A QUARTER CENTURY OF AGRICULTURAL ECONOMICS IN RETROSPECT AND IN PROSPECT*

Lloyd C. Halvorson

INTRODUCTION

A philosopher once said that history should be studied because the past repeats itself, but I prefer the statement that those who do not sudy history are condemned to repeating it. I also like the quote at the National Archives: The past is "prologue." When a tourist asked what this meant, a Washington taxi driver said, "It means you ain't seen nothing yet." That was in 1950. He was right. Who in 1950 expected to see sputniks, satellites, and men walking on the moon?

WHITHER AGRICULTURAL ECONOMICS?

We entered the 1950-1975 era thinking that agricultural economics consisted of: (1) production economics, (2) marketing, (3) price analysis, (4) agricultural policy, and (5) land economics. We are leaving it thinking that it covers: (1) commercial agriculture, (2) natural resources development, and (3) human and community development.

In my opinion, only research for commercial agriculture should be defined as agricultural economics. Treating (1) natural resources development and (2) human and community development as part of the field will undermine economic research for our great agricultural industry. Natural resources research can pertain to commercial agriculture, but as time goes on it will increasingly pertain to nonagricultural resources and to recreational, residential, commercial, and industrial uses of rural lands and even suburban land.

Funds for natural resources and human and community development research were squeezed out of the budget share that historically had been devoted to economic research for commercial agriculture. This situation may well continue until the budget share for commercial agriculture is only one-third of what it has been. If this happens, economic research for commercial agriculture will be sparce, resulting in a serious decline in our rate of agricultural progress, a rise of monopolistic situations in agricultural markets, and loss of a cherished way of life and valued institutions. Furthermore, farms and agribusiness firms will operate at an efficiency level that adds hundreds of millions, if not billions, annually to the cost of food and fiber.

Present industrial, commercial, and residential trends indicate that in the future most rural areas may well have more people employed in nonagricultural than in agricultural enterprises. Also, the problems of many rural communities will be more related to industrial, commercial, and residential development than to agricultural enterprises. Certainly agricultural colleges have the expertise to be of substantial assistance to all rural people and to every rural community. There is, however, a question as to whether research on nonfarm rural problems and opportunities should be financed from the budget share that has, historically, gone largely to economics research for commercial agriculture; or whether a new budget share should be developed for nonfarm rural problems.

Past proposed changes in the names of our professional associations and in the names of agricultural college departments indicate that over time economics research in agricultural colleges

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has expanded to new areas. This is good, but, at the same time, we should make certain that this expansion does not undermine the support, ethos and cohesiveness of economics research for our agricultural industry as established earlier by agricultural economists.

It is possible that the only way agricultural economics, properly defined, can be preserved is by devoting a department solely to it. This would also mean separate departments for natural resources and for human and community development. Possibly the ideal would be a social science division, commensurate in size with plant and animal science divisions, with the three program areas discussed above within it, each with its own leadership. An organizational unit for the economics of commercial agriculture would greatly affect a professional's outlook, approach, and dedication to commercial agriculture.

FINANCIAL PROGRESS

I once heard a department head argue that his job was not too different from that of a salesman. He said he had to "sell" the research services of his staff to the station director, sometimes to legislators, and sometimes to the people of the State. As a result, he looked upon the size of his appropriation, or budget allocation, as a measure of his department's progress.

Total funds available to experiment stations rose from $60 million in 1950 to $414 million in 1974, a seven-fold increase [1]. No wonder people refer to this past era as the "Golden Age of Science."

By how much did budgets of agricultural economics departments increase in this period? By looking at old CSRS data, CRIS data, research by Robinson [2], Smith [3], and Nielson [4], and by doing some calculating myself, I concluded that budgets of agricultural economics departments (or their replacements) stayed very close to 6 percent of those experiment stations throughout the 1950-1975 era. Their budgets, then, also increased about seven-fold.

Far more of the increase in research funds was lost to inflation than the consumer price index would indicate, so staffs of agricultural economics departments at experiment stations only about doubled in this period.

While agricultural economics departments received only 6 percent of the experiment station funds, they accounted for more than 6 percent of station research staffs. Cost of a science man-year in the physical and biological sciences is roughly 50 percent more than in the social sciences. Therefore, economists make up close to 9 percent of station staffs.

It is paradoxical that in spite of the growing importance of economic problems in our society, and in spite of research legislation emphasizing the importance of socio-economic problems, budgets of agricultural economics departments have not increased relatively. They have been nevertheless forced, by the necessity of the times, to take on new problem areas outside of commercial agriculture.

