually two courses in Production Economics. However, the victory for production economics was far from complete. Glenn Johnson was pointing out by the early 1950's that farm management was more than production economics and that exclusive concentration on the theory of the firm would result in many relevant management problems going unnoticed.

The development of certain quantitative techniques made it possible for certain people to work "across the board" with little reference to traditional areas of specialization. Even so, in most Departments of Agricultural Economics considerable specialization still exists. In part this may be due to the earmarking of certain funds for marketing research, although this can explain only a minor part of the specialization which prevails.

Whatever the case may have been historically, in the present setting the disadvantages of specialization on a factor or commodity basis come down to four:

(1) The most rewarding research problems do not come to our attention.

(2) The pressure of a particular clientele may put undue emphasis on routine problems with a low "pay off" in terms of adding to our store of knowledge.

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5/ Heady, E.O., "Elementary Models in Farm Product on Economics Research", JFE, Volume XXX, No. 2, and "Implications of Particular Economics in Agricultural Economics Methodology", JFE, Volume XXXI, No. 4, Part 2, November 1949; Salter, Leonard Austin, A Critical Review of Research in Land Economics, University of Minnesota Press, 1948.

(3) This kind of specialization prevents an individual from specializing in a different and possibly more fruitful way. This latter type of specialization might involve considerable familiarity with a particular empirical technique or a particular area of economic theory.

(4) Vested interests within a department may lead to the "building of fences" around an area. In other words a researcher may not want a colleague working in an area on which he has staked a claim. If this occurs problems may not be meaningfully formulated or invested in a fundamental manner.

Still another disadvantage might be in training. If our graduate students are given so much training in the factual and institutional considerations pertaining to a particular factor of production or commodity, they may have insufficient time to spend on the tools of analysis such as economics and statistics. However, I believe most institutions have overcome this obstacle and the chief problem at present is not the quantity but the quality of instruction in these areas.

With respect to the first disadvantage, it would be fruitless to deny that it may not be operative. On the other hand, agricultural economists do operate on a broad scale at the present time and if significant questions escape them, it must in large part be due to their lack of imagination in recognizing these problems. The fact that these traditional specialities have persisted is some indication of their utility. It seems a reasonable assumption that people would not turn out in large numbers to attend extension conferences centered around these subjects if they did not believe some value was being obtained.

I am somewhat more concerned with the second problem. In our effort to have something for these people on an annual or more frequent basis we may have created a pressure that forces us to short-term routine research efforts. I do not believe there is an obvious answer to this. We need to be aware of this possibility and attempt to minimize its impact.

If there is a need for commodity and factor specialists from the standpoint of satisfying our clients it may be desirable to retain these specialties for extension purposes but at the same time permit research workers to specialize in quite another way. This would involve an individual selecting an area of theory that he believes most relevant to his area of research. For example, a person working with problems of rural development may be particularly concerned with location theory. He then may take the special responsibility of keeping abreast of this field. Such a person might also be interested in aggregate input-output analysis as being an important empirical technique that might be used. If our research workers specialized in this way they could take the responsibility of keeping the remainder of the staff informed, at least superficially, about developments in these areas by means of seminars or by reporting on the results of their research.

On the other hand, there should be some people in the department who are free to apply their tools across the board. One who knows the theory of markets and a good knowledge of simultaneous equations and other empirical techniques may be able to apply this knowledge equally well to commodity and factor markets.

If the above ideas have merit, study should then be given to the basic disciplines which are relevant to agricultural economic problems. Perhaps we should be more concerned with this kind of balance than we are with having so many people in marketing, farm management, etc. If this were done promotions and salary advancement could be based in part on concrete evidence of professional improvement in the area the researcher has chosen as his specialty.

Disadvantage number 4 is operative in some departments. However, where it is operative it is a symptom of a more basic difficulty than merely specialization. If such provincialism is supported administratively then the administrator is solving personnel problems by a least resistance means. If a staff imposes such a restriction on itself then it is obvious an infusion of new thinking in the staff is needed. The above should not be interpreted as giving support to the person who flits from one area to another as his fancy dictates but it is intended to support the right of an individual to pursue a research problem to its roots regardless of where those roots lie.

The above brief discussion of specialization does not, however, get at some of the basic difficulties associated with the selection of research problems nor the way we should organize to tackle those problems. To treat these issues it is necessary to explore some methodological considerations of a more fundamental nature.

