IMPACT ASSESSMENT DISCUSSION PAPER NO. 9

RETURNS TO POLICY-RELATED SOCIAL SCIENCE RESEARCH IN AGRICULTURE

Bruce L. Gardner

Director General s Office

International Food Policy Research Institute
2033 K Street, N.W.
Washington, D.C. 20006
Tel: (202) 862-5600
Fax: (202) 467-4439
Email: IFPRI@CGIAR.ORG

May 1999

Little is known about the impact of social science research in general, and food policy research, in particular. In order to expand the scope of available academic research and to develop quantitative methods for estimating the impact of IFPRI's work, several papers were commissioned from social scientists. Furthermore, IFPRI held an essay contest to solicit research from a broader range of scientists. The resulting papers were discussed at a two-day symposium organized by IFPRI in 1997. This Discussion Paper is a revised version of a paper prepared for and discussed at the symposium. Other papers will be published in this Discussion Paper series over the next months.

CONTENTS

ABSTRACT

Policy research is valuable as a source of information for decisionmakers. The value of research is the expected social gain from policy decisions influenced by the information generated. The gain from a decision depends on choosing the best policy given the state of the world, which is uncertain. The output of policy research is a conclusion about that state. Taking a Bayesian approach, the ex ante value of a research program is estimated from information about decisionmakers' prior probabilities of the state of the world and the likelihood of correct and incorrect research findings.

Case studies of U.S. agricultural policy research cover consequences of trade liberalization, farm commodity program analysis, effects of publicly supported commodity promotion, and the value of publicly funded crop forecasting and research on agricultural technology. The quantification of the value of these research programs is highly conjectural, but consideration of their likely magnitude, as compared to the costs of the research, suggests that there are substantial net social gains.

INTRODUCTION

Measuring the returns to social science research is a task so daunting that it might be best to avoid even attempting it, except for one fact: decisions have to be made about funding such research. As a former U.S. government official in charge of arguing the merits of budget proposals for the Economic Research Service of the U.S. Department of Agriculture, the author felt keenly the need for reasonably objective criteria for evaluating the program proposed. This need was never satisfied. Nonetheless, budgetary arguments were laid out and decisions made. Some lessons from this activity will be discussed later. The paper will begin by going to first principles to consider how a systematic evaluation of the merits of policy-related research might be organized conceptually. Then it will examine several cases. Unfortunately, these can be only feebly quantified. The evidence available indicates that the returns to policy-related research in agricultural economics have been larger than the costs of the research, most likely by a substantial amount.

ANALYTICAL METHODS FOR MEASURING RETURNS TO POLICY RESEARCH

The valuation of policy research requires estimation of the difference the research makes in peoples' actions and its value. Most of the actions involved are political. Political actions are those of policymakers in government. They are also lobbying actions of private citizens seeking to influence government. For example, evaluations of the returns from publicly funded research on agricultural production influence both decisions about public spending on agricultural research and lobbying actions by producer or consumer groups for or against such spending.

Policy Research as Information for Decisionmakers Facing Uncertainty

Policy research is an intermediate product, an input into a political decision. It is helpful to think of the product of policy research as information. Following Hirshleifer and Riley (1992, Chapter 5) information is not a stock of certain knowledge, but a flow or increment of "news" or "messages" of uncertain reliability about a state of affairs that is itself uncertain. This characterization of the output of policy research lends itself naturally to a treatment of policy actors as Bayesian decisionmakers under uncertainty. Lindner (1987) takes a similar view of agricultural economics research generally, whether policy-directed or not.

To see how this characterization works analytically, Figure 1 uses as an example a two-state world. State 1, S_1, is a world in which export demand for a commodity is inelastic. State 2, S_2, is a world in which export demand for that same commodity is elastic. It is not known which of these two states the world is in. A goal for a research program is to determine which state prevails.

Two policy actions are possible: A_1, an acreage control program; and A_2, a production subsidy program. The value of any policy outcome is measured by $V(i, j)$ where i is the state of the world and j is the policy chosen. In Hirshleifer and Riley, as in decision theory generally, V is utility, but for this paper V is a weighted sum of benefits to producers and consumer-taxpayers. There are four possible outcomes, shown as points M_1, N_1, M_2, and N_2. If policy A_1 is chosen, the result is point M_1 if demand is inelastic and M_2 if it is elastic. If A_2 is chosen, the result is N_1 if demand is inelastic and N_2 if it is elastic. The policymaker will regret choosing A_1 if demand is elastic, and A_2 if demand is inelastic. The horizontal dimension of Figure 1 measures subjective probabilities, π_2, of S_2 (increasing left to right from 0 to 1)—and this implies π_1 since $\pi_1 + \pi_2 = 1$. Suppose there is maximum uncertainty about S_1 and S_2 in the sense that either outcome could occur with equal probability: $\pi_1 = \pi_2 = 0.5$. The expected value of V can then be maximized by choosing policy A_1, giving expected V at point H.

Now consider the value of a research program to estimate the elasticity of export demand. The research program will conclude that demand is either inelastic or elastic, but it may be incorrect. Suppose the uncertainties are as given by the following probabilities:

	Research findings	
True state	Inelastic	Elastic
S_1: Inelastic	0.8	0.2
S_2: Elastic	0.3	0.7

If demand is inelastic in reality, there is an 80 percent probability that the research will correctly obtain this finding. But there is a 20 percent probability the research will incorrectly say that demand is elastic. It is slightly harder to detect that the true state is elastic—the research program will give the correct result with a 70 percent probability and the incorrect result with a 30 percent probability. These likelihoods provide us with an operational measure of research quality—the probability that the research findings are accurate.

If Bayes' theorem is used to calculate the posterior probabilities, and the assumption is made that it would be equally likely beforehand that demand would be elastic or inelastic (that is, probability is 0.5 for each state), the following posterior probabilities would result:[1]

	Research findings	
True state	Inelastic	Elastic
S_1: Inelastic	0.7	0.2
S_2: Elastic	0.3	0.8

In Figure 1, if the finding were that demand was inelastic, then $\pi_2 = 0.3$, and $A1$ would be chosen with expected benefits at R. If the finding were that demand was elastic, then $\pi_2 = 0.8$ and the choice would be A_2 with expected benefits at point T. So the research makes no difference in the choice in the first case, but it causes a reversal of course in the second.

What is the ex ante value of the research program? Assuming that each finding was equally likely before the research program was undertaken, the expected value of V is the mean of R and T, which is plotted at point G. The value of the research program is the distance G-H (the length of the double-headed arrow).