I should point out that the 20 percent Hatch marketing requirement resulted in some grotesque distortions of research programs of agricultural economics departments. These could largely have been avoided had funds for farm management, land economics, and rural development been increased in line with needs. However, this would have meant a larger share of station funds for economics.

PROGRESS IN AGRICULTURAL ECONOMICS

How does one measure progress in agricultural economics? Just looking at size of our budgets or the number of Science Man Years in our field is not satisfying because it measures inputs, not outputs. We want to be sure we are not merely successful snake oil salesmen.

Because research findings in agricultural economics are perishable, I cannot put much weight on adding to the store of empirical findings as a measure of progress. Not only do magnitudes of the coefficients of our systems or phenomena change with time, but also the nature of variables operating in our systems. I believe we sometimes delude ourselves in calculating confidence limits on our coefficients as if our data, even complete enumeration data, were but a sample from a constant population spread over time.

If we are to know what really counts as progress in our science, we must look at the unique aspects of the subject of our research man and his institutions, and especially his relationship to the purpose of all science and the purpose of all creation.

Man is a real problem for science because each counts. Each man, community, and culture is unique. Man learns through time, so his responses change with time. Only man has free will and only he can choose his own destiny. How can science deal with such an unpredictable and self-controlling creature as man? Certainly it

does not make sense to study man as if he were but a fortunate combination of atoms, or as if the natural sciences' positivistic approach were the right orientation for social sciences.

Just because findings of positivistic research in the social sciences are perishable is no reason to conclude that a science of man is useless. What we really need to put first in the social sciences is normative science, or the science of optimization and the science of strategy, to help man achieve his goals and his chosen destiny. All science and research, then, should be linked to this, making social science integrative of all findings of all sciences.

Because the coefficients of our empirical models are perishable, progress in agricultural economics must be measured in terms of how useful the findings were to man in his decision making or his problem solving at the time. In the natural sciences, there is at least the hope that irrelevant findings will become relevant later. We in the social sciences do not share this luxury.

As I view empirical findings of our research during the past 25 years, I find that they have been both relevant and useful to society. I will mention the high points.

In agricultural policy research, Southern agricultural economists did outstanding work in evaluating the effectiveness of various means by which society was raising prices and income for cotton, tobacco, peanut, and rice producers, and creating a better understanding of undesirable side effects. Agricultural economists played a significant role in bringing about transferability of allotments and in opening the way for poundage instead of acreage allotments. Economic system coefficients were so quantified as to permit program administrators to more precisely and efficiently raise farm prices and income. While policy research was abundant and prestigious in the 1950s and early 1960s, it is now at low ebb.

In the reconversion of our agriculture from a war-time to a peace-time basis, and in transition from a labor intensive to a mechanized agriculture, Southern agricultural economists performed with diligence and brilliance; they pointed the way that sound farm adjustment should go. The Southern postwar adjustment project was the best conceived and best executed such project in the nation. Nowhere in the nation was the need for this kind of research so great.

Supply, demand, and price research in the South has struck me as being a reliable input into the farmer's price expectations as well as his guide to long and short run enterprise planning.

The greatest advance in all of agricultural economics took place, in my opinion, in farm management, in the calculus of optimization—namely in linear programming, quadratic programming, dynamic programming—and in the technique for handling risk and uncertainty in decision making. Unfortunately, we did not do enough to refine our knowledge of the production coefficients within which optimization should take place, nor of what farmers were really trying to optimize. We did not properly organize to make the benefits of our computer based optimization models generally available. Advances in ADP farm records systems were also real accomplishments in the past era.

In cotton marketing, great advances were made in understanding the relationship of physical proprieties of cotton to end-use performance, and in adjusting grades, standards, and the organization of the marketing system to it. Some research contributed to more efficient cotton ginning, to fewer fire losses, and to better ways of coping with them. Right now cotton marketing specialists are leading the way into understanding price reporting, price making, and price information systems under conditions of contract selling. Economists have also made progress in developing hedging strategies that farmers can use to meet the threat of bankruptcy that arises from high rigid production costs on the one side and erratic commodity prices on the other.

In grain marketing, economists have done outstanding research in indicating optimum location for grain marketing and in fostering price efficiency in grain markets.

In fruit and vegetable marketing, researchers have played a significant role in helping the industry identify consumer wants and preferences and in adjusting marketing practices accordingly. Now they are gaining an insight into structural change in the industry so it can be more efficiently organized.

No region has had such excellent research in the marketing of flowers, shrubs and horticulture specialties in general as the South. This has included identifying consumer wants and needs and reducing costs of marketing, especially transportation. Excellent research is now under way to identify future demands and to understand the role of public agencies in influencing use of ornamentals equality.

