Returns to Policy-Related Social Science Research in Agriculture

by
Bruce L. Gardner

WP 97-06

March 1997

Department of Agricultural and Resource Economics
The University of Maryland at College Park

Returns to Policy-Related Social Science Research in Agriculture*

Bruce L. Gardner
University of Maryland

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 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, I felt keenly the need for reasonably objective evaluative criteria. This need was never satisfied. Nonetheless, budgetary arguments were laid out and decisions made. Some lessons from this activity will be discussed later. I will begin by going back to first principles to consider the possibilities for systematic evaluation, and how such evaluation might be organized conceptually. Then I will turn to several case studies, which unfortunately can be only feebly quantified.

Analytical Methods for Measuring Returns to Policy Research

The valuation of policy research requires us to estimate what difference the research makes in peoples' actions, and the value of this difference. The actions involved are principally political actions. Political actions are those of policymakers in government, and also lobbying actions of private citizens seeking to influence government. For example, the actions influenced by research on returns to publicly funded agricultural production research are decisions about levels of public spending on agricultural research, and lobbying actions by producer or consumer groups who seek to influence such research spending. [Policy research may also affect other

---

* Prepared for the symposium, "Measuring the Benefits of Policy-Oriented Social Science Research," International food Policy Research Institute, April 4-5, 1997.

actions, e.g., private companies' decisions to spend on agricultural research, but these effects are ignored here.]

Policy Research as Information for Decisionmakers Facing Uncertainty

Policy research is an intermediate product, which is an input into political decision. A helpful way to think about the nature of policy research output is as information. Following Hirshleifer and Riley (Chapter 5, 1992) 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 which is itself uncertain. This way of characterizing the informational output of policy research lends itself naturally to a model of policy actors as Bayesian decision-makers 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 (at end of paper) uses a 2-state example. Let the two states of the world be: S_1, export demand for a commodity is inelastic; S_2 , export demand is elastic. The world is in one of these states, but we don't know which. A research program is to estimate 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 our policy evaluation purposes V is measured by a weighted sum of producer and consumer-taxpayer benefits. 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 elastic. If A_2 is chosen, the result is N_1 if demand is inelastic and N_2 if elastic. We'll 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 we are maximally uncertain about S_1 and S_2, so $\pi_1 = \pi_2 = .5$. We then maximize the expected value of V by choosing policy A_1, giving expected V at the level of point H.

Now consider the value of a research program to generate an estimate of whether export demand elasticity is elastic or inelastic. The research program will deliver an estimate (message) of either inelasticity or elasticity, but the message may be incorrect. Suppose the uncertainties are as given by following probabilities:

		research finding	
		inelastic	elastic
true state	inelastic	.8	.2
	elastic	.3	.7

If the true situation is that demand is inelastic, the research will correctly obtain this finding with 80 percent probability, but with 30 percent probability the research will incorrectly say that demand is elastic. If the true state is elastic, this is slightly harder to detect — the research program will give the correct result with 70 percent probability and the incorrect result with probability 30 percent. These likelihoods provide us with an operational measure of research quality — the higher the quality the closer to 1's on the diagonal and zero off.

Applying Bayes' Theorem to the calculate the posterior probabilities, assuming prior probabilities that elastic or inelastic demand were equally likely (probability = .5 for each state),

we get the following posterior probabilities:[1]

		research finding	
		inelastic	elastic
state	S_1: inelastic	.73	.22
	S_2: elastic	.27	.78

In figure 1, if the finding were of inelastic demand, we have $\pi_2 = .27$, and we choose A_1, with expected benefits at R. If the finding were of elastic demand, we have $\pi_2 = .78$ and choose A_2 with expected benefits at point T. So the research makes no difference in our choice in the first case, but the research causes us to reverse course in the second.

What is the ex ante value of the research program? Assuming, according to our priors, 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 measured by the distance G-H (the length of the double-headed arrow).

The 2-state Bayesian model is simple but nonetheless helpful in pinpointing the determinants of the value of research. In particular three elements of the situation determine the value of the research:

(i) The value of acting upon the information the research provides, if the information were correct. (This is $M_1 - N_1$ if the research finds S, and $N_2 - M_2$ if the research finds S_2.)

---

[1] The calculations are:

state	prior probability	Likelihood of state: given inelastic estimate	prior times likelihood	posterior prob. given estimate
inelastic	.5	.8	.40	.40/.55 = .727
elastic	.5	.3	.15	.15/.55 = .273
		given elastic estimate	.55	
inelastic	.5	.2	.10	.10/.45 = .222
elastic	.5	.7	.35	.35/.45 = .778
			.45	

(ii) Prior knowledge about the subject of the research. (In figure 1, the prior probabilities of .5 for each state represent zero prior knowledge.)

(iii) Quality of the research, as measured by the likelihood that research findings are correct. (In figure 1, 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 score for obtaining the ex ante value of the research. Morever, the model helps confirm which of our common-sense intuitions about the value of research are most likely to make a difference. For example, we can straightforwardly get from the model the following propositions:

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

2. If the research does not reveal information about the true state of the world, it is valueless. Proof: No information means the likelihoods of the 2 states are .5, .5 regardless of the research findings. Applying the model with this likelihood matrix means the posterior probabilities are the same as the priors, so R and T in figure 1 cannot differ from their pre-research position, and (if we started at point H, we would end up at H.)