The two-state Bayesian model is simple but nonetheless helpful in pinpointing the determinants of the value of research. In particular, three elements determine the value of the research. First, the value of acting upon the information the research provides, if the information is correct. (This is $M_1 - N_1$ if the research finds S_1, and $N_2 - M_2$ if the research finds S_2.) Second, prior knowledge about the subject of the research. (In Figure 1, the prior probabilities of 0.5 for each state represent the absence of prior knowledge). Third, the quality of the research, as measured by the likelihood that research findings are correct. (In Figure 1, this is the likelihood matrix that places us at point R or T.)

The significance of the model is, first, that these three elements are the only aspects of the research situation that matter, and second, that the model shows how to put these elements together to calculate a dollar value for the ex ante value of the research. Moreover, the model helps confirm which common-sense intuitions about the value of research are most likely to make a difference. For example, the following propositions can be obtained straightforwardly from the model:

- If we already know the state of the world, research is valueless. Proof: "knowledge" means $\pi = 0$ or $\pi = 1$. Applying the model using these prior probabilities always generates the same posterior probabilities (0 or 1). Therefore, the policy choice cannot be influenced by research and there is no opportunity for social gain.

- The research is valueless if it does not reveal information about the true state of the world. Proof: No information means the likelihood of each of the two states is 0.5, regardless of the research findings. Applying the model with this likelihood matrix means that the posterior probabilities are the same as the prior probabilities, so that R and T in Figure 1 cannot differ from their pre-research position (someone who started at point H, would end up at point H.)

- The research is valueless if the same policy is preferred for all states of the world the research considers. Proof: In Figure 1, this situation would be represented by having both $N_2 > M_2$ and $M_1 > N_1$ (the inequalities could be reversed). A_1 (or A_2) would then be chosen regardless of the location of H, R, or T. So there would be no gain from the research.

- The ex ante value of research is nonnegative. This is a less obvious result. It is true that some findings (or "messages") have no value: in the export demand elasticity example, a finding of inelastic demand would make no difference in any action taken. But even so the research program — the "message service," in Hirshleifer and Riley's terminology — has some value as long as it reduces uncertainty about the world sufficiently. In Figure 1, π would increase to about 0.6, where N_1N_2 and M_1M_2 cross. Even uninformative research — research that delivers no news, that leaves the posterior probabilities the same as the prior probabilities — has value no less than zero because it makes no difference in action taken.[2]

In contrast to this result, analyses of crop information, as in Lave (1963) or Bullock (1976), show that improved information can make its users (producers) worse off. In the cases used by Lave and Bullock, this occurs because producers react to short-crop information by making prices lower than they would be without the information. No single producer generates these prices, but their joint response does.[3] More generally, a problem is that the direct users of information make up only part of an economy. Bradford and Kelejian (1978) provide a more complete evaluation of the welfare of crop forecasting information with Bayesian market participants. In their model the social value of improved information is nonnegative (though of course the additional value may be less than the cost of providing the additional information).

The approach used by Hirshleifer/Riley (or Bradford/Kelejian) is relevant for an evaluation of the information generated by policy research. They assume that policymakers take into account all the effects of what they do and can use the research or not as they see fit.[4] Nonetheless, this approach, with its ex ante consideration of the research program in the Bayesian context, has demanding requirements for information, as Graham-Tomasi (1984) notes with reference to papers by Chavas and Pope (1984) and Antonovitz and Roe (1984) on producers' use of information under uncertainty. One needs not only the probabilities of true states of the world given researchers' estimates, but also the ex ante probabilities of the accuracy of researchers' findings. It is necessary to assume that policymakers can carry out a professional evaluation of the research findings they use, at least sufficiently to determine their expected value. This is largely what their professional staffs are for.

The Hirshleifer/Riley framework also suggests that research can be valuable simply by reducing the range of uncertainty about the state of the world, no matter what point estimate is discovered. In Figure 1, increased evidence moves the user toward one state or the other (the probability of state 2 gets nearer 0 or 1). Whichever way the research program takes the user, its ex ante value increases as points R and T are pushed farther apart. This will be true so long as these are policy options better suited to some states of nature than to others. If the policies generate the same utility no matter what the world is like, then there would be no point in using policy research to determine the state of the world, according to the proposition that research is valueless if the same policy is preferred for all states.

Quantifying the Gains: National Income

The effect of policy research on political choices will be referred to as the action-difference caused by the research. It is a measure of the output of policy research. The value of this output will often be difficult to measure. The approach used in this paper is grounded in the idea that the value of the research is the well-being generated by the action-difference. Welfare economics offers a rigorous theoretical basis for some judgments about well-being, using compensation principles. However, this body of theory is not fully applicable even in well-worn areas such as the use of GDP as a measure of social well-being. A practical approach must frankly be limited to a partial assessment. The discussion in this paper will for the most part be limited in precisely the way it would be in a discussion of national income accounting: The value of social science research is taken as its contribution to society's aggregate real income.

A second limitation is that no attempt is made to measure the total value of policy-oriented social science research. Instead, attention is restricted to marginal changes, or to partial budgeting of social science research projects. That approach places the focus where it really is in practical discussion, on decreases or increases in the amount of social science research.

The focus on the marginal valuation of aggregate effects on real income effects is limiting in several respects. It means, for example, that there is no place for the scientific value of research as opposed to its economic value. It means there is no place for a purely cultural value of research. This may be more of an issue in the humanities, but it is also pertinent to the noneconomic aspects of social science. Among other things, the focus on GDP means that one gives no consideration to whether income redistribution of income harms any income, ethnic, or occupational group in relation to another. Economic research can and often should try to estimate distributional effects, such as the gains or losses of farmers. Indeed, this information is often sought as a product of research. But these findings add to the returns to research only as they affect an aggregate measure of well-being. Finally, a focus on national GDP omits any gains or losses to foreigners.

Basic and applied social science research can be assessed by using the same criteria, but basic research can be conceived of as a source of two inputs crucial to applied research: raw data and methods for drawing inferences from data. Consequently, the generation of data on agriculture and rural areas comes under the purview of policy-oriented social science research, to the extent that it influences policy actions.

As an example, consider economic assessments of the returns to agricultural production research. An evaluation of this body of work requires estimates of its action-difference, and of the real income the action difference has generated. Neither estimate could be made without a study far larger than is feasible in this paper. The absence of such estimates in the voluminous literature on agricultural research policy indicates the practical impossibility of producing such estimates with confidence (see reviews in Alston and Pardey 1996; Huffman and Evenson 1993). The amounts and trends in public research funding can be observed for nations and subnational areas, which are arguably influenced by economists' estimates of returns to research, but estimation of the action-difference requires an estimate of the amount that would have been spent had the economists' estimates not been published. Huffman and Evenson (1993, 238) argue that governments have responded only weakly to economists' estimates of returns. Their evidence consists of estimates that imply that returns to research have not been equalized over alternative categories of research spending. Interest-group politics appear to be more important in allocating research spending over the alternatives.

