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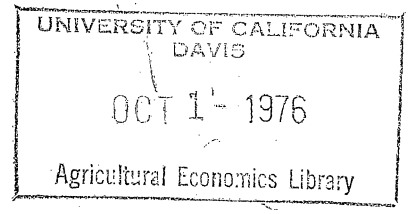
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Research

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RESEARCH NEEDS AND PRIORITIES IN THE FOOD SYSTEM:

AN INDUSTRY VIEWPOINT*

Richard T. Crowder**

To be given at a meeting of professional economists, the subject of this paper implies (1) a gap between the subject matter and emphasis of current research, (2) a lack of communications between those doing research and those in industry who are impatient for answers to constantly changing problems, or (3) both. And, while the nature of institutional research and the nature of economics training have been perennial themes of industry sessions at these meetings, increasingly there are similar challenges coming from professionals in academia and government.

Robert A. Gordon, in his presidential address to the American Economic Association, said, "...the mainstream of economic theory sacrifices far too much relevance in its pursuit of ever-increasing rigor." (p. 1.) His challenge to the profession was "relevance with as much rigor as possible, not rigor regardless of relevance." (p. 12.) This challenge is precisely the one industry economists have expressed for some time now.

There have also been similar challenges from the leaders of our own association. Jim Bonnen, in his presidential address last year, stated, "There is evidence that we are failing also to update our conceptual base

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at a pace sufficient to keep up with the major changes in agriculture."

(p. 756.) Carroll Bottum, in his Fellow's Address last year, stated,

"We need today in our universities, more individuals and groups with the ability and the courage to tackle fundamental issues facing U. S. citizens."

(p. 765) Jim Neilson, in his presidential address two years ago, said,

"In the past decade, I believe that we have overinvested in the development and refinement of quantitative methods. We have spent too little time and energy on discovering and tackling the emerging economic and social problems that most trouble our society." (p. 869.)

When only that minority of our profession who call themselves industry economists were questioning the relevance of the research of our profession, it could be dismissed as coming from a maverick group of our profession that perhaps did not appreciate the rigor of academic research. However, when it is brought into question by such professionals as those quoted above, it is time that all of us, as professionals, start listening. And, it is time for those of us in industry to insure that we are communicating our needs properly and effectively.

The objective of this session is to discuss research needs and priorities in the food system from an industry viewpoint. By no means would it be possible to delineate and rank all research needs of the food industry in a single session such as this. Each of us has our own endless list of needs and priorities. My objective is not to present a laundry list of needs ranked on some ordinal basis, but to discuss basic procedures and key issues as I see them. Specifically, I want to:

1. Comment on criteria for good research,
2. Identify some of the research needs in the food industry,
3. Comment on the extent to which the profession is meeting these needs,
4. And, finally, recommend measures to span the gap that has been identified in our profession.

Criteria for Research

Historically, research by the agricultural economics profession has made many significant contributions to the food industry. Despite this historical performance, there is a tendency for many in the profession to drift away from fundamental criteria in structuring and carrying out research. I think there are two reasons for this.

First, I think the professional rewards and training encourage an emphasis on rigor at the expense of relevance. Let's face it - the professional rewards and recognition of those who developed and expanded quantitative techniques have encouraged what Jim Neilson referred to as an overinvestment and refinement of such models. Without the professional rewards and recognition, the pursuit would not have been as intense as it has been. In graduate programs, the emphasis is definitely on rigor and with this training, the young professional, by nature, carries forward those things for which he has been duly rewarded in graduate school.

Secondly, I think the profession is slow to adjust to a changing environment. Therefore, research tends to lag the needs of decision makers, public

and private. Once a new concept or program receives professional recognition, there is a "bandwagon" effect that is difficult to slow. Example - there has been a major shift in the location of cattle feeding and beef slaughtering in the U. S. With the development and popularization of spatial equilibrium models and with a shift occurring in cattle feeding, a plethora of "optimum location of cattle feeding" studies were made. With the major shifts behind us, we are still seeing studies on the optimum location of cattle feeding. While we are after-the-fact studying shifts in the location of cattle feeding, there has been a major shift taking place in the meat industry that has yet to be studied properly. This is the shift to centralized breaking of cattle. The meat packing and retail industries have been in the midst of this change without any broad-based support from academic research. It is this type of inconsistency that results in the questioning of the relevancy of our research.