3. If the same policy is preferred for all states of the world the research considers, the research is valueless. Proof: In figure 1, this situation would be represented by having both $N_2 > M_2$ and $M_1 > N_1$ (or the inequalities reversed). We would always choose A_1 (or A_2), regardless of the location of H, R, or T. So there is no gain from the research.

4. The value of research is nonnegative. This is a less obvious result. It is true that some particular findings (or "messages") have no value: in the export demand elasticity example, a finding of inelastic demand would make no difference in action taken. But even so the research program — "message service", in Hirshleifer and Riley's terminology — has some value as long as it reduces uncertainty about the world sufficiently, in figure 1 to increase π to the level (about .55) where N_1N_2 and M_1M_2 cross. Even uninformative research has no less than zero value, ex ante, because it is simply ignored. The ex ante value of information is positive, essentially because of a free disposability assumption, that it costs nothing to ignore a message.[2]

In contrast to this result, analyses of crop information, e.g., Lave (1963) or Bullock (1976), show that improved information can make its users (producers) worse off. This occurs, in the Lave and Bullock cases, because producers' reactions to short-crop information, for example, result in lower prices than would occur absent the information, which no single producer generates but their joint response does.[3] More generally, a problem is that the direct users of information encompass only part of the economy. Bradford and Kelejian (1978) provide a more complete welfare evaluation of crop forecasting information with Bayesian market participants.

---

[2] Research can make us worse off ex post if it is wrong, e.g., if it places us at T but the .2 event with probability occurs, that the true state is "inelastic". Moreover, research can make us worse off ex ante if we are overconfident 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 we know the likelihood of getting the wrong answer. If the research is of lower quality than we thought, our posterior probabilities should be closer to the prior 0.5 than is shown in figure 1. For example, if the message that placed us at T (π = .78) should really have placed us just to the right of H (π = .51), we would have made the wrong decision to switch policies and the ex ante value of he research would have been negative.

[3] Lave considered an economy-wide context, and still estimated a net loss to better information, but he assumed the opportunity cost of resources in production (of raisins, the commodity he analyzed) to be zero, and this apparently drove his (quite informal) economy-wide assessment.

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 Hirshleifer/Riley (or Bradford/Kelejian) approach is the relevant one for evaluating the information generated by policy research, because policymakers are by assumption taking into account all the effects of what they do (however they may weight and trade off among these effects) and can use the research or not as they see fit. Nonetheless, implementation of this approach with respect to its ex ante consideration of the message service (i.e., research program) in the Bayesian context makes severe informational demands, as Graham-Tomasi (1984) notes with reference to papers on producers' use of information under uncertainty by Chavas and Pope (1984) and Antonovitz and Roe (1984). One needs not only the likelihoods of true states of the world given researchers' estimates, but also the ex ante probabilities of researchers' findings. It is necessary to assume that, at least in expected value, policymakers can carry out a professional evaluation of the research findings they use. This is in large part what their professional staffs are for.

The Hirshleifer/Riley framework is also helpful in indicating why research can be valuable simply in virtue of reducing the range of uncertainty about the state of the world (e.g., demand elasticities) regardless of what point estimate is discovered. In figure 1, increased evidence moves us toward one state or the other (the probability of state 2 gets nearer 0 or 1). Whichever way the research program takes us, its ex ante value increases the further points R and T are pushed 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 generated the same utility no matter what the world was like, then there would be no point in policy research to determine what the world was like, as proposition 3 above says.)

Quantifying the Gains: (1) 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 policy research output. The value of this output will often be very 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 GDP as a measure of social well-being. In a practical approach we must frankly limit ourselves to a partial assessment. In this paper, I will for the most part limit the discussion in precisely the way we do in national income accounting: The value of social science research is taken as its contribution to society's aggregate real income.

A second limitation is to eschew any attempt to measure the total value of policy-oriented social science research. Instead, attention is restricted to marginal changes, or partial budgeting of social science research projects. That approach places the focus where it really is in practical discussion, on decreases or increases in social science research from existing levels.

The focus of our consideration on marginal valuation of aggregate real income effects is limiting in several key respects. It means, for example, no place for a concept of scientific value of research as opposed to its economic value. It means no place for purely cultural value of research — perhaps more an issue in the humanities, but also pertinent to the non-economic aspects of social science. Among the narrower economic issues, the focus on GDP means one gives no consideration to the income redistributional issues of whether any income, ethnic, or occupational group is harmed relation to another. We can and often should try to estimate distributional effects, e.g., he gains or losses of farmers, and indeed this information is often

sought as product of economic research. But these findings contribute 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 loses accruing to foreigners.

One may reasonably be unhappy with these limitations but I accept them all as the price that must be paid to proceed.

The roles of basic and applied social science research are assessed by the same criteria, but basic research is conceived as an input used in applied research. Basic research provides two crucial inputs: 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 it influences policy actions.

As an example, consider the case of economic assessments of the returns to agricultural production research. Evaluation of this body of work requires an estimate of the action-difference it made, and of the real income generated by that action difference. Neither estimate could be made without a far larger study than is feasible in this paper, and the absence of such estimates in the voluminous literature on agricultural research policy indicates the practical impossibility of producing such estimates with any confidence of accuracy (see reviews in Alston and Pardey, 1996; Huffman and Evenson, 1993). We can observe levels and trends in public research funding for nations and subnational areas, which are arguably influenced by economists' estimates of returns to research, but estimation of the action-difference requires the counterfactual level of spending had the economists' estimates not been published. Huffman and Evenson (1993, p. 238) argue that governments have acted only weakly in response to economists' estimates of returns. Their evidence is estimates that imply that returns to research have not been equalized over alternative categories of research spending. Interest-group politics appear more important.