Quantifying the Gains: Redistribution

Because so much political action is aimed at a redistribution of well-being, the question remains how to value policy research that influences political choices that involve redistribution. It is possible that research on the elasticity of demand for exports influences policy not so much because the deadweight loss to domestic national income from acreage left idle goes up as demand becomes more elastic, but because producer gains, narrowly defined, go down. A Bergsonian social welfare function can measure gains in such cases by increases in social welfare. A practical approximation would be the much-used weighted sum of individual gains and losses from a policy, where the weights are indicators of political preference that vary among individuals or groups. The difficulty is in determining what weights to use. There is one respect, however, in which it may be possible to measure gains from policy research without having to assign weights to interest groups. This is when policy research influences the political actions of interest groups themselves. For example, U.S. wool growers in 1996 voted down a program through which they assessed themselves funds to be used for promoting wool. If that outcome had been the result of social science research that measured the returns of promotion activities to producers, then the claim could justifiably be made that the gains to producers (the difference between the market rate of return and the presumably lower rate of return on funds invested in wool promotion) were returns to social science research.

An important aspect of policy-related social science research, whether used by policymakers or interest groups, is that much of it is oppositional. A research program on the environmental regulation of agriculture, for example, may consist of a range of studies that increase uncertainty about the state of the world. In terms of the Hirshleifer/Riley framework of Figure 1, this research program would reduce expected utility. This result does not violate the proposition that new knowledge always has nonnegative value; rather, this is a research program that reduces what appeared to be knowledge. In terms of the earlier, more formal discussion, it is a research program on the probabilities in the likelihood matrix (relating research findings to the true state of the world).

The preceding suggests that the framework constructed here is too narrow, for surely there are situations in which it is better to be ignorant of it than to believe things that are untrue. In fact, some political actions are constructed to be appropriate when the state of the world simply is not known. These actions are not typically a decision to do nothing.

Consider policy choice in the following situation. A large group of pesticides is considered to be a possible danger to human health. Regulating them would cause substantial economic losses. But it can also be argued that no human health dangers have been established. The situation can be modeled as a revision of Figure 1. The world is either in state 1 (pesticides are dangerous) or state 2 (pesticides are benign). If the world is in state 1, policy A_1, regulating pesticides, generates good results; but policy A_2, laissez faire, generates severe losses. If the world is in state 2, policy A_1 causes large costs, while policy A_2 is appropriate. Now consider an oppositional research program that results not in information that increases certainty that either state 1 or state 2 prevails, but instead increases uncertainty about the true state. This reduces the expected value of either policy.

This is not the end of the story. Knowing that little is known, the best course of action would be to devise a policy attuned to this situation. Devising policies to fit a situation, including its uncertainties and conflicts, is indeed a key skill of the successful policymaker. One might suggest policy A_3, a pilot program of limited regulation, along with monitoring of pesticide use and residues, and controlled studies in the field. This policy has nonlinear utility in the probabilities of the states. It is a poor policy if the state of the world is known to be either 1 or 2 but a good policy if the state is uncertain.

Here the following objection may be made. The state of the world really is 1 or 2, even if no one knows which. What is known is that policy A_3 will be wrong in either state. Indeed, this line of argument is typically raised against compromise policy proposals that call for a pilot program or for further research. An appropriate mechanism for choosing a policy if the experimental learning approach is inadvisable might be to choose policy A_1 or A_2 randomly, that is, by flipping a coin. However, the expected value of the random-choice approach, if $\pi = 0.5$, could well be less than the utility of A_3 (the experimental policy) at $\pi = 0.5$. What such a case would reflect is that if $\pi = 0.5$, a pilot program or study would help determine whether the true state was state 1 or state 2. What gives A_3 its higher utility is the expected value of being able to choose A_1 or A_2, as appropriate, in subsequent legislation. In this example, policy-oriented social science research is itself a policy option.[5]

When oppositional research leads to greater uncertainty about the state of the world, and information-generating policies like A_3 are not available, the oppositional research program may still prove beneficial. This will occur when the policy choices being determined are those of interest groups themselves, such as a choice between pieces of legislation that a commodity group should lobby for. In this case, with A_3 unavailable, the decision may be to lobby for neither A_1 or A_2, but rather to withdraw from the political debate. If the policies are alternative schemes for redistribution, this passive outcome will tend to increase national income, because any redistributional scheme will have deadweight costs. For example, the American Farm Bureau Federation has at times held back from lobbying for higher support prices and has instead made a general argument for lower taxes or a balanced budget. This stance probably reflects doubt about how much regulated support prices benefit Farm Bureau members, who are typically the more business-oriented farmers, in the long run. To the extent that these doubts have been engendered or reinforced by agricultural economists who question the income-increasing effects of price supports (for example, see Johnson 1973), then the work of such economists has contributed to national income.

CASE STUDIES

Returns to Research: The Value of Publicly Provided Information

The literature on the value of information on crop forecasting, which dealt mostly with estimates produced by the U.S. Department of Agriculture (USDA), was discussed earlier as it related to the evaluation of policy-related research. This literature's more direct application is to the debate on public spending for agricultural commodity forecasting. In addition to the papers cited earlier, some notable published research can be found in Doll (1971), Freebairn (1976), Ryan and Perrin (1974), Antonovitz and Roe (1984), and Babcock (1990). Irwin (1996) provides a comprehensive review and assessment of the implications of this literature for the social value of public situation and outlook programs.

As an example of the use of this kind of research in policy discussion, the AAEA Data Task Force states:

> While valuing information presents a complex challenge conceptually and empirically, several efforts have been made to measure the economic value of agricultural statistics. Based on data from the 1960s, Hayami and Peterson estimated the net benefits of improving the accuracy of NASS (U.S. National Agricultural Statistics Service) (then the Statistical Reporting Service) production estimates for a large number of farm

> commodities. Even under conservative assumptions, a reduction from 2.5 to 2.0 percent error (which in general is a NASS goal) returns $100 for every dollar invested. A little later Bradford and Kelejian (1977 and 1978) — using a different model, data from 1955–1975, and a Bayesian rather than a "naive" loss function — provided a different measure. They produced an estimate of $64.29 million (1975 dollars) for the annuitized annual value to the U.S. economy of eliminating sampling errors in NASS's monthly estimates, just for winter wheat production. Antonovitz and Roe calculate the annual social benefit gain of $78 million annually from the adoption of USDA outlook forecasts by U.S. feed cattle producers. (AAEA 1996, 10).