Milk marketing research is excellent in all regions with respect to relevance and quality of

findings. Because milk is a closely regulated commodity with state, regional, and national dimensions, findings in this area have been quickly used by public officials to improve program effectiveness and efficiency. In no commodity group have producers looked so directly to economists for strategy guidance. Also, the processing industry has benefited from economy of scale studies, spatial equilibrium models, and model plant studies.

The South is the leading poultry area. Here researchers have also performed well with outstanding plant efficiency studies.

In livestock marketing, economists have done good research on the relationship of physical and chemical properties of meat to palatability and grades and standards. Research has helped increase efficiency of auction markets and transportation of livestock. It has contributed to advances in market news services for livestock producers. More recently we have seen studies that indicate optimum size, number, and location of livestock and meat marketing facilities for the South. Some define how coordination between farmers, marketing agencies, processors, and even consumers can be improved. Right now we are seeing the beginning of a research effort to enhance production and market opportunities for forage fed beef in the South.

In land economics, a number of outstanding studies were made on land tenure instruments, changes in land tenure forms, intergeneration transfer of property, and the effect of government programs on land values. There was also some outstanding research on the effect of water development projects on land use and community development. We are now at the threshold of great developments in land use planning. The role of agricultural economists in recreational developments has been outstanding in some states to the extent that state agencies look considerably, if not primarily, to agricultural economics departments for guidance in planning location and nature of parks.

Agricultural economists are just now beginning to move into the economics of chemical use in our environment. This is a big new area.

Human resources research, particularly relating to the value of training, was slow in coming. Even now attention to the subject is spotty. Only a few stations have in-depth programs. Economics, as well as Sociology, has a big role to play here.

We are just now beginning to get meaningful research on how public services, cultural amenities, commercial services, the rural infrastructure, and the rural environment in general can be improved.

Rural development, in the sense of industrial development, has had slight attention throughout the past era but recent activity in his area is much increased.

The greatest advance in agricultural economics between 1950-1975 was in methodology, both normative and positivistic. This kind of knowledge is not perishable and has permanent value. While I am most impressed by the usefulness of optimizing models, I recognize the importance of advances in positivistic methodology—being able to identify and qualify relevant parameters of empirical systems and their mode of action and interaction as needed in the optimization calculus of our clientele.

Application of econometrics to empirical problems was greatly aided by computers. They represented a breakthrough of gigantic proportions for economists, who now find it feasible to develop empirical models of great size and sophistication. Use of such models in decision making, however, awaits developments in the next era.

In reflection, I believe that agricultural economists earned their keep with dividends to society several times greater than their cost. But this is not enough. Research growth is not assured by a benefit/cost ratio in excess of one; we must have a benefit/cost ratio higher than our sister disciplines and, in any case, as high as we can make it.

LIMITATIONS IN AGRICULTURAL ECONOMICS RESEARCH

The overriding limitation in agricultural economics research—perhaps I should say rural economics research—in the 1950-1975 era arose out of the legacy of the previous era as well as the budget constraints.

Agricultural economics research in the 1950s and early 1960s was dominated by the philosophy behind the Research and Marketing Act of 1946. The main impetus for this Act was fear by farmers and their leaders that there would be a postwar agricultural depression of unprecedented proportions. Farm output had expanded greatly during the war, so overcapacity was feared. Even economists were teaching the lessons of history—namely, that great wars are followed by great depressions. Farmers had hopes, some would say naive hopes, that promotion and utilization re-

search could solve the surplus and overcapacity problem.

Because of the 20 percent Hatch marketing requirement (arising from the Research and Marketing Act of 1946), many agricultural economics departments became predominantly marketing research departments and remained so into the late 1960s and even into the 1970s. It is my perspective that marketing research money was, on the whole, well employed, but because the budget share for agricultural economics did not grow, research for farm management, resource economics, human and community development suffered.

When distortion of agricultural economics research became painful enough, marketing money was given up in exchange for unearmarked money.

What overall psychology or outlook should have guided agricultural economics research in the postwar era? In my opinion, the greatest development in the 1950-1975 era was the technological revolution in agriculture and organization changes that took place in response to it. If instead of a depression neurosis we had had a growth outlook, we would have been more productive in helping rural America and all of society adjust to the agricultural-technological revolution.

For the next 25 years experiment stations should not proceed with any significant technological research without companion research on socio-economic implications and social and economic adjustments needed to accompany it. This is especially true of research reducing labor requirements in agriculture.