Quantifying the Gains: (2) Redistribution

Because so much political action is aimed at redistribution of well-being, the question remains how to value policy research that bears on political choice involving redistribution. It is quite possible that export demand elasticity research influences policy not so much because the deadweight loss (in domestic national income) of acreage idling goes up with more elastic demand, but because more narrowly defined producer gains go down with more elastic demand. If we possessed a Bergsonian social welfare function, we could measure gains in such cases by increases in its level. A practical approximation would be the much-used weighted sum of individual gains and losses from a policy, where the weights are political preference indicators that vary among individuals or groups. The difficulty however is determining what weights to use. I prefer to use equal weights for everyone, which is a return to the national income maximizing approach of the preceding section.

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, namely 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, measuring the producers' returns from promotion activities, then we could justifiably claim 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) as returns to social science research.

An important aspect of policy-related social science research, whether used by policy-makers or interest groups, is that so much of it is oppositional. A research program on environmental regulation of agriculture, for example may consist of a range of studies that

increase our uncertainty about the state of the world. In terms of the Hirshleifer/Riley framework of figure 1, expected utility would be reduced by this research program. This result does not violate the proposition that new knowledge always has nonnegative value; rather, this is a research program that reduces (what we thought was) knowledge. In terms of our earlier, more formal discussion, it is a research program on the likelihood matrix (relating research findings to the true state of the world) that was used in the calculation of posterior probabilities.

The preceding suggests that our framework is too narrowly constructed, for surely there are situations in which it is better to be aware of our ignorance than to believe we know things that are not so. In fact, some political actions are constructed so as to be appropriate when we just don't know the state of the world. And, these actions are typically not a decision to do nothing.

Consider policy choice in the following situation. A large group of pesticides is considered possibly dangerous to human health, but 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 a research program which results not in information that makes us relatively certain that state 1 or state 2 prevails, but instead leaves us even more uncertain about the true state. This reduces the expected value of either policy option.

This is not the end of the story. Knowing now how little we know, perhaps we can devise a policy that is attended to this situation. Indeed, devising policies to fit the perceived situation

including its uncertainties and conflicts is the stock in trade of the successful policymaker. One might suggest policy A_3, a pilot program of regulation of a limited nature, along with pesticide use and residue monitoring, 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 either 1 or 2 but a good policy if we are uncertain about the state.

Here the following objection may be made. The state of the world <u>really is</u> 1 or 2, even if we don't know which. What we do know is that policy A_3 will be wrong no matter which state prevails. Indeed, this line of argument is typically raised against "compromise" policy proposals along the lines of A_3. An appropriate policy choice mechanism if this were the case might be to randomly choose policy A_1 or A_2, e.g., by a coin flip. However, the expected value of this approach, if $\pi = .5$, could well be less than the utility of A_3 at $\pi = .5$. What such a case would reflect is that if $\pi = .5$, a pilot program or study would help determine whether state 1 or state 2 really held. 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.[4]

When oppositional research leads to greater uncertainty about the state of the world, and information-generating policies like A_3 are not available, the research program may still be beneficial in terms of maximizing national income. This will occur when the policy choices being

[4] 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 the 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).

determined are those of interest groups themselves, e.g., what legislation 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 withdraw from the political debate. If the policies are alternative redistributional schemes, 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 argued generically for lower taxes or a balanced budget. This stance probably reflected doubt whether higher support prices benefited Farm Bureau members, who are typically the more business-oriented farmers, in the long run. To the extent that these doubts were engendered by the writings of agricultural economists who questioned the income-increasing effects of price supports (e.g., D. Gale Johnson, 1973), then the work of such economists contributed to national income.

Case Studies

Returns to Research on Value of Publicly Provided Information

The fairly substantial literature on valuing crop forecasting information, which dealt mostly with estimates produced by USDA, was discussed earlier as related to the evaluation of policy-related research. This literature has been used in discussion and debate on public spending for agricultural commodity forecasting. In addition to the papers cited earlier, some notable published research is 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 improving the accuracy of NASS (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 alone. 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, p. 10).

Irwin (1996) develops a further argument that observed market price effects of the release of USDA quantity reports are evidence of generally quicker response to changing conditions than would otherwise occur, and that this is a significant source of welfare gains.

A full Bayesian analysis of the social value of NASS's commodity estimates is not in the cards, but it may help in thinking about that value to suppose that the finding of the literature, that net social benefits of NASS data are large, is correct. To be concrete, suppose we are considering a cut of $10 million annually in the NASS commodity data program — as was in fact the case in the early 1990s.[5] Further suppose that the social value of the information lost would be $20 million annually, i.e., 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

---

[5] The FACT 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 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, p. 341).

assertions that some NASS surveys were generating negative net gains, and even the views of some legislators and citizens that NASS estimates as a whole are valueless or worse (e.g., Weber, 1997).