Irwin (1996) develops a further argument that the observed effects of USDA crop reports on market prices are evidence that these prices generally respond to changing conditions more quickly with these reports than they would otherwise, and that this is a significant source of welfare gains.

Suppose that the finding of the literature is correct, that net social benefits of NASS data are large. To be concrete, suppose that a cut of $10 million annually in the NASS commodity data program is being considered — as it was in the early 1990s.[6] Suppose further that the social value of the information lost would be $20 million annually, that is, that the studies are correct about the net social gains from these estimates. This estimate of gain is far less than the $100 gain per $1 spent of Hayami and Peterson, but one has also to give some weight to others' far lower estimates of net gains, OMB assertions that some NASS surveys have costs in excess of their gains, and even the views of some legislators and citizens that NASS estimates as a whole are valueless or worse (for example, see Weber 1997).

To take a more modest assessment, suppose there is a $10 million annual net social gain from maintaining rather than cutting the NASS budget ($20 million marginal information value minus $10 million marginal cost). To place a value on the social science research that generated this estimate, one has to estimate the change in the probability of a budget reduction caused by the research findings. How has research on the value of information actually influenced the policy process that determines the NASS budget? The key places to look are decisions by the Office of Management and Budget (OMB) and USDA's arguments to OMB in the executive branch and the appropriations committees in Congress along with the authorizations made by agriculture committees concerning agricultural data. While commodity groups dominate agricultural policy, their voice is usually muted and the messages somewhat mixed with respect to NASS and data issues. This means that the influence of agricultural interests in maintaining NASS barely outweigh the generic arguments of budget cutters' for reducing NASS appropriations. In this context, the value-of-information literature does make a difference. It makes a difference not because congressional appropriators or their staffs read this literature, but because they listen to experts from the administration and nongovernmental institutions, upon whom they rely for substantive judgment. Experts in the administration and elsewhere do not typically rely on particular studies cited, but on the general climate of opinion among professional economists — who are well represented both on the staffs of congressional committees and in the executive branch (and not just in USDA). And it is this climate, expressed in the AAEA Task Force report quoted above, that provides the favorable reception to additional spending and resistance to cuts in NASS.

In short, it is not extravagantly optimistic, and indeed it is fairly conservative, to suppose that the body of social science research on agricultural commodity data bears half the responsibility for the decision not to cut $10 million from NASS in FY 1991. Given the earlier estimate of a $10 million net gain for this decision ($20 million marginal social benefit minus $10 million marginal cost), the expected value of the social science research would be $5 million annually, for as long as its influence lasts.

This preceding is a marginal calculation, related to a $10 million change in the NASS budget change. The corresponding total assessment would involve an estimate of total federal expenditures on agricultural statistics (by NASS, parts of the Economic Research Service, the Agricultural Marketing Service, and in the agricultural censuses) in the absence of research findings on the value of all publicly generated information in agriculture. The literature contains no estimate of that value, or even a wild guess. But again, the policy influence is through a general sense that the public information enterprise generates net social gains. That general sense would, however, probably exist among economists even in the absence of research on the specific value-of-information issues cited earlier. So even if economics as a field of expertise is responsible for the size of the agricultural statistics budget (though it is too small, according to the predominant estimates), it would not be correct to attribute the gains to specific empirical studies. Nonetheless, these studies have strengthened the general view that favors the provision of data by public agencies.

RATE OF RETURN TO AGRICULTURAL RESEARCH

A related, but larger and more widely known body of social science research is on the benefits of research that results in technological improvements in agricultural production. The value of that social science research can be assessed in a way that parallels the discussion of agricultural statistics. Again, prevailing estimates are that the social rate of return is quite high, well above going rates of interest, and even above the arguably more meaningfully comparable rates of return to private entrepreneurial investment before taxes. Estimates that public investment yields net social gains prevail among economists, even with the complications that research is undertaken privately as well as publicly and that there are a variety of technical reasons why many studies overestimate the returns relevant at the current margin (see Alston and Pardey 1996, Chapters 6 and 7).

Thus there is once again the question of the role that research on the rate of return has played in determining the amount of the public spending on research. In this case, however, none of the many authors who have worked on returns to research have published an assessment of the difference this body of work has made in policy choice. Huffman and Evenson supply evidence, as noted earlier, that public decisions on the allocation of research funds do not follow exactly the suggestions made in rate of return studies. But that does not mean that policymakers are uninfluenced by those studies, or that policymakers use their findings suboptimally. One of the services of the Bayesian valuation model of Figure 1 is to show how research findings can be valuable to rational decisionmakers even though decisionmakers do not gain certainty from the findings of researchers.

While a dollar-value assessment of the value of rate-of-return research cannot be provided here, it is worth noting that the dollar-value stakes for research on agricultural production are much higher than for statistical information. Instead of perhaps $150 million in annual public outlays on U.S. agricultural data, federal and state spending on agricultural production research totals about $1.5 billion. If rate-of-return research has caused this spending to be 10 percent higher than it would have been otherwise, and if that marginal spending generated a social rate of return 10 percent above the relevant opportunity return, then the rate-of-return research generates a social gain of $15 million annually ($1,500 million x 0.1 x 0.1).

Trade Liberalization Studies

In the last 20 years extensive social science research, both positive and normative, has been undertaken on the consequences of the international trade policies of individual countries and on the benefits of both regional and global trade liberalization. What is the value of that research? In the 1990s a series of policy changes has occurred that make it plausible that the value of this research has been substantial. The most significant movement toward global liberalization of trade in agricultural products since World War II was the agriculture agreement reached in the Uruguay Round of the GATT negotiations, concluded in 1994 and being phased in over six years. In addition to relaxing some long-standing nontariff barriers to trade, the Uruguay Round agreement set up arrangements under the new World Trade Organization that should help minimize the use of health, safety, or other quality control measures as disguised measures of protection for domestic industries. The agreement also established important groundwork for further liberalization through negotiations to expand upon the Uruguay Round agreement at a later date. Moreover, beyond global liberalization, important movements toward liberalizing agricultural trade within regions have taken place in the North American Free Trade Agreement (NAFTA), the Mercosur agreement among Argentina, Brazil, Paraguay, and Uruguay, and the negotiations to bring Central European countries into the European Union and increase the access of the newly independent states of the former Soviet Union to EU markets. In all these developments economists were important not only in developing the public-interest rationale for liberalization, but also in providing technical analysis and advice to both private-sector interests and to governments.