To overcome this problem, I recommend that the following criteria be applied to all research:

1. The research must be problem oriented. This does not mean that we do not need theoretical as well as applied research. Quite the contrary. We need both. New problems require new tools and methods of analysis. As the system changes, we need new theories to explain the operation of the system. Problem-oriented research is research that will improve the decision making process or will improve the methods of observation, measurement, and interpretation of data that should be used in decision making, public or private. It is not applying a popular quantitative technique to obsolete data.

2. The problem must be real. Research should not be done on a problem that does not exist. I was recently told of a potential study on the location of new lamb slaughtering plants. Lamb slaughter in the U. S. has been declining for 15 years; and, in the past year, I learned of four lamb slaughtering operations that have been closed. To me, the real problem is one of lamb supply and demand and logically research should be directed to these areas and not on the location of new plants.

3. Assumptions must be realistic and explicit. Two of the most frequent weaknesses of research are: (1) assumptions are not stated explicitly and consequently a potential user does not know if or how the results apply to his particular problem, or (2) the problem is many times assumed away.

4. The research results must be usable. There are three elements here:
 - a. The proper technique must be applied to the problem.
 - b. Reliable data must be available on a timely and repetitive basis.
 - c. The results must be updatable as conditions change.

5. Finally, the results must be timely. The results of the research must be available for use before the decision requiring the results has to be made. A time objective should be built into every research project.

Industry Orientation

To put in perspective the comments that follow in research needs and priorities, I think it is important to discuss the industry orientation to research. While there is variability in research emphasis from company to company and, to some extent, from industry to industry, the orientation is toward one goal -- better decision making. And, in a firm, most decisions are based on expected supply, demand, price, and policy variables. Consequently, our greatest need is in assessing the future patterns of product demand, input costs, product and input prices and in the policy decisions that influence each of the others. In addition, we concentrate on applied, short-term problems rather than theoretical and long-term problems.

Because of this orientation and the fact that most company economic research groups are not staffed to be self contained, industry is dependent on and looks to university and government researchers to fill three basic needs -- theoretical research, long-term research, and well-trained people. Or, to put it another way, we look to the public researchers to supply those needs that span company and industry boundaries.

Theoretical research: Despite our persistent call for applied and relevant research, we, in industry, must face the fact that we do need theoretical research and new techniques and methods of analysis. We do use quantitative techniques in our research that were developed in academia. We do use the economic theories that were formulated by the theorist as a starting point for our analyses. If we reflect properly on the source of our frustrations, we will find that it is not necessarily the rigor of theoretical research but the

fact that while theory and technique are lagging our needs, the academic community is applying already-developed theory and rigor to problems that do not exist or at least are not involved in our decision making needs.

Long-term research: The time demands of those in industry for short-term answers are such that we do little long-term "macro" type research. Our focus is on short-term, company or industry problems. However, for many strategic decisions we need an evaluation of the long-term growth and shifts in the food industry. For example, plant location studies require a projection of long-term availability and location of raw materials. As a basis for such a projection, we evaluate production trends based on such "public" studies as optimum farm organizations and interregional studies. We also use analyses of long-term shifts in product demand and population shifts. Economies of scale dictate that such studies be done at the academic level for use across industries. And, many of us in industry have made good use of such analyses.

People: No output from the academic or government system is more important to us than well-trained people. And, while the subject of training has been discussed at two of these meetings in recent years, I want to make one point. The primary deficiency of those we interview is a lack of understanding of the decision making process and how economic research relates to it. I cannot help but believe that this stems, in part, from the orientation of the research being done at the graduate level.

With this background that points out our orientation to research as well as our dependency on academic research, I now want to discuss needs and priorities as I see them.