Given a $10 million annual net social gain ($20 million marginal information value minus $10 million marginal cost) from maintaining rather than cutting the NASS budget, to place a value on the relevant social science research, one has to estimate the change in the probability of budget reduction caused by the research findings. How is such research actually used in the policy process? The key places to look are OMB, and USDA's arguments to OMB, in the Executive Branch, and the Appropriations Committees in Congress along with Agriculture Committees' authorizations concerning agricultural data. My estimates here are necessarily subjective, a Bayesian prior updated by informal observation of ongoing Federal budget decisions. While interest groups, mostly commodity groups, are of course predominant in agricultural policy, with respect to NASS their voice is usually muted and the messages somewhat mixed. This leaves commodity groups' interests in maintaining NASS barely outweighing the generic budget cutters' arguments 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 the Administration and other experts they rely on for substantive judgment. The Administration and other experts themselves do not typically rely on particular ones of the studies cited, but rather on the general climate of opinion among agricultural economists — who are well represented in both Congressional committee staffs and the Executive Branch (and not just in USDA). And it is this climate, represented for example in the AAEA Task Force report quoted above, that provides the favorable reception to additional spending and resistance to cuts in NASS.

In short, in my opinion it is not extravagantly optimistic, and indeed is fairly conservative, to suppose that 50 percent of the reason $10 million was not cut from NASS in FY 1991 is attributable to the body of social science research on agricultural commodity data. Given the earlier estimate of a $10 million net gain for this decision ($20 million social benefit — $10 million cost), the expected value of the social science research is $5 million annually, for as long as its influence lasts.

This preceding is a marginal calculation, related to a $10 million NASS budget change. The corresponding total assessment would involve an estimate of what total federal agricultural statistical expenditures (NASS, Agricultural Censuses, parts of ERS and AMS) would be in the absence of the body of research findings on the value of all publicly generated information in agriculture. The literature contains no estimate, or even wild guess to my knowledge, of that value. But again, the policy influence is through a general sense that the public information enterprise does generate net social gains. That general sense would however probably exist among economists even in the absence of the research efforts on specific value-of-information issues cited earlier. So even if economics as a field of expertise is responsible for the agricultural statistics budget being as large as it is (albeit still too small by the predominant estimates), it would not be correct to attribute the gains to the specific empirical studies. Nonetheless these studies have strengthened the general view favorable to public data provision, in my own case quite considerably. So I feel quite comfortable with the $5 million net gain for the marginal case, and perhaps $10 million on a total basis — most of the real political action being at the margin even though only about 5 to 10 percent of the statistical spending is.

Rate of Return to Agricultural Research

A related, but larger and more widely known body of social science research deals with 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 parallel to the previous discussion of agricultural statistics. Again, prevailing estimates are that the social rate of return are quite high, well above going rates of interest, and even above the arguably more meaningfully comparable pre-tax rates of return to private entrepreneurial investment. Even with the complication of national, sub-national, and international sponsorship of agricultural production research, and the existence of private as well as public research undertakings, as well as various technical reasons why many studies overestimate the returns relevant at the current margin, estimates of net social gains to public investment prevail (see Alston and Pardey, Chs. 6 and 7).

Thus, we again have the question in positive politics of the role that rate-of-return research has played in causing the public research spending level. In this case I can't help being chastened by the fact that of the many authors who have worked on returns to research, and several who have carried out wide-ranging assessments of the literature, many if not all of whom are better acquainted with both the scientific and political issues than I am, none have ventured in print an assessment of what 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 what rate of return studies would suggest. However, that does not mean that policy makers are uninfluenced by returns-to-research studies, or that policymakers use the studies' 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 take the findings of researchers as providing certainty about the issues the research addresses.

While I can't provide a dollar-value assessment of the value of rate-of-return research, it is worth noting that the dollar-value stakes are much higher than in the statistical information issues. Instead of perhaps $150 million in U.S. annual public outlays, we have about $1.5 billion (federal and state). 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 5% above the relevant opportunity return, then the rate-of-return research has generated a social gain of $1500 \times .1 \times .05 = \$7.5$ million annually.

Trade Liberalization Studies

In the last 20 years a substantial quantity of both positive and normative social science research has been undertaken on the consequences of individual countries' international trade policies 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 which make it plausible that the value has been substantial. The most significant move toward global liberalization of trade in agricultural products since World War II has been achieved in agriculture agreement reached in the Uruguay Round of GATT negotiation, concluded in 1993 and being phased in over 6 years. In addition to relaxing some long-standing, seemingly intractable non-tariff 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 economics protection for domestic industries. The agreement also established important groundwork for further liberalization through negotiations to expand upon the Uruguay Round agreement when its currently agreed liberalization steps are completed. 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 on bringing Central European countries into the European Union, and permitting newly independent states of the former Soviet Union increased access 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 I will call the "bootstrap" approach suggested by Harberger (1954). He balanced the antimonopoly efforts of economists against his estimate of a $300+ million annual welfare gain if monopolies were replaced by competition in the U.S. economy.[6] Analogously, we can use the estimates of the World Bank (1986), Tyers and Anderson (1992), or others as summarized in Blandford (1990) to indicate net social benefits of $30 to 40 billion annually from complete global trade liberalization in agricultural commodities. Then, if trade were liberalized, and if that liberalization were attributable in part to economists' assessments, we could estimate the social value of economists' work. (But we must also include on the cost side the work of economists who have argued against trade liberalization.)

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 to complete liberalization have been achieved, and economists can claim 25 percent of the responsibility for the liberalization that has been

[6] The "bootstrap" label is to indicate that the measure of value of the research depends on economic values which the research itself measures.

achieved, we have .02 × .25, or .005 of $30 to $40 billion, that is, $150 to 200 million annually, as the worldwide benefit of the body of trade policy research of agricultural economists.