In assessing the value of these activities, one may reasonably invoke what can be called the "bootstrap" approach suggested by Harberger (1954). He balanced the costs of antimonopoly efforts of economists against his estimate that there would be an annual gain in welfare of more than $300 million if monopolies were replaced by competition in the U.S. economy.[7] Analogously, the estimates of the World Bank (1986), Tyers and Anderson (1992), and others summarized in Blandford (1990) can be used to show that the complete global liberalization of trade in agricultural commodities would bring net social benefits of $30 to 40 billion annually. So if trade were liberalized, and if that liberalization were attributable in part to assessments by economists, the social value of those assessments could be estimated. (But the work of economists who have argued against trade liberalization must also be included on the cost side.)

Now that the Uruguay Round, NAFTA, and Mercosur are in effect, this issue can be posed in a more practical context. The difficulty is that these agreements appear to have had only small effects so far. But if even 2 percent of the gains from complete liberalization have been achieved and economists can claim 25 percent of the responsibility for this, then the worldwide benefit from the trade policy research of agricultural economists would be $150 to 200 million annually (0.02 x 0.25 of $30 to $40 billion).

Analysis of National Commodity Market Intervention

Many countries have changed their national policies in the past decade, usually in the direction of free-market reforms. New Zealand led the industrial countries by making such changes in 1984–86. The reform of the Common Agricultural Policy of the European Union in the 1990s was a paler version of the New Zealand reforms, but it had larger deadweight losses to begin with.[8] Many developing countries, notably Chile, Argentina, and China, have implemented limited market-oriented reforms. Other countries, such as India and Egypt, seem to be starting out on similar paths. The former Soviet sphere is rebuilding its agricultural policies as part of its post-Communist evolution. Agricultural economists have taken part in analysis and debate in all these countries. In New Zealand, Hungary, Poland, Latvia, Ukraine, India, and Egypt, where the author has been able to observe these debates, there is evidence that contributions of economists have made a difference in the form and content of agricultural policies. But their impact appears marginal, and the reforms have typically not been extensive enough to reduce economic waste and inefficiency.

It would be going too far even to guess the value of the economists' by attempting to quantify the real-income effect of their advice and the extent to which policy decisions have been influenced by that advice. However, there exists a more direct quasi-market indication of economists' value in this sphere. This is the willingness to pay for policy research. It is reasonable to use the willingness of the public to pay to place a provisional lower bound on value of the analytical services provided by social scientists employed by government agencies. The lower bound is provisional because two further considerations may weaken the case. First, some theories about bureaucracy have suggested that employees of government agencies may be paid more than the value of their output (see the parallel arguments on agricultural production research in Pasour and Johnson 1982). Second, some public research and analysis is conducted largely if not entirely to respond to or refute economic assessments by private-sector interests or foreign governments. To this extent, national income might remain unchanged if both sides reduce their efforts. (A similar point could be made concerning lawyers' services, brand advertising, and military preparedness).

This approach suggests valuing the output by looking at costs, as is often done in measuring the services of lawyers, accountants, entertainers, and other providers of intangible services. However, because of arguments such as those referred to above, it is useful to consider attempts to measure the value of the output independently.

In the United States, the deadweight loss from farm programs can be estimated to have been $4–5 billion annually in 1985–87. This was mostly due to productive cropland left idle (Gardner 1990, 59). In 1990, policy reforms were undertaken that reduced the deadweight losses substantially, arguably by three-fourths or more, principally by eliminating annual acreage-idling programs. This was done more dramatically following enactment of the 1996 Farm Act. The author has argued that agricultural economists made a significant contribution to these reforms (Gardner 1996). The influence did not stem from quantitative estimates of deadweight loss, but from work on a variety of topics that showed that the commodity programs cost taxpayers and consumers many billions of dollars, but accomplished much less for farmers. These findings did not influence legislators directly, but through newspaper editorialists, government experts in both the executive and legislative branches, and commodity group representatives.

It has to be recognized that economists have not spoken with one voice on agricultural policy. Pasour (1988) emphasizes the role played by agricultural economists in putting U.S. commodity programs into place in the 1920s and 1930s. However, it would be wrong to say that because agricultural economists argue both sides of policy issues, or first one side and then the other of the same issue, their efforts largely cancel themselves out. There appears to have been a consensus in favor of government action in commodity markets before World War II that evolved into a consensus opposing such actions in the 1980s and 1990s.[9] It could well be that the consensus was correct in both cases — that commodity policies had net social benefits in the 1930s, as argued for example by Cochrane (1993) and Clarke (1994), but that the policies generated net social costs after World War II.

The key point is that these policy issues involve difficult scientific questions as well as potent political forces. In this case, the extent to which policy research has reduced the range of uncertainty about the effects of public choices is an appropriate indicator of the ex ante gains that should be attributed to the research.

Beyond the more sweeping work on agricultural policy, a larger body of economic research has been devoted to particular questions raised in the implementation of farm programs. Examples are the amount of acreage that should be placed in set-aside programs each year, the size of sugar import quotas needed to achieve legislated price targets, selection criteria for land offered by farmers for inclusion in the Conservation Reserve Program, purchase prices for butter and nonfat dry milk needed to reach the legislated farm price of milk, the size of the export bonuses offered under the Export Enhancement Program, and the rules needed to implement pilot programs in revenue and crop insurance. The provision of alternative answers for these and a hundred similar questions can reduce the efficiency loss from commodity programs by many millions of dollars. The net benefits are not quantifiable, but one can have greater confidence in an appeal to the quasi-market mechanism through which government agencies hire researchers because they believe the benefits from the findings justify the staff costs. The author had several years' experience in negotiations on the budget of the Economics Research Service (ERS), a part of USDA. These took place within USDA, between USDA and the White House Office of Management and Budget (OMB), and between the Bush Administration and the appropriation committees of the House and Senate. In these discussions, OMB made a serious attempt to weigh the value of services provided by ERS against the agency's costs. It asked why agriculture was endowed with so many more government economists, given the size of agriculture relative to other sectors of the U.S. economy). OMB ended up accepting an annual budget for ERS of more than $50 million principally because of its acceptance of the ex ante value of the product in policy formulation.

RESEARCH ON THE EFFECTS OF ADVERTISING AND PROMOTION

A narrower and more precisely defined area of inquiry is the effectiveness of generic product promotion, generic meaning not specific to a product identified by a brand name. Such promotion is typically funded largely by producer groups, and the basic issue is whether funding promotion is profitable for the industry.