Needs and Priorities

When we teach economic theory, we separate static and dynamic economic analysis. Unfortunately, many of our research efforts do not anticipate and, in many cases, do not keep up with the dynamics of our environment. I think this has been particularly true during the past four to five years. The environment just passed us by. Industry has been faced with a decision-making environment that it has not seen before and has had no where to turn for help. While everyone has his own priorities and an endless list of needs, there are three areas I want to discuss: risk analysis, market analysis, and policy.

Risk Analysis

For years, with the exception of that part of production economics that made appropriate distinctions between risk and uncertainty and calculated variances and covariances among returns from alternative farm enterprises, the primary emphasis of risk analysis and management was in using the futures market in inventory management. And, in this case, the teaching was that futures markets were reserved for commodities possessing certain characteristics, one being that the commodity must be storable. Of course, the market, and not our research, has long since made these concepts obsolete and ineffective.

This obsolescence was brought about by a drastic change in our operating environment. We went from an environment of surplus commodities and stable prices to one of low stocks and volatile prices. With the exception

of some commodity support prices and supply controls, we went from a relatively free market to one of price and margin controls. We went from a market where exports were promoted to one where exports were controlled. We went from a market where the Soviet Union was not a factor to one where it is a swing factor. We went from a market where farmers had to concern themselves with not only cost minimization but also with the marketing of their crops and livestock. And, we went from a market where procurement in the food industry changed from primarily a physical process to an economic process.

In such an environment, new needs developed for (1) measuring risk, (2) determining acceptable risk levels for a particular business, and (3) management tools for managing risk within the constraints of the first two needs.

Obviously, we, as a profession, did not offer to producers, processors, financiers and distributors of agricultural commodities the decision tools needed to manage in such an environment - particularly the tools needed for measuring risk and determining acceptable levels of risk. Our abstract concepts of risk averters and risk lovers are not sufficient for an individual firm. The individual firm must make specific, quantitative decisions concerning risk levels. If we had provided the appropriate tools, we would not have seen the severe financial losses and, in some cases, bankruptcy in the food industry. We would not have seen, and still see, major corporations reporting severe financial downturns as a result of volatile commodity markets.

What went wrong? What can we do to avoid repeats? In terms of what went wrong, I think our traditional models and thinking with respect to the application of those models quickly became outdated. In terms of what can be done to avoid repeats, I think we need to expand our concepts of risk and risk management.

At Pillsbury, we have broken our risk down into three categories according to the nature of the business. These categories are:

1. Risk associated with product held for inventory or merchandising.
2. Risk associated with the erosion of finished product margins.
3. Risk associated with physical availability of the commodity.

The management of each risk varies. For the first type of risk, traditional hedging or cross-hedging models are used. For the second type of risk, we have developed a modified game theory model illustrated in Figure 1. In this model, we attempt to assess the outcomes of alternative procurement and product inventory strategies, given alternative market outcomes and strategies by competition. Procurement decisions are based on the probabilities and magnitudes of the gains and losses of each strategy.

While the model is presented in simplified form, there is a broad spectrum of inputs that go into the model. Inputs to the model include the likelihood of alternative competitor strategies and the likelihood of alternative market directions. In turn, the likelihood of alternative market directions

FIGURE 1. PROCUREMENT STRATEGY MODEL

<u>COMPETITOR STRATEGY</u>	<u>COMPANY STRATEGY AND MARKET OUTCOME*</u>			
	<u>MARKET UP</u>		<u>MARKET DOWN</u>	
	<u>LONG</u>	<u>PDS</u>	<u>LONG</u>	<u>PDS</u>
LONG	G	LL	L	GG
PDS	GG	L	LL	G

* PDS - PRICE DATE OF SHIPMENT

G - GAIN AS A RESULT OF STRATEGY

L - LOSS AS A RESULT OF STRATEGY

is a function of our assessment of government policy, weather, foreign demand (particularly the level and timing of Soviet demand) and the standard errors of our forecasts.

In managing risk, we have a specific dollar level of risk that we are willing to assume. And, we have specific procedures for measuring the risk that we are willing to assume. Our measurement of risk is based on historical price levels, absolute price levels and the errors associated with our forecasts. The error probabilities associated with our forecasts are based upon our forecasting track record and not from standard errors of models we estimate. Four years ago I argued before these meetings that forecasting track records (the difference between actual price and the forecasted price) and not standard errors of equations should be the basis for assessing the probabilities of alternative price ranges. (Crowder, pp. 781-782.) Based on articles since then, obviously I did not make a strong impression.