Analysis of National Commodity Market Intervention

National policies have been changed, usually in the direction of free-market reforms, in many countries in the past decade. New Zealand led the way in the industrial countries, in 1984-86, with reform of the Common Agricultural Policy of the European Union in the 1990s a much paler version but with a great deal larger deadweight losses to start from.[7] Many developing countries have implemented limited market-oriented reforms, notably Chile, Argentina, and China, with others such as India and Egypt beginning to look like starting out on similar paths. And, the former Soviet sphere is rebuilding its agricultural policies in its post-Communist evolution. Agricultural economists have been participants in analysis and debate in all these countries. In the cases where I have some observational experience — New Zealand, Hungary, Poland, Latvia, Ukraine, India, and Egypt — these is evidence that contributions of economists have made a difference in the form and content of agricultural policies. But the impact appears marginal, and the extent of reform has typically been disappointing in not being extensive enough to materially reduce economic waste and inefficiency.

It would be going too far to venture even a guess at quantifying the overall value of economists' action-inducing influences 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, namely the willingness to pay for policy research. It is reasonable to use the public's revealed willingness to pay at least to

[7] Blandford (1990, p. 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.

place a provisional lower bound on the analytical services provided by social scientists employed by government agencies.[8] This approach suggests valuing the output by looking at costs, as is often done in measuring the services of lawyers, accountants, entertainers, and other intangible-service providers. However, because of arguments such as those in footnote 8, it is useful to consider attempts to measure the value of the output independently.

United States. I will consider in more detail, and more parochially, research on U.S. farm programs. I have estimated the deadweight loss for these programs as of 1985-87 at $4 to 5 billion annually, mostly die to idling of productive cropland (Gardner, 1990, p. 59). In 1990, and more dramatically in 1996 with the enactment of the FAIR Act, U.S. policy reforms have been undertaken which reduce the deadweight losses substantially, arguably by three-fourths or more, principally by eliminating annual acreage-idling programs. I have argued in some detail (Gardner, 1996) that agricultural economists made a significant contribution to the achievement of these reforms. The influence did not turn on quantitative estimates of deadweight loss, but rather on the work of many economists on a variety of topics all generating results that the commodity programs were costing taxpayers and consumers many billion of dollars, while accomplishing much less for farmers. These findings influenced not legislators directly, but newspaper editorialists, government experts (Executive and Legislative staffs), and commodity group representatives.

---

[8] The lower bound is "provisional" because two further considerations may weaken the case. First, some theorizing about bureaucracies has suggested employees of government agencies may manage to be paid more than the value of their output (see parallel arguments on agricultural production research in Pasour and Johnson, 1982). Second, some public research and analysis is aimed largely if not entirely at responding to, or refuting economic assessments by private-sector interests or foreign governments. To this extent we might find national income not reduced or even increased if both sides would reduce their efforts. (A similar point could be made concerning lawyers' services, brand advertising, and military preparedness).

It has to be recognized that economists have not spoken with one voice on agricultural policy. Pasour (1988) emphasizes the role of agricultural economists in putting U.S. commodity programs into place in the 1920s and 1930s. However, it would be wrong to characterize agricultural economists as arguing both sides of policy issues, or first one side and then the other of the same issue, and so largely canceling themselves out. There really does appear to have been a consensus in favor of government action in commodity markets before World War II, which evolved to a consensus opposing such actions in the 1980s and 1990s.[9] And 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 in the post-World War II period.

The key point is that these policy issues involve very difficult scientific questions as well as potent political forces. Therefore the success of the body of policy research, in reducing the range of uncertainty about the effects of public choices, granted the contentious nature and great variation in quality of the research, is a true indicator of ex ante gains that should be attributed to the research.

Beyond the more sweeping work on agricultural policy, a larger corpus of economic research has been devoted to particular policy issues that must be resolved in the implementation of farm programs. Examples are: annual determination of the percentage of acreage in set-aside programs, the level of sugar import quotas to achieve legislated price targets, selection criteria for land offered by farmers for inclusion in the Conservation Reserve Program, the relative levels of butter and non-fat dry milk purchase prices to achieve the legislated farm price of milk, levels of

---

[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).

export bonuses offered under the Export Enhancement Program, and rules for implementing pilot programs in revenue and crop insurance. Alternatives in these and a hundred similar decisions make many millions of dollars of difference in the efficiency loss resulting from commodity programs. The net benefits are not quantifiable, but this is a case where I feel more confident in appealing to the quasi-market mechanism according to which government agencies pay for the research because they believe the benefits from the findings justify the staff costs. I had several years' experience in negotiations involving the Economics Research Service budget, within the U.S. Department of Agriculture, between USDA and the White House Office of Management and Budget (OMB), and between the Bush Administration and Congressional Appropriation Committees. In these discussions, OMB made the most serious attempt to weigh the issue of the value of services provided by ERS in relation to the agency's costs (in considering why the agriculture sector was endowed with so many more government economists, relative to the size of the underlying sector than other parts of the U.S. economy). OMB ended up accepting a $50+ million annual budget for USDA's Economic Research Service principally because of the ex ante value of the product in ongoing policy formulation.