## Kinds of Research Problems

There is considerable evidence that the difference between the true pioneering research worker and the mediocre researcher lies in the formulation of the problem to be investigated and the hypothesis to be tested. There is also evidence that no rules exist whereby the pioneer can transmit this ability to others except by example and stimulation. In other words, intuition and imagination must be brought to bear in making the leap from our present conception of the ways things are. This is not to say that it is inherently impossible to impart this ability to others. It is to say that at this time it cannot be taught as we can teach a person to fly an airplane, to bake a cake, or to work a regression problem.

It is possible, however, to classify research efforts and in this way arrive at some judgment as to where current emphasis in agricultural economics is being placed. Numerous classifications have been suggested. Classification on the basis of subject matter specialization--land economics, farm management, etc.--has already been mentioned. Another classification is on the basis of normative versus positive orientation. Still another might be basic versus applied research. For certain purposes a micro versus a macro orientation may be used.

Presumably research is undertaken to answer some question. A question occurs when someone experiences a problem. Problems may be said to be of two kinds--theoretical and practical. A practical problem arises when some person or group of people experience a gap between their objectives and their actual achievements. As a result such people are primarily interested in questions of "what to do". Theoretical problems, on the other hand, arise because of someone's dissatisfaction with the current state of knowledge with respect to "what is". A theoretical problem can be discovered either in seeking an answer to a practical problem or to satisfy man's inherent intellectual curiosity. In other words, theoretical problems can arise because questions of "what to do" cannot be satisfactorily answered with existing "what is" knowledge.

Let us return to practical problems that can be expressed as "what to do" questions. Questions of this kind are usually referred to as normative. If a gap exists between a person's objectives and his current achievements, it is clear that the difficulty can be resolved in two general ways. First, the person may change his actions so that he more nearly achieves his objectives. Secondly, he might change his objectives to more nearly conform with his achievements. It is usually the first kind of advice that economists are called upon to give. We may be expected to develop a more profitable farm plan or to recommend a more profitable product mix to a marketing firm. If the objective of the client is clearly stated or well understood, value judgments are not necessarily implied. For example, the statement "If you want to maximize profit, you should eliminate your 20-head herd of dairy cows and expand your hog enterprise by 30 sows", does not necessarily involve a value judgment. By the same token such statements as "If you wish to have price supports and avoid surpluses you must provide for some type of production control" do not involve value judgments. Value judgments may enter when we do not accept the client's objectives, although this may be a way of solving his problems. In fact, psychologists and psychiatrists rely heavily on this method. Even here, value judgments do not necessarily enter if the professional person merely shows the inconsistency of means and ends. There are a great many "what to do" questions that are normally classed as normative, that may not involve value judgments and which may not call for a loss of objectivity on the part of the researcher. Of course, there is always the danger of phrasing the answer to a "what to do" question in such a way that our value judgments do play a role without this being recognized and the value judgments identified.

If the above is accepted, it becomes possible to offer the following propositions:

1. That there are some "what to do" questions that can be answered without any additional "what is" information.
2. There are other "what to do" questions that are being answered routinely on the basis of existing "what is" information but we have little notion about the reliability of those answers. For example, how much confidence can we place in our routine outlook work? How confident are we of our recommended farm plans? Have we really tested our "what to do" recommendations against subsequent performance?

3. There are many "what to do" questions where all reasonable people recognize that the "what is" information is grossly inadequate for the development of a "what to do" answer.

4. There are "what is" questions which intrique the researcher but which, at this time, are not necessary to answer current "what to do" questions. However, the amount of research resources currently being devoted to answering such questions in agricultural economics is relatively small.

The two way table which follows may illustrate the above points.

| Type of Interest | Type of Problem "What is" | "What to do" | Percent of Research Resources |
|---|---|---|---|
| Primarily Popular | | 20 | 20 |
| Professional & Popular | 30 | 30 | 60 |
| Primarily Professional | 15 | 5 | 20 |
| Percent of Research Resources | 45 | 55 | 100 |