While our model and methodology needs refinement, it is better than traditional approaches to the problem. With the increase in risk associated with weather, policy and variation in foreign and domestic demand, there is an increasing demand for improved risk management models by decision makers and I think it is a challenge to the profession to develop models that are not only theoretically sound, but are also models that can be understood and applied by everyone in the food industry from producer to retailer, including the agricultural policy makers.

Market Analysis

Because agricultural economists have been making "market" analyses for years, we might easily overlook this as an area for research emphasis. However, our track record and our current reputation in this area leaves something to be desired. Our poor performance in market analysis over the past four to five years implies that we have spent too much time refining our quantitative techniques and too little time on analyzing changes and structural shifts in the food industry.

Historically, our "market" analyses have been price and quantity oriented. Price analyses have been made on the basis of a given supply, prices or supplies of competing products and a demand shift variable such as income. Supply analyses have primarily been cobweb analyses - current supplies are a function of lagged prices.

Based on the work we have done, these simplistic descriptions of the system analyzed with complex quantitative models are not nearly as effective in helping the decision making process as are simplistic models that properly reflect the market system and process. Let me illustrate with a simple model.

As major users of boneless beef, one of the major inputs to our procurement strategy is the cow kill during a year. Based upon an analysis of the decision making process in the beef industry, we know that the number of cows killed in a given year is not a function of last year's or this year's

cow prices but of the current and historical profitability of the cow-calf enterprise and of the number of cows available for slaughter.

Figure 2 depicts the model we use to forecast cow slaughter in a given year. The percentage of the January 1 cows slaughtered during the year is a function of current and lagged profits to cow-calf operators.

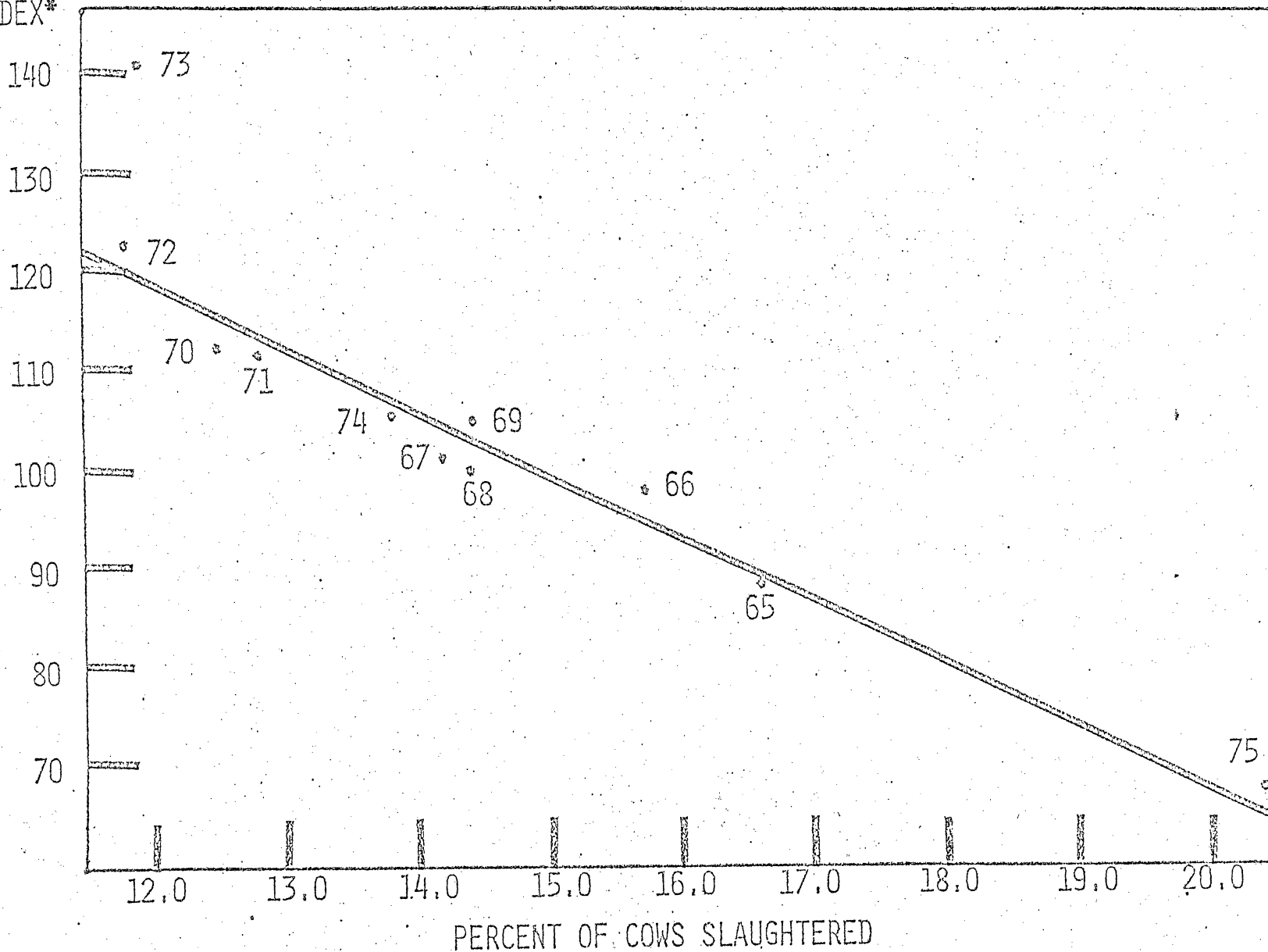
There are four important points about the model. First, it reflects the decision making process of the industry. The investment recovery in cow-calf operations is over an extended period. Therefore, the decision to keep or retain cows is a function of not only current profits but also of those that were accumulated in previous years.^{1/} Second, the model can be estimated by forecasting only one variable - this year's profitability. Third, the model is simple but yet describes the system. Finally, the model is easily understood and accepted by non-economists.