Research on the Effects of Advertising and Promotion

A narrower and more precisely defined area of inquiry is studies of the effectiveness of generic product promotion, generic meaning not specific to a brand-identified product. Such promotion is typically funded by producer groups, at least in large part, and the issue arises whether funding promotion is profitable for the industry.

A recent example is Blisard, Blaylock, and Smallwood (1996). They estimate that over a ten-year period, 1984-1994, generic advertising raised fluid milk sales 5.6 percent, or 12.8 billion pounds. Fluid milk advertising expenditures were $296 million over this period, of which $110

million was attributed to a 15-cent per assessment per hundredweight of milk sold, mandated by the Dairy and Tobacco Adjustment Act of 1983. Blisard et al. estimate the "gain per act-increased advertising dollar" at 117 pounds of additional milk consumption.

Should (a) milk producers, and (b) U.S. citizens at large, rejoice in this finding (assuming for the moment they can believe it)? To give a rough answer for (a), supposing the elasticity of demand, η, is in the range -.5 to -1.0, and the elasticity of supply, ε, is in the range .5 to 1.5 (over a ten-year period of adjustment), then the price effect of a 5.6 percent demand shift is in the range $0.056/(\varepsilon - \eta) = .022$ to .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 $.24 and $.62 per hundredweight. With 2.3 billion hundredweight sold (= 12.8/.056/100), the gain to producers, measured by producers' surplus, is $550 to $1400 million. Since this gain was achieved by spending $110 million, the implication of the research finding (although not stated in this way by Blisard et al.) 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 milk consumers' well-being. There is no standard method in welfare economics for accomplishing this. If the advertising changed tastes, an underlying assumption of consumer benefit estimates has been violated. If the advertising conveyed information, and the increased willingness of consumers to pay for milk measures the value of this information to each consumer, then consumer are no worse off. The producers' net gain is a rough measure of society's net gain.[10]

---

[10] The main possibility that would render this argument invalid is that only some consumers are influenced by the advertising. Their added consumption drives up that price for all consumers, and the added costs to the uninfluenced consumers should be counted as a loss attributable to the program.

For purposes of this paper, the issue is how to value the social science research that led to the net-gain estimate, i.e., the value of studies such as that of Blisard, Blaylock, and Smallwood. Many such studies have been carried out, and they tend to show substantial net benefits, at least to producers, due to promotion programs.[11] But the accuracy of the findings are open to question, probably more so then the studies of gains to agricultural research. (With reference to export promotion programs, see Sumner, 1995, pp. 106-109, and works he cites.) One of the policy actions in the case of promotion programs is producers' decision through voting whether to assess themselves or not. 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 terms of the 2-state, 2-policy version of Hirshleifer-Riley model of valuation, research on the consequences of generic advertising, even when it delivers a message of support for its message [advertising pays (S_1), as against advertising does not pay (S_2)], may have little effect on the action chosen [impose an assessment on producers (A_1), as opposed to not imposing an assessment (A_2)]. The reason is that the perceived quality of the research findings, as represented in the likelihood matrix of the true state given research findings about the state, is low in the sense that likelihood of S_2 is not much different whether the research estimates 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 is going to 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

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

the research, which is possibly the situation for research on the value of generic commodity promotion.

In the case of promotion programs, and of some other policy-related social science research, there is a further 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 orange juice promotion. So instead of relying on public cost-benefit analysis, we have a market test for the ex ante value of social science research. Even if such market decisions are questionable, as they have been questioned for example when people pay for stock market advice, we should not be too quick to write off the research as not worth what is paid for it when the payment is voluntarily made by people who economists' normally assume are rational.

Costs of Agricultural Policy Research

The costs of agricultural policy research are mainly personnel costs. The Directory of the American Agricultural Economics Association lists about 730 people as having the subject matter specialization of agricultural policy (S840). Many members list more than one specialization, however. The directory lists about 3,400 members, and 4,800 reported specialties. My estimate is 3,400 × 730/4,800 = 520 full-time equivalent AAEA members who spend their research time on agricultural policy. Assuming an average of 40% of the average academic working year is spent on research (as opposed to teaching and extension), this implies 210 agricultural policy research years, which at an average cost of $60,000 plus 50% benefits and overhead support amounts to $90,000 × 210 = $19 million annually. However, some agricultural policy research is done under specialties other than agricultural policy, e.g., resource economics, and some agricultural policy researchers are not members of the AAEA. To roughly accommodate these omissions, 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 plausibly add up to $40 million annually. Without wishing to claim much for the rather sketchy case studies, I suggested about a $5 million annual return from value-of-information research and $7.5 million for returns-to-research studies. Assuming the United States reaps 20 percent of the trade liberalization gains, research output is that area would be valued at $30 to 40 million annually. Assuming one-third of the Economic Research Service budget is implementation research whose value is what is paid for it, the annual benefit is $18 million. The value of broader agricultural policy research is impossible to estimate with any pretense of precision, but to have a figure to work with, assume that FAIR Act reforms eliminate one-half of $4 billion in deadweight losses of pre-reform policies, and that 10 percent of the credit for this achievement goes to agricultural policy research findings. This implies a $200 million annual benefit from this research.