A recent example is Blisard, Blaylock, and Smallwood (1996). They estimate that over a 10-year period, 1984–1994, generic advertising raised fluid milk sales by 5.6 percent, or 12.8 billion pounds. Fluid milk advertising expenditures were $296 million over this period. Of these, $110 million were attributed to a $0.15 assessment on each per hundredweight of milk sold, mandated by the Dairy and Tobacco Adjustment Act of 1983. Blisard and his colleagues estimate the "gain per act-increased advertising dollar" to be 117 pounds of additional milk consumption.

Should milk producers, and U.S. citizens at large, rejoice in this finding, assuming that they can believe it? To give a rough answer for milk producers, suppose that the elasticity of demand, η, is between -0.5 and -1.0, and that the elasticity of supply, ε, is between 0.5 and 1.5 (over a 10-year period of adjustment). Then the price effect of a 5.6 percent demand shift ($0.056/[\varepsilon - \eta]$) is between 0.022 and 0.056 for the range of elasticities considered. Using an average farm price of $11 per hundredweight, this implies a price increase for farmers of between $0.24 and $0.62 per hundredweight. With 2.3 billion hundredweight sold (12.8/0.056/100), the gain to producers, measured by producers' surplus, is between $550 million and $1,400 million. Since this gain was achieved by spending $110 million, the implication of the research finding (although Blisard et al. do not put it this way) is that the fluid milk promotion program is a good investment for farmers. Indeed they ought to spend more on promotion.

The gain to society as a whole must also consider the well being of milk consumers. There is no standard method for determining this in welfare economics. If advertising changed tastes, an underlying assumption behind estimates of consumer benefit has been violated. If advertising conveyed information, and the increased willingness of consumers to pay for milk measures the value of this information to each consumer, then consumers are no worse off as a result of the promotion program. The producers' net gain is a rough lower-bound measure of society's net gain.[10]

For this paper, the issue is how to value the social science research that led to the net-gain estimate, that is, what is the value of studies such as Blisard, Blaylock, and Smallwood (1996). Many such studies have been carried out, and they tend to show that promotion programs have substantial net benefits, at least to producers.[11] But the accuracy of the findings is open to question, probably more so than the studies of gains to agricultural research. (With reference to export promotion programs, see Sumner 1995, 106–109, and works cited there.) One of the policy actions often taken in promotion programs is a vote by producers on whether to assess themselves. In 1996 the sheep producers, in a close vote, rejected their promotion program, despite the support of the leadership of their main producer organization.

In the two-state, two-policy version of the Hirshleifer-Riley model of valuation, research on the consequences of generic advertising may have little effect on the action chosen (impose an assessment on producers [A_1], as opposed to not imposing an assessment [A_2]). This would be true even when the advertising delivers a message of support for its message (advertising pays [S_1]), as against advertising does not pay [S_2]). The reason is that the perceived quality of the research findings is low in the sense that the likelihood of S_2 is not much different whether the research estimates the true state to be S_1 or S_2. In this case the posterior probabilities will be so close to the prior probabilities of S_1 and S_2 that the ex ante gain from research will also be low. Figure 2 illustrates the gain in such a case as G-H, the distance of the double-headed arrow. The ex ante gain may well be so low that it is not worth paying for the research, which is possibly true for research on the value of generic commodity promotion.

In the case of promotion programs and other policy-related social science research, there is another aspect to the issue in that private-sector enterprises or organizations themselves fund the research. For example, the citrus industry has commissioned studies of the value of generic promotion of orange juice. So instead of relying on public cost-benefit analysis, there is a market test for the ex ante value of social science research. Even if such market decisions are questionable, and they have been questioned, for example, when people pay for stock market advice, one should not be too quick to write off the research as not worth what is paid for it. The payment, after all, is voluntarily made by people who economists normally assume are rational.

COSTS OF AGRICULTURAL POLICY RESEARCH

The costs of agricultural policy research are mainly for personnel. The directory of the American Agricultural Economics Association (AAEA) lists about 730 people as specialists in agricultural policy (category S840). Many members list more than one specialization, however. The directory lists about 3,400 members, and 4,800 reported specialties. This suggests that 520 full-time equivalent AAEA members spend their research time on agricultural policy (3,400 x 730/4,800). Assuming that an average of 40 percent of the average academic working year is spend on research (as opposed to teaching and extension), this implies that each year these members spent the equivalent of 210 years conducting agricultural policy research. At an average cost of $60,000 plus 50 percent of that in benefits and overhead support, this amounts to $19 million annually ($90,000 x 210). However, some agricultural policy research is done by specialists in subjects in other than agricultural policy, resource economics, for example, and some agricultural policy researchers are not members of the AAEA. To roughly accommodate these omissions, one can double the cost and round up to $40 million annually.

The relevant question about the benefits of U.S. policy-related economic research in agriculture is, then, whether they add up to $40 million annually. Without wishing to claim much for the rather sketchy case studies, earlier discussions suggested annual returns of approximately $5 million from value-of-information research and $15 million for returns-to-research studies. Assuming that the United States reaps 20 percent of the gains from trade liberalization, research output in that area would be valued at about $40 million annually. Assuming that one-third of the budget of the ERS is spent on implementation research whose value is what is paid for it, the annual benefit would be $20 million. The value of broader agricultural policy research is impossible to estimate with any pretense of precision. In order to have a figure to work with, however, assume that 1996 Farm Act reforms eliminate one-half of the $4 billion in deadweight losses of pre-reform policies, and that 5 percent of the credit for this achievement goes to agricultural policy research. This implies a $100 million annual benefit from this research.

Summing up, the U.S. economy receives benefits equal to$180 million ($5 + $15 + $40 + $20 + $100) from the $40 million spent on agricultural policy research. Taking into account remaining costs and benefits left out of these numbers; policy issues not covered in this analysis, such as soil conservation, environmental and food safety regulation, and domestic marketing policy; the lag between research findings and policy results; and the uncertainty in all the benefit values given, one might double the costs and place a doubling and halving range around the benefits. One could then say that benefits of between $90 million and $350 million resulted from costs of $40 to $80 million. Even with the most pessimistic view of annual benefits ($90 million) and costs ($80 million), U.S. agricultural policy-oriented economic research pays.

CONCLUSIONS

This paper analyzes policy research as a source of information, primarily for policymakers, who are Bayesian decisionmakers. The value of their decisions depends on the state of the world, and different policy choices are optimal depending on that state. The true state, however, is unknown, and the output of research is a reduction in uncertainty about that state. The value of proposed research is the ex ante addition to the expected value of the social objective function. This value is nonnegative for decisionmakers who can accurately assess the likelihood that a research program will reveal the true state. But the value of the research may not justify the costs, especially the costs of research that is not expected to contribute significantly to the identification of the true state.