Note that the model uses profits, not prices. With the increases in levels and volatility of prices in the past several years, we have turned increasingly away from price variables to profit and dollar expenditure variables in our analyses. For example, the dollars cattle feeders are willing and able to spend for feed grains is a function not so much of the price of the feed grain, but of the dollars (profits) they have to spend for feed grain. Demand for a given item represents not only an ability but

^{1/} Additions to or liquidation of the cow herd is also a function of expected profits. With no explicit "expectations" variable in the model, the implicit assumption is that "expectations" are also a function of current and lagged profitability.

PROFIT INDEX*

FIGURE 2 PERCENT OF JANUARY 1 COWS SLAUGHTER & PROFIT INDEX, 1965-1975



* COMPUTED ON CURRENT AND LAGGED PROFITS

a willingness to pay for a given item. Ability is determined by the profits (income in case of the consumer) in hand or the credit available. The willingness is determined not only by current and expected profits (income) but by past profits (income).

From supply and expenditure levels, prices can be estimated. A theoretical problem with the approach is the variability of elasticities at different quantity levels. However, we have not found the theoretical problem here to be greater than the practical success resulting from an adequate description of the system. A real challenge to our profession is to determine the proper theoretical and practical basis for analyzing market forces and outcomes in the food industry.

Not only have many of the fundamentals of the cash markets eluded us, but so also have those of the futures markets. The trading of non-storable commodities caught the profession completely by surprise and to date I have yet to see an acceptable theoretical or practical analysis of the live cattle or live hog futures markets. Neither have I seen any research designed to improve the management of converting margins through the simultaneous use of live cattle, feeder cattle, soybean meal, and corn futures. It would seem logical that these four futures markets used properly would be a vehicle through which the cattle industry could transfer market risk to speculators. I suspect that part of the reason we have not seen more work in this area stems from the complexity of managing the four price bases involved, and that many do not understand the nature of or function of a basis.^{2/}

^{2/} Basis is defined as the difference between cash and futures price. In addition, there are geographical bases (price differences among geographical areas) that must be managed.