Summing up, indicated benefits to the U.S. economy are 5 + 7.5 + 30 to 40 + 18 + 200 $\cong$ $260 to 270 million returns resulting from $40 million spent on agricultural policy research. The specificity of these numbers half-fills me with guilt. Taking into account remaining left-out costs, left-out benefits (some policy issues were not covered, e.g., soil conservation, environmental and food safety regulation, domestic marketing policy), the lag between research findings and policy results, and the great uncertainty in all the benefit values given, one might double the costs and place a doubling and halving range around the benefits to say that we have benefits of $130 to $540 million resulting from costs of about $80 to $100 million. Even with the most pessimistic view of annual benefits ($130 million) and costs ($100 million), 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 reduction in uncertainty about the true state. The value of proposed research is the ex ante addition to the expected value of policymakers' objective function. This value is nonnegative for decisionmakers who can accurately assess the likelihood that a research program will reveal the true state. But, especially for research that is not expected to make a significant contribution to identifying the true state, the value of the research may well not justify the costs.

The case studies of agricultural policy research considered in this paper suggest 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, I feel justified in claiming that the preceding conceptual framework and empirical evidence provides 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 on an overall or aggregate basis, policy-related social science research in agriculture could be managed more efficiently. A large number of policy-related studies and publications, even the entire output of some researchers, could have been omitted without loss. So we would have needed to fund only a part, and perhaps even a small part, of the research that has been funded.

And with better management, we could fund less policy research in the future without loss of expected future net benefits.

The flaw in that conclusion is its presumption that better management is feasible, in the sense of picking winners in advance. It is likely that, as with drilling for oil, recruiting football players, or breeding race horses, one has to back the low-return efforts in order to obtain the successes. As a practical matter, that is, if we were to cut the policy research budget, we would be likely to end up cutting almost as many (or the nearly same percentage of) productive as unproductive projects. On the other hand, it might be worthwhile to fund additional research on how to "pick winners" and in other ways manage policy research better.

References

AAEA Data Task Force. "Final Report," mimeo, University of California, Berkeley, December 1996.

Alston, Julian M., and Philip G. Pardey. Making Science Pay: The Economics of Agricultural R&D Policy, Washington, D.C.: AEI Press, 1996.

Antonovitz, Frances, and Terry Roe. "The Value of a Rational Expectations Forecast in a Risky Market," Am. Jour. Agr. Econ. 66 (Dec. 1984): 717-723.

Babcock, Bruce A. "The Value of Weather Information in Market Equilibrium," Am. Jour. Agr. Econ. 72 (Feb. 1990): 63-72.

Blandford, David. "The Costs of Agricultural Protection and the Difference Free Trade Would Make," in F. Sanderson, ed., Agricultural Protectionism, Washington, D.C.; Resources for the Future, 1990, pp. 398-432.

Bradford, D.F., and H.H. Kelejian. "The Value of Information for Crop Forecasting with Bayesian Speculators: Theory and Empirical Results," Bell Jour. of Economics 9 (1978): 23-44.

Bullock, J.B. "Social Costs Caused by Errors in Agricultural Production Forecasts," Am. Jour. Agr. Econ. 58 (Feb. 1976): 76-80.

Chavas, Jean-Paul, and Rulon D. Pope. "Information: Its Value and Measurement," Am. Jour. Agr. Econ. 66 (Dec. 1984): 705-710.

Clarke, Sally H. Regulation and the Revolution in U.S. Farm Productivity, New York: Cambridge University Press, 1994.

Cochrane, Willard W. The Development of American Agriculture, Minneapolis: University of Minnesota Press, 1993.

Doll, J.P. "Obtaining Preliminary Bayesian Estimates of the Value of a Weather Forecast," Am. Jour. Agr. Econ. 53 (1971):651-5.

Fox, Glenn. "Is the U.S. Really Underinvesting in Agricultural Research?" Am. Jour. Agr. Econ. 67 (1985): 806-12.

Freebairn, J.W. "Welfare Implications of More Accurate Rational Forecast Prices," Australian Jour. Agr. Econ. 20 (1976): 92-102.

Gardner, Bruce L. "The United States," in F. Sanderson, ed., Agricultural Protectionism, Washington, D.C.: RFF, pp. 19-63.

_____. "Agricultural Economists in the Policy Arena: The 1995 Farm Bill," Rev. Agr. Econ. 18 (1996): 155-65.

Graham-Tomasi, Theodore. "Discussion," Am. Jour. Agr. Econ. 66 (Dec. 1984): 724-25.

Harberger, Arnold C. "Monopoly and Resource Allocation," Am. Econ. Rev. (May 1954): 77-87.

Hayami, Y. and Willis Peterson. "Social Returns to Public Information Services: Statistical Reporting of U.S. Farm Commodities," Am. Econ. Rev. 62 (1972): 119-30.

Hirshleifer, Jack, and John G. Riley. The Analytics of Uncertainty and Information. New York: Cambridge Univ. Press, 1992.

Huffman, Wallace E., and Robert E. Evenson. Science for Agriculture, Ames: Iowa State Univ. Press, 1993.

Irwin, Scott. "An Essay on the Value of Public Situation and Outlook Programs," mimeo, Ohio State University, October 1996.

Johnson, D. Gale. World Agriculture in Disarray, New York: St. Martins Press, 1973.

Lave, L.B. "The Value of Better Weather Information to the Raisin Industry," Econometrica 31 (1963): 151-64.

Lindner, Bob. "Toward a Framework for Evaluating Agricultural Economics Research," Australian Jour. Agr. Econ. 31 (August 1987): 95-111.