The case studies of agricultural policy research considered in this paper suggest that there are substantial net gains to the ongoing policy research agenda. However, the quantification of these gains is highly conjectural and narrowly focused on agricultural economics in the United States. Nonetheless, the conceptual framework and empirical evidence given provide reasonable grounds to believe that policy-related research has generated benefits that more than cover the costs.

One line of skepticism about this conclusion as a justification for maintaining or increasing public support for social science research is that even if the arguments of this paper are accepted in the aggregate, policy-related social science research in agriculture could be managed more efficiently. Many policy-related studies and publications, even the entire output of some researchers, could have been omitted without loss. So only a part, and perhaps even a small part, of the research need have been funded. With better management, less policy research can be funded in the future without a loss of expected future net benefits.

The flaw in that conclusion is its presumption that better management is feasible, in the sense that winners can be picked in advance. It is likely that, as with drilling for oil, recruiting football players, or breeding racehorses, one has to back the low-return efforts to get the successes. As a practical matter, that is, if the policy research budget were to be cut, it is likely that almost as many productive projects would be cut as unproductive ones.[12]

NOTES

1. The calculations are:

State	Prior probability	Likelihood of state: Given inelastic estimate	Prior probability times likelihood	Posterior probability, given estimate
Inelastic	0.5	0.8	0.40	0.40/0.55 = 0.727
Elastic	0.5	0.3	0.15	0.15/0.55 = 0.273
Given elastic estimate	...	...	0.55	...
Inelastic	0.5	0.2	0.10	0.10/0.45 = 0.222
Elastic	0.5	0.7	0.35	0.35/0.45 = 0.778
Given elastic	...	...	0.45	...

2. Research can make the user worse off ex post if its findings are wrong: for example, if one is at T but the true state is inelastic, which has a probability of 0.2. Moreover, research can make the user worse off ex ante if he or she places too much confidence in it. One of the beauties of the Bayesian approach is that it takes into account the likelihood that the research gives the wrong answer. However, that approach assumes that the user knows the likelihood of getting the wrong answer. If the research is of lower quality than thought, the posterior probabilities should be closer to the prior probability of 0.5 than is shown in Figure 1. For example, if the message that placed the user at T ($\pi = 0.78$) should really have placed him or her just to the right of H ($\pi = 0.51$), a decision to switch policies would have been wrong and the ex ante value of the research would have been negative.

3. Lave considered an economywide context and still estimated that better information yielded a net loss, but he assumed that the opportunity cost of resources in production (of raisins, the commodity he analyzed) was zero, and this apparently drove his (informal) economywide assessment.

4. While the approach of Hirshleifer and Riley is identified here because they use a diagram like Figure 1 to analyze the value of information, this approach to the graphical analysis of the two-state decision problem under uncertainty goes back

to Savage (1972).

5. The 1990 Farm Act contained over 100 mandates and authorizations for studies, reports, and pilot programs. They typically reflected stalemate after extended debate on other policy options. The sum of studies became so large that section 2515, "Scarce Federal Resources," was inserted, stating that the Secretary of Agriculture may "rank by priority the studies or reports authorized by this Act and determine which of those studies or reports shall be completed. The Secretary shall complete at least 12 such studies or reports." (U.S. Code, 104 STAT. 4075).

6. The Farm Act of 1990 required, and the Bush Administration also desired, expansion of USDA's data base on chemical use by farmers. The expense of this could be covered by cutting some of USDA's National Agriculture Statistics Services' (NASS's) ongoing statistical activity — commodity coverage or sample size — or by adding to NASS's budget. It was decided to add to NASS's budget, despite general stringency in "discretionary" programs. NASS Appropriations went from $67 million in FY 1990 to $76 million in FY 1991 (U.S. OMB 1991, 341).

7. The "bootstrap" label indicates that the measure of value of the research depends on the economic values that the research itself measures.

8. Blandford (1990, 425) cites an estimate of $13 billion (1985 dollars) annually as the net loss in EU countries due to the CAP in the mid-1980s.

9. The evidence for this consensus in the 1990s is the near unanimity of agricultural economists' general views in Congressional testimony on the 1990 and 1995 farm bills, and in expressions of net social costs of existing programs in outlets such as Choices (see Gardner 1996).

10. The main possibility that would render this argument invalid would be if not all consumers were influenced by the advertising. Then the additional consumption of some consumers would drive up that price for all consumers, and the added costs to the uninfluenced consumers should be counted as a loss attributable to the program.

11. Some studies have analyzed the consequences of promotion programs financed by taxpayer funds, notably the Market Promotion Program (MPP, formerly Targeted Export Assistance) which makes grants for the promotion of U.S. agricultural products abroad. Cost-benefit analysis of the MPP has been required by OMB.

Such analysis has estimated that the program has had substantial net benefits.

12. To this point it is natural to append the standard researchers' refrain: It might be worthwhile to fund additional research on how to "pick winners" and in other ways manage policy research better.

Figure 1 Value of a research program to estimate the elasticity of export demand

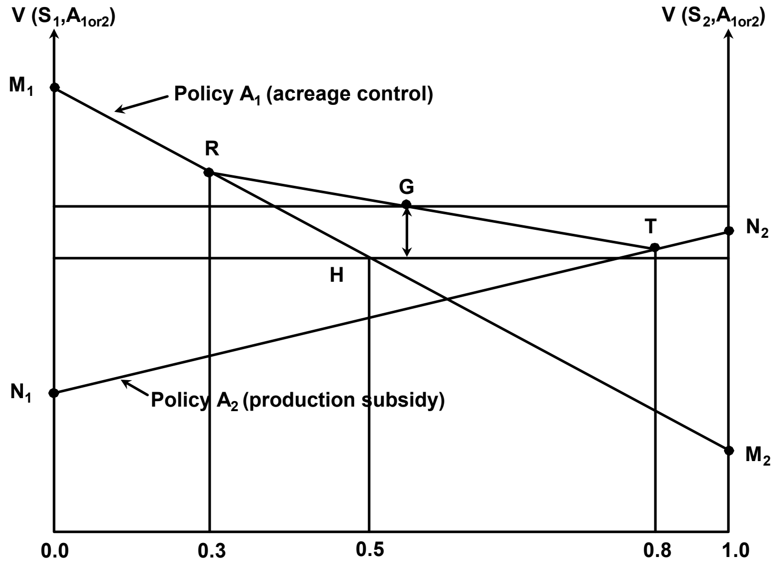
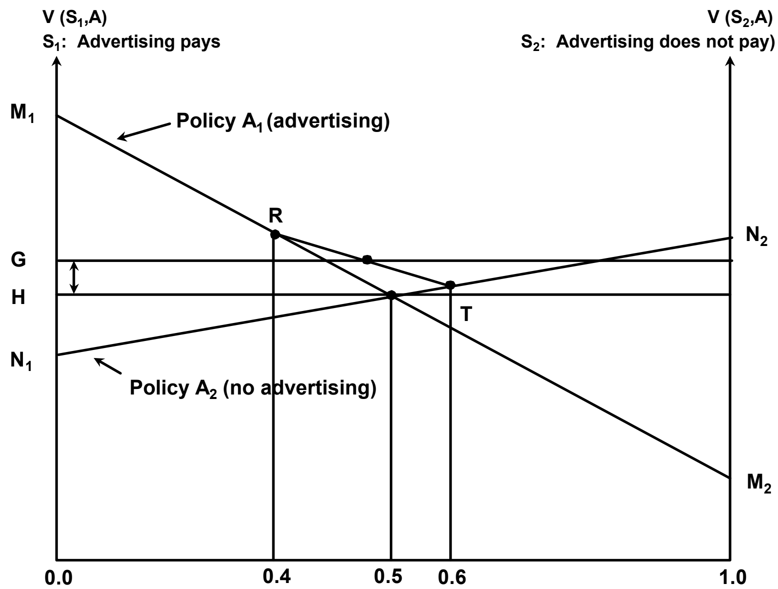