In a talk to a seminar on futures markets sponsored by the Chicago Board of Trade, Dan Amstutz, President of Cargill Investors Services, Inc., said, "It has become increasingly clear to me over the past year that many, too many, do not understand basis. Understanding basis is fundamental to understanding futures and hedging. Your acceptance of this axiom should provide ample incentive to understand work in this area." In our teaching of futures courses, we have traditionally taught the mechanics of transferring risk by hedging where hedging is defined as the sale (purchase) of a cash position offset by the purchase (sale) of a futures position. We ignored the risk involved in the ownership or the sale of the basis. As all who have had basis positions know, there is risk in a basis position and it must be managed just as the risk involved in a futures or a cash position.

With increasing price volatility, with an increasing need to manage risk with new futures markets offering additional market alternatives, and with important rules governing commodity marketing trading being made by a new regulatory agency, the need and the professional incentive should exist to improve and intensify our research and education in understanding the function and mechanics of futures trading. Outside of a few people, such as Roger Gray, our profession has neglected a research area that has the potential for a large pay-off.

In his comments, Amstutz stated, "I am totally unconcerned about the possible demise of the futures industry as a result in a 'breakthrough' in accurate, reliable, quantitative forecasting techniques." While I agree with a

literal interpretation of this statement, realistic quantitative models are a useful vehicle for improving decision making. Our challenge is to spend more time in understanding market systems and less in developing more complex and sophisticated quantitative models.

Policy

Of all the disappointments many of us have had with respect to our profession during the past five years, none has been greater than the lack of evaluation of key economic policy decisions. The response of the profession was silence during the many market interventions of recent years and our analyses have been on the premise that the price volatility during this period was due to market forces alone. However, I would argue that in addition to an examination of policy options for unstable prices, we need an analysis of policy as a source of price instability.

It would be easy to dismiss the price controls that were imposed as a one-time phenomenon that is unlikely to recur. However, the sensitivity of consumers and political leaders to inflation suggests that we cannot summarily dismiss them as a one-time phenomenon. Neither does the fact that we have had export controls applied and taken off again a couple of times since price controls were first applied. Therefore, a key issue is an assessment of the costs of the distortions caused by such market interventions. We need, for example, an assessment of the costs to consumers and livestock producers of the market intervention we have had. Perhaps, more importantly, we should have an analysis of how the need or

apparent need for such interventions can be avoided. And, finally, we need to communicate effectively the results to key policy makers.

While this paper is not the place for such an analysis, let me examine one example of such costs. Marketing patterns of the beef industry were severely disrupted by the price controls of 1973. During controls, cattle were inventoried in feedlots and on ranches. Then, when price controls were removed, beef came to the market at rates faster than the market could absorb. Returns to cattle feeders plummeted. So, in turn, did the feedlot demand for feed grains and feeder cattle. The result was severe losses on the part of cattle feeders and cow/calf operators.

Hasbargen and Egertson recently published estimates that the losses over feed costs for cattle feeding and cow herds were \$21.16/cwt. and \$139/head respectively in 1974 (Hasbargen and Egertson, p. 5). This compares with losses of \$6.50 per cwt. and \$39 per head respectively in 1953. Taking the difference in losses between the two periods and using the number of fed cattle marketings in 1974 and the January 1, 1974 cow inventory, results in a loss of over \$7.0 billion to the cattle industry, that, in my opinion, can be attributed to the effects of economic policy.^{3/} The consequences of this are predictable. We are having the largest liquidation of the cow herd that we have had since the 1930's. This effectively reduces our ability to produce beef.

^{3/} It can be argued that in 1973 returns to cattle feeders were higher than normal because of the artificially high prices during the "shortage." However, if one adjusted for this based on volume differences, the losses would still be massive.

The net result of the price controls was sharply higher prices in the short run, abnormally high losses for beef producers in the intermediate period and higher beef prices for the consumer in the long run. The beef industry is not an isolated case. There has been variability in returns to pork and poultry producers during this same period. And subsequent to the price controls, the variability of livestock producers' cost and returns as well as the returns to grain producers have been exaggerated by the on-again/off-again export controls.