Dr. Hardin has estimated the uncircled figures in the table for Agricultural Economics at Purdue. I entered the circled figures for illustrative purposes on the basis of deduction and sweeping assumption. I assumed that the research of popular interest only is concerned mainly with "what to do" questions based on "what is" knowledge that is generally assumed to be satisfactory. Therefore, I credited the full 20 per cent to "what to do" questions; I divided the professional and popular equally between the two problem types although I have no basis for any division. I assumed further that "primarily professional" included some "what to do" effort based on a reading of the Journal of Farm Economics. I therefore, come to the conclusion that there is not a great amount of effort being devoted to "ivory tower" work without application to a practical problem in mind. More searching analysis might disprove this hypothesis and I certainly intend to do additional testing. However, a reading of the Journal of Farm Economics and other scholarly articles written by agricultural economists certainly support this position. Almost every research project proposal that I have seen attempts to relate the anticipated research results to some practical problem. In not all cases do the results have practical application but this is usually due to an unsuccessful research effort, or because of a poor formulation of the practical problem. In any case, at this stage I fail to find evidence that too much emphasis is being given to "positive" research and that too little emphasis is being given to "normative" research if "normative" is defined as being the same as the "what to do" category described above.[6/][7/]

## Decision-Making with Respect to "What is" and "What to do"

Now let us turn to the nature of the decisions which we face. First, consider research oriented to "what to do" questions but which depends on "what is" information. In this case the eventual pay-off is in terms of the economic decision which is affected and the probability of obtaining the relevant information.

If: $G_A$ = Actual gain from research as reflected in its impact on decision-making.

$P$ = Probability of a research project resulting in the discovery of the relevant information.

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6/ For a contrary point-of-view see: Johnson, G. L., Some Philosophic Thoughts about NCR (4)'s Work. Mimeographed.

7/ I am of the opinion that the word "normative" as now used by agricultural economists does not convey a precise meaning. There is need for someone to re-define the word or provide a new classification.

$E(G_A)$ = Expected pay-off from research in terms of its impact on decision-making.

Then, $E\ (G_A) = P \cdot G_A$

If $r_P \cdot G_A$ is $\overline{<} 0$ then the largest pay-off probably would occur from research on those problems where $G_A$ and P are intermediate in nature. In other words, the routine work with a higher probability of success probably does not have as large a potential pay-off as those research efforts dealing with large pressing social problems. However, the probability of success is undoubtedly much higher for the former. Between these extremes there are intermediate problems with substantial pay-off but with a probability of success in the (say) .7 to .4 range. If the above hypothesis is correct it would appear that this is the type of problem that we should emphasize. It is recognized, of course, that these functions will be different for different individuals. The trick, of course, is to get each individual to obtain his maximum pay-off. This may mean that a research organization will have a "peaked" or perhaps even a rectangular distribution depending upon the individuals making the research effort. Of course, this distribution can be affected by the type of individuals added to a research staff.

We now turn to the development of "what is" information that appears unrelated to a "what to do" question. We must recognize that individuals may become interested in some questions for no reason except intellectual curiosity. This type of interest may result in significant information in the long run. The question is one of how much of this should be encouraged or permitted. First, there do not appear to be many research resources currently being devoted to this type of work. I believe most agricultural economists' interest in "what is" information stems basically from some practical problem. However, to the extent that such interest does exist there are certain things that we can say about it. The most fruitful view of this problem to me was provided by an "Adventures of the Mind" article in the Saturday Evening Post article by Segle entitled "What Makes Basic Research Basic?"[8] Segle lists three characteristics of basic research:

1. Findings are true not as facts but in the way they are interpreted.
2. The findings can be generalized.
3. They are surprising in the light of what was known at the time of discovery.

If the above is accepted, we have some criteria for judging the relevance of so-called fundamental or basic research. In what way, assuming a hypothesis is proved or disproved, will this affect our conception of things? In some cases, regardless of how the research turns out, the findings will be trivial. By trivial I mean in terms of the basic discipline involved whether it be economics or statistics or some other field. (I assume we are not at this time considering its impact on economic decision-making.) So-called "fundamental" research can be judged, subjectively to be sure but nevertheless judged, in terms of what difference it would make if it is successful.

## Recent Development in Economics With Special Attention to Econometrics

At the present time, few people in agricultural economics will deny that the roots of our field of specialization lies in general economics. Almost without exception our graduate students take substantial work in general economics. It is therefore appropriate to make a few remarks with respect to

[8] Segle, H., Volume 231:30, January 24, 1959.

developments in general economics and to speculate regarding the impact these developments have on research in our field.

In recent years agricultural economists have been in the vanguard of developments in micro-theory. We have relied heavily on this body of knowledge and it is to our credit that the applications to agricultural problems on both a firm and industry basis has refined the theory in important respects.