There was a failure to develop comprehensive and systematic approaches to the total research needs of our clientele in the 1950-1975 era, a second major research failure. A great deal of our research was ad libitum and ad hoc. We had excursions into our field of responsibility instead of planned approaches to cover it all in a balanced way relating to our clientele's needs.

Limitations in agricultural economics research are not all external. I would list internal limitations as follows: 1. Low priority and even irrelevant research; 2. failure to link positivistic research findings to decision models; 3. methodological overkill; 4. inadequate data; 5. loss of credibility with clients; 6. academic freedom overdone; 7. fragmentation and non-additivity; and 8. inadequate organizational structure and process for large economic problems.

Time and space do not permit elaboration of all these limitations. The main problem in agricultural economics today (rather than methodological overkill) often seems to be staffs of agricultural economics departments increasingly being made up of quantitative economic analysts who lack empirical knowledge of agriculture. A quantitative economic analyst tends to be an ad hoc researcher but, if he uses interaction with real world agents as well as research to gain an understanding of agricultural economic phenomena, he can become an outstanding agricultural economist.

RESEARCH CHALLENGES, 1975-2000

The External Situation

What kind of overall outlook or psychology should guide our approach for the next 25 years? We should not expect to see another Golden Age in research appropriations, though there may well be some overall growth in publicly supported research. At least for the next few years the world food situation will be more favorable to expansion in agricultural research than research generally. Already concern for food production has been reflected in a tight market for farm management and farm commodity marketing specialists.

Unfortunately, growth in natural resources economics and human and community development research will be stunted unless new sources of funds can be developed for them.

It will be important in our future research to take into account pervasive forces operating in our economy. The economist's job is often to anticipate change and to facilitate adjustment to it. However, he must not have a deterministic outlook; he should be able to indicate how undesirable trends can be changed.

Scientific industrialization of agriculture will proceed, but less rapidly than in the past; however, special public measures may need to be developed by researchers if differential access to low cost credit and advantages in buying and selling, that do not add to economic efficiency, are not to distort our agricultural structure.

It appears the birth rate in this country is falling to surprisingly low levels. I expect that our nation will not choose to support high birth rates abroad for long by food give-away programs. I doubt that hungry people of the world will ever gain much dollar purchasing power, so for these and other reasons agricultural surpluses will probably return as a serious problem.

This means that questions as to the role of government in agricultural policy, especially with respect to raising farm income, will be in the high

priority category again. Closely related to this will be a need for research on commodity reserves and other means of achieving price and income stability in agriculture. Some would add that future policy researchers will need to give more attention to food and nutrition needs of the nation than they have in the past.

Changes in the world's energy situation food conditions have many implications for the entire agricultural research community as well as for farmers. Considerable research into trends in international trade in farm products will be required to fully develop these implications. Along with this goes the need for research into foreign trade policies of the United States and countries abroad.

In marketing research, we will need to do a better job of studying marketing systems on an integrated regional and even a national basis, in evaluating market performance, and in suggesting ways of improving it. Attention to vertical coordination between stages and between firms in the production marketing chain should be increased. We still have a long way to go in improving grades and standards, in defining industry production functions, in improving efficiency of marketing firms, and in product promotion research. Our entire agricultural transportation industry is undergoing a restructuring. There is a critical need for research to guide state and federal agencies involved therein and for the industry itself.

In production economics, substantial refinements are needed in our knowledge of production functions, in keeping cost and returns budgets up-to-date, and in making the advantages of computer-based optimization models more generally available to farmers. We also lack knowledge of how individual farm firm adjustments must be tied into optimum industry or aggregative adjustments.

I do not expect real cost of energy to raise for long above the equivalent of $11 a barrel for crude oil because of substitutes for petroleum. Nevertheless, there is a great need for research to determine the long run impact, or better yet, the optimizing adjustments to higher prices for energy.

In the next 25 years, adjustment between agriculture and the rest of the economy will be minimal compared with the past, but adjustments within agriculture will be both substantial and difficult. However, there may be a tremendous amount of rural industrialization even to the extent of attracting people out of cities. Accommodating rural industrialization and residential development in an optimizing fashion will require considerable economics research.

We can expect a continued high level of interest in environmental quality, but a more rational approach to it with more demand for sound economic analysis.

The welfare function of society will be defined less in terms of GNP (monetary income), so social indicators and all aspects of the quality of life will need to receive more attention. Social costs and externalities of all public and private activities will receive more attention.

Land use planning and the general curtailment of individual property rights, in favor of social rights, will become far more prevalent. How best to internalize the external costs will need considerable research.