Norton, G. W. and G.E. Schuh. "Evaluating Returns to Social Science Research: Issues and Possible Methods," in Fishel, Paulsen, and Sundquist, eds., Evaluation of Agricultural Research, Univ. of Minnesota Agr. Exp. Sta. Misc. Pub. No. 8-1981, April 1981.

Pasour, E.C., Jr. "Financial Support and Freedom of Inquiry in Agricultural Economics." *Minerva* 26 (1988): 31-52.

Pasour, E.C., Jr., and M.A. Johnson. "Bureaucratic Productivity: The Case of Agricultural Research Revisited," Public Choice 39 (1982): 301-17.

Ruttan, V.W. "Bureaucratic Productivity: The Case of Agricultural Research — A Rejoinder," Public Choice 39 (1982).

Ryan, J.G., and R.K. Perrin. "Fertilizer Response Information and Income Gains," Am. Jour. Agr. Econ. 56 (1974): 337-43.

Sumner, Daniel A. Agricultural Trade Policy, Washington, D.C.: AEI Press, 1995.

Tyers, Rod, and Kym Anderson. Disarray in World Food Markets, New York: Cambridge University Press, 1992.

U.S. Office of Management and Budget. Budget of the United States Government, Fiscal Year 1992, U.S. Government Printing Office, Feb. 1991.

Weber, Barry. "Let's Put an End to USDA's Commodity Reports," Progressive Farmer, Jan. 1997.

World Bank. World Development Report, 1986, New York: Oxford Univ. Press, 1986.

Figure 1. Value of a Research Program to Estimate the Elasticity of Export Demand

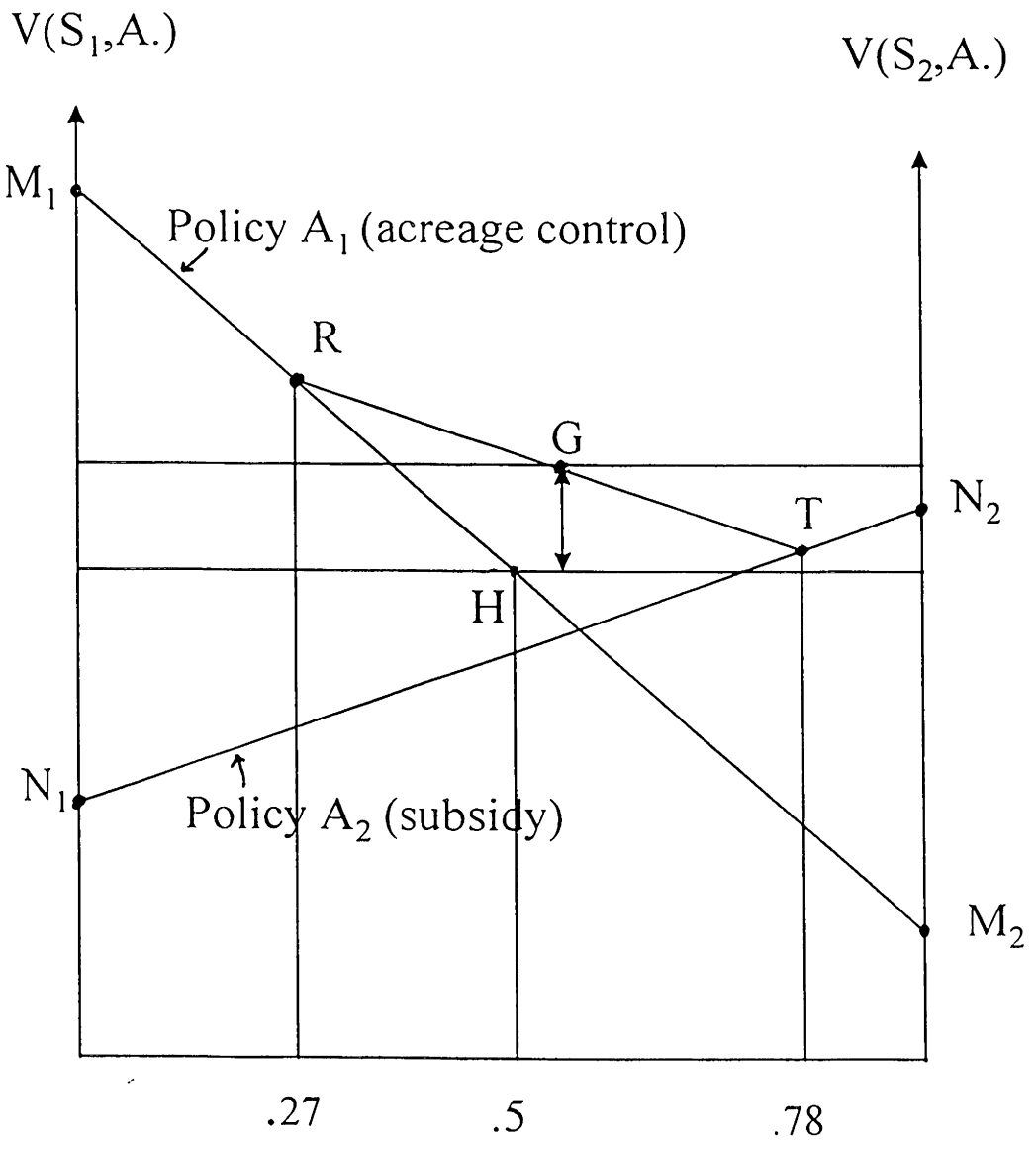