Figure 2 Value of a research program to estimate whether advertising pays for an industry



Note: The ex ante value of the research program is the distance between G and H.

REFERENCES

American Agricultural Economics Association, Data Task Force. 1996. Final report. University of California at Berkeley, December. Mimeo.

Alston, J. M., and P. G. Pardey. 1996. *Making science pay: The economics of agricultural R&D policy.* Washington, D.C.: AEI Press.

Antonovitz, F., and T. Roe. 1984. The value of a rational expectations forecast in a risky market. *American Journal of Agricultural Economics* 66 (December): 717–723.

Babcok, B. A. 1990. The value of weather information in market equilibrium. *American Journal of Agricultural Economics* 72 (February): 63–72.

Blandford, D. 1990. The costs of agricultural protection and the difference free trade would make. In *Agricultural protectionism*, ed. F. Sanderson. Washington, D.C.: Resources for the Future.

Blisard, N., J. Blaylock, and J. Smallwood. 1996. *Fluid milk and cheese advertising*. ERS Staff Paper No. 9601. Washington, D.C.: Economic Research Service, U.S. Department of Agriculture.

Bradford, D. F., and H. H. Kelejian. 1978. The value of information for crop forecasting with Bayesian speculators: Theory and empirical results. *Bell Journal of Economics* 9 (1): 23–44.

Bullock, J. B. 1976. Social costs caused by errors in agricultural production forecasts. *American Journal of Agricultural Economics* 58 (February): 76–80.

Chavas, J. P., and R. D. Pope. 1984. Information: Its value and measurement. *American Journal of Agricultural Economics* 66 (December): 705–710.

Clarke, S. H. 1994. *Regulation and the revolution in U.S. farm productivity.* New York: Cambridge University Press.

Cochrane, W. W. 1993. *The development of American agriculture.* Minneapolis, Minn.: University of Minnesota Press.

Doll, J. P. 1971. Obtaining preliminary Bayesian estimates of the value of a weather forecast. *American Journal of Agricultural Economics* 53 (4): 651–655.

Fox, G. 1985. Is the U.S. really underinvesting in agricultural research. *American Journal of Agricultural Economics* 67 (4): 806–812.

Freebairn, J. W. 1976. Welfare implications of more accurate rational forecast prices. *Australian Journal of Agricultural Economics* 20: 92–102.

Gardner, B. L. 1990. The United States. In *Agricultural protectionism*, ed. F. Sanderson. Washington, D.C.: Resources for the Future.

________. 1996. Agricultural economists in the policy arena: The 1995 Farm Bill. *Review of Agricultural Economics* 18 (2): 155–165.

Graham-Tomasi, T. 1984. Discussion. *American Journal of Agricultural Economics* 66 (December): 724–725.

Harberger, A. C. 1954. Monopoly and resource allocation. *American Economic Review* 44 (May): 77–87.

Hayami, Y., and W. Peterson. 1972. Social returns to public information services: Statistical reporting of U.S. farm commodities. *American Economic Review* 62 (1): 119–130.

Hirshleifer, J., and J. G. Riley. 1992. *The analytics of uncertainty and information.* New York: Cambridge University Press.

Huffman, W. E., and R. E. Evenson. 1993. *Science for agriculture*. Ames: Iowa State University Press.

Irwin, S. 1996. An essay on the value of public situation and outlook programs. Ohio State University, Columbus, October. Mimeo.

Johnson, D. G. 1973. *World agriculture in disarray.* New York: St. Martins Press.

Lave, L. B. 1963. The value of better weather information to the raisin industry. *Econometrica* 31 (January-April): 151–164.

Lindner, B. 1987. Toward a framework for evaluating agricultural economics research. *Australian Journal of Agricultural Economics* 31 (August): 95–111.

Norton, G. W., and G. E. Schuh. 1981. Evaluating returns to social science research: Issues and possible methods. In *Evaluation of agricultural research*, ed. Fishel, Paulsen, and Sundquist. University of Minnesota Agricultural Experimental Station Miscellaneous Publication No. 8-1981, St. Paul, Minn.

Pasour, E. C., Jr. 1988. Financial support and freedom of inquiry in agricultural economics. *Minerva* 26: 31–52.

Pasour, E. C., Jr., and M. A. Johnson. 1982. Bureaucratic productivity: The case of agricultural research revisited. *Public Choice* 39: 301–317.

Ruttan, V. W. 1982. Bureaucratic productivity: The case of agricultural research — A rejoinder. *Public Choice* 39 (2): 319–329.

Ryan, J. G., and R. K. Perrin. 1974. Fertilizer response information and income gains. *American Journal of Agricultural Economics* 56: 337–343.

Savage, L. J. 1972. *The foundations of statistics.* New York: Dover (originally Wiley 1954).

Sumner, D. A. 1995. *Agricultural trade policy.* Washington, D.C.: AEI Press.

Tyers, R., and K. Anderson. 1992. *Disarray in world food markets*. New York: Cambridge University Press.

U.S. Office of Management and Budget. 1991. *Budget of the United States government, fiscal year 1992*. Washington, D.C.: U.S. Government Printing Office.

Weber, B. 1997. Let's put an end to USDA's commodity reports. *Progressive Farmer* (January).

World Bank. 1986. *World development report, 1986*. New York: Oxford University Press.

Bruce L. Gardner is a distinguished university professor of the Department of Agricultural and Resource Economics of the University of Maryland.