Organization and control of American agriculture and monopolistic situations in agribusiness industries will be a growing concern.

The cybernetics revolution is just getting started and will probably have more impact than further mechanical revolution. We have just scratched the surface in developing the computer's potential in all our public and private economic activities, especially in marketing.

Consumerism will have more impact than in the 1950s and 1960s on our research approach, but if we want a constituency that we can count on we will have to keep the interests of farmers, agribusiness, and rural residents uppermost in our minds.

We need a strong futurism approach in our research planning. I endorse the projections research of ERS and believe the state stations should be brought into this more fully. However, positivistic extrapolations are only half of the futurism approach. We also need to develop normative models of the future we want to achieve. From a comparison of the extrapolative model against the normative model we will know quite well what our research challenge really is—to help design strategies by which man can achieve his chosen destiny.

Not only will growth in research funds be slow in the future but there will also be tighter accountability for research funds. Research proposals may well be subjected to benefit/cost analysis and even social assessment of consequence of the findings. If we are certain that our research has a high marginal value product, we should welcome this development. Economists will undoubtedly play a role in applying social

accountability to all research projects.

Our Research Approach

In the future, we cannot assume we have done our job if we supply our clientele with a random assortment of pieces of positivistic knowledge obtained from ad hoc excursion research.

We will first need to gain a better understanding of our clientele's values and goals. Then, secondly, we will need to see if we can improve decision models or optimization calculus; next, we need to define what needs to be known about the real world systems and what particular ratios or empirical coefficients would be useful in the optimization calculus. Finally, we need to develop an integrated experiment station and USDA approach to keeping relevant coefficients of our production and marketing systems reasonably up-to-date over time.

I shall not in this paper indicate what I see as the high priority research needs of our clientele. Instead I urge that we develop appropriate organizational means and processes for interacting with our clientele for the purpose of identifying and defining its research needs.

There is the problem that for some of the greatest research needs there is a very weak and unorganized clientele. We agricultural economists must realize that we are the watchdogs of that part of our socio-economic structure affecting the welfare of our agricultural industry and rural America generally. Lay people are likely to take the institutional structure as given. While they may never see opportunities for improvements. In some instances we must develop our clientele.

If we do a good job of identifying only high priority research needs of commercial agriculture, it will be obvious that 6 percent of the station budget will be needed for this alone. Can we assure society a good return on its tax dollars invested in commercial agricultural research? We should try. If, in farm management research alone, we could bring about a $100 improvement in the net income of each of our 2.5 million farmers, the benefit to society would be $250 million dollars, more than three times the current Hatch appropriation.

COORDINATION OF AGRICULTURAL ECONOMICS RESEARCH

The best approach to bringing about coordination in agricultural economics research is first to establish a process for identifying all research needs for our clientele for all problem areas and in all geographical areas; secondly, to establish a process to decide who is to cover what; and thirdly, a process to decide what kind of a research organization at station regional and interregional levels is needed to bring about necessary research on an efficient and effective basis.

I do not have much faith in a research information system as a tool for research coordination. It might be able to prevent duplication of research and reduce overlap, but there is no substitute for joint preplanning of research on an integrated, comprehensive and systematic basis.

The present regional-national planning system with its regional task forces is certainly a right beginning but it is far from adequate. First of all, it does not provide enough opportunity for interaction with clientele, and it really does not provide a comprehensive and systematic approach to all economic research needs of our clientele The CRIS (Current Research Information System) research problem areas are not adequate framework for planning agricultural economics research. The planning system also has involved relatively few researchers. What we need is a broader-based planning process in which most researchers participate and from which ideas are carefully sifted and move upward into increasingly larger coordinated research plans.

In some regions now we have strongly organized regional research seminar groups in agricultural economics which develop good research plans, but their plans seem to have no integral place in the regional-national planning system. We could easily end up with a set of regional-national planning priorities that are ignored by researchers, or researchers might be forced to study problems which they feel and possibly could show are less important than problems they have identified.

We need many interregional projects in agricultural marketing if we are to properly describe and quantify the parameters of commodity marketing systems in their full national dimensions.

We should have executive directors with small core staffs for all large scale interregional and regional projects to provide cohesiveness and leadership in the research. There is already a North Central project with this arrangement.

There is need for an interregional or national committee to plan data banks and management information systems on a nationwide basis.

There are too many aspects of coordination to cover in this paper, but I do want to say that I see many needs for coordination of research also

at the individual researcher level, department level and station level. There is even need for individual researchers to do a better job of coordinating their own research. An individual researcher should plan his entire career to the full extent possible with attention to how his research today will coordinate with what he will do later on.

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