My point is this. Economic policy in recent years has been a source of instability and costs; and, consequently, has contributed to rather than reducing inflation. As Carroll Bottom pointed out in his lecture last year-"...if there is more than one choice, and there usually is, the procedure is to identify and delineate the problem, develop possible solutions, set forth the consequences of each solution and make the decision." We, in my opinion, have not only overlooked part of the problem - economic policy as a source of instability - but we have and still are failing to analyze and set forth the consequences of alternative solutions. Price stability will remain an objective of those in public decision-making positions, if not among economists. However, with an ever-increasing demand for food and an ever-increasing interdependence in the world, it will remain an elusive objective. Therefore, it is our responsibility as economists to set forth the economic consequences of alternative solutions, particularly the artificial controls that create costly dislocations throughout the system.

Comments on Current Research

In summary, we are not and would not ask our profession to abandon rigorous, theoretical analyses. Rigorous, theoretical analyses designed to provide a better theoretical understanding of our food industry and/or to provide better tools of measurement and analysis are needed. Our concern is that we expend our energies on rigorous analyses of obsolete data or on problems that do not exist. Let me illustrate with the following "do" and "do not" examples.

Do give us an objective evaluation of the economic consequences of changes in beef grades. Do not give us an analysis of where new lamb slaughtering plants should be located.

Do give us an evaluation of central cutting and packaging of beef. Don't give us another study on the optimum location of cattle feeding.

Do give us an analysis of the nature and extent of shifts in demand for our product. Don't flood us with explanations of how coefficients vary from two-stage to three-stage least squares.

Do give us research that is current and useful in today's decisions. Don't give us an analysis of a decision we had to make yesterday and will not have to make tomorrow.

Do tackle key policy issues. Don't delay analyses until after the policy decisions have been made.

Industry Responsibilities

The responsibility for the gap that exists between the type of research that is being done and that we in industry would like is not entirely the responsibility of our colleagues in academia and government. Communication is a two-way street. Our approach of using these meetings as a once-a-year forum to vent our frustrations is not satisfactory. Our requirements and the contribution our academic and government colleagues can make are too great for a once-a-year exchange. Therefore, there are do's and do not's for those in industry also.

Do communicate problems immediately. Do not rely on the AAEEA meetings as the only medium of communication.

Do take the initiative in establishing a meaningful dialogue with your academic counterparts. Do not expect those in our universities and government counterparts to understand our needs if we have not discussed our needs with them.

Do read the "rigorous" articles in the Journal and elsewhere with the thought of gaining new ideas or techniques that could be useful to you.

Do not put the Journal aside because the articles do not provide specific answers on particular questions that you have.

Do share new ideas or techniques with academic counterparts. Do not cloak your research in secrecy. In industry, implementation of an idea is as important as the idea itself.

Recommendations

While there are those of us in industry who, at times, threaten to abandon the Association, I think there are more positive approaches and recommend the following.

First, since research efforts and funds flow to those areas that receive professional recognition, I think the Association should appoint a committee, including industry representatives, that would select for an award the article in the Journal or elsewhere that makes the most significant contribution to the industry decision-making process. This award would be in addition to the outstanding Journal article that is now made.

Secondly, I think that the Association should set aside a section of the Journal for short, applied articles to be written by those in industry or in other decision-making roles. This would be a vehicle of communicating to the profession key areas of concern to decision makers. Of course, industry economists would have the responsibility of writing such articles.

Third, I would encourage academic institutions and government agencies to have industry participation in evaluating academic, professional and research programs. This is done now by many business schools, and I think it is a prime reason for the success of MBA's in the corporate environment.

Finally, I think there should be Association-sponsored seminars in which there would be an industry-academic exchange of problems and methodology.

Our Association is rich in professional talent and technical expertise. Our number one problem is identifying common problem areas and ordering priorities. This can be overcome only through effective communication. Hopefully, implementation of the above recommendations will provide a greater degree of continuity in the dialogue between the industry and academic economists in our Association. History has demonstrated that these once-a-year critiques are not satisfactory.

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