Developments in macro-economics have been somewhat marginal in recent years. Yet I am not convinced that this body of theory has been applied as imaginatively in agricultural economics as has micro-economic theory. It may be that it has more application to problems of resource development and rural development than has been realized.

The literature on the economics of growth is, so far as I can tell, somewhat unmanageable at this time. A wide variety of approaches are being tried and many disciplines are contributing to the effort. One can find elaborate mathematical growth models on the one hand and sociological and institutional treaties on the other. One has the intuitive feeling that the mathematical models are assuming as exogenous those items which are of crucial importance in understanding the process of growth. On the other hand, the sociological and psychological literature does not appear yet to be highly operational from a research standpoint. Even so, it appears that we should attempt to keep abreast of this area. Not only do we need a framework for our work in other countries but we also need to know more about the growth of firms in our own country to say nothing of the development of rural areas.

Perhaps the area that has developed the most rapidly and which is only beginning to be exploited in agricultural economics is that of econometrics.

The techniques that have been developed here indicate that it is possible to make operational many of our theories and to supply answers to questions of fact. Instead of reasoning about the general shape of functions, it has become possible to estimate the function directly. In my opinion this has considerable application to problems of both group and individual decision-making. In the area of policy, it has the potential of narrowing the areas of disagreement. I do not believe we have fully exploited the application of these techniques and I hope that agricultural economics would continue to develop in this direction. Unless we do we will be "solving" problems by the least efficient means. This does not mean that everyone should become an econometrician. It does mean that we need to be sufficiently aware of the potentiality of these tools so that we do not use inappropriate techniques on a problem. The development of these tools opens up, rather than curtails, opportunities for the non-econometrician. One can select an activity for which he has the greatest comparative advantage and exploit that while leaving more advanced econometric work to the specialist in this field. This kind of specialization will need to be accompanied by a better level of general understanding of the potentiality of these tools. We need to give attention to achieving a rather wide intuitive understanding of these techniques for most professionals rather than having two groups--one highly trained, the other with little knowledge. This will throw some teaching responsibility on those who have acquired these tools. In my opinion, current teaching in this area is generally intended for the developer or user of these tools rather than for the person who needs an intuitive grasp of the subject particularly as it relates to his area of specialization.

Perhaps a word of caution on this point is in order. The "pay-off" from quantitative work will be determined by the adequacy of the underlying social theory. Therefore we have a responsibility in organizing graduate programs to take this into account. Otherwise quantitative tools will be applied to the same problems over and over by students who are well trained in quantitative techniques but who are not economists. If too narrowly trained, they will not see the possibilities of advances in social theory. I believe there is some evidence that this has occurred and that it is something that needs to be watched and controlled.

## Conclusions

The preceding material is general, vague, and tentative in nature. However, it does permit the drawing of some conclusions. The conclusions must also be tentative and should be viewed as hypothesis for subsequent testing. This testing may come from general observation or from the consequences of implementation.

1. It is appropriate that Departments of Agricultural Economics continue to concentrate research efforts on problems of both professional and popular interest. Effort should be exerted to select those practical problems whose solution rests on significant unanswered "what is" questions. We should minimize the answering of routine questions under research activity. However, this is an appropriate extension activity. As our field matures the number of people working on questions of popular interest only should increase. An analogy can be drawn here between the practicing physician and the medical researcher.
2. We should look carefully at the questions we have been answering on a routine basis to become more certain of the "what is" information on which our answers are based. For example, our outlook work is

based largely on intuition and judgment. Yet procedures do exist by which predictions can be made objectively. The results of both types of work can be subjected to rigorous ex post analysis. We should then be able to draw some conclusions as to the relative reliability of the two methods.

3. It appears that it would be logical to concentrate relatively more of our research resources in areas where the potential pay-off is substantial but avoid those problems at the extremes, i.e., where the pay-off is extremely large but where operational tools of analysis do not as yet exist, and routine problems mentioned above.
4. That the relevance of so-called "basic" research can be judged in light of its potential effect on the discipline involved.
5. That traditional specialization in agricultural economics has both advantages and disadvantages. If this specialization is retained for extension purposes it should be accompanied by specialization on the research side that will permit the researcher to work on the frontiers of the tools of analysis most appropriate to his type of activity.
6. Greater sophistication in quantitative techniques calls for a coordinate responsibility in keeping abreast of relevant social theory. It is believed that to this time the two have been complementary. We should work to keep them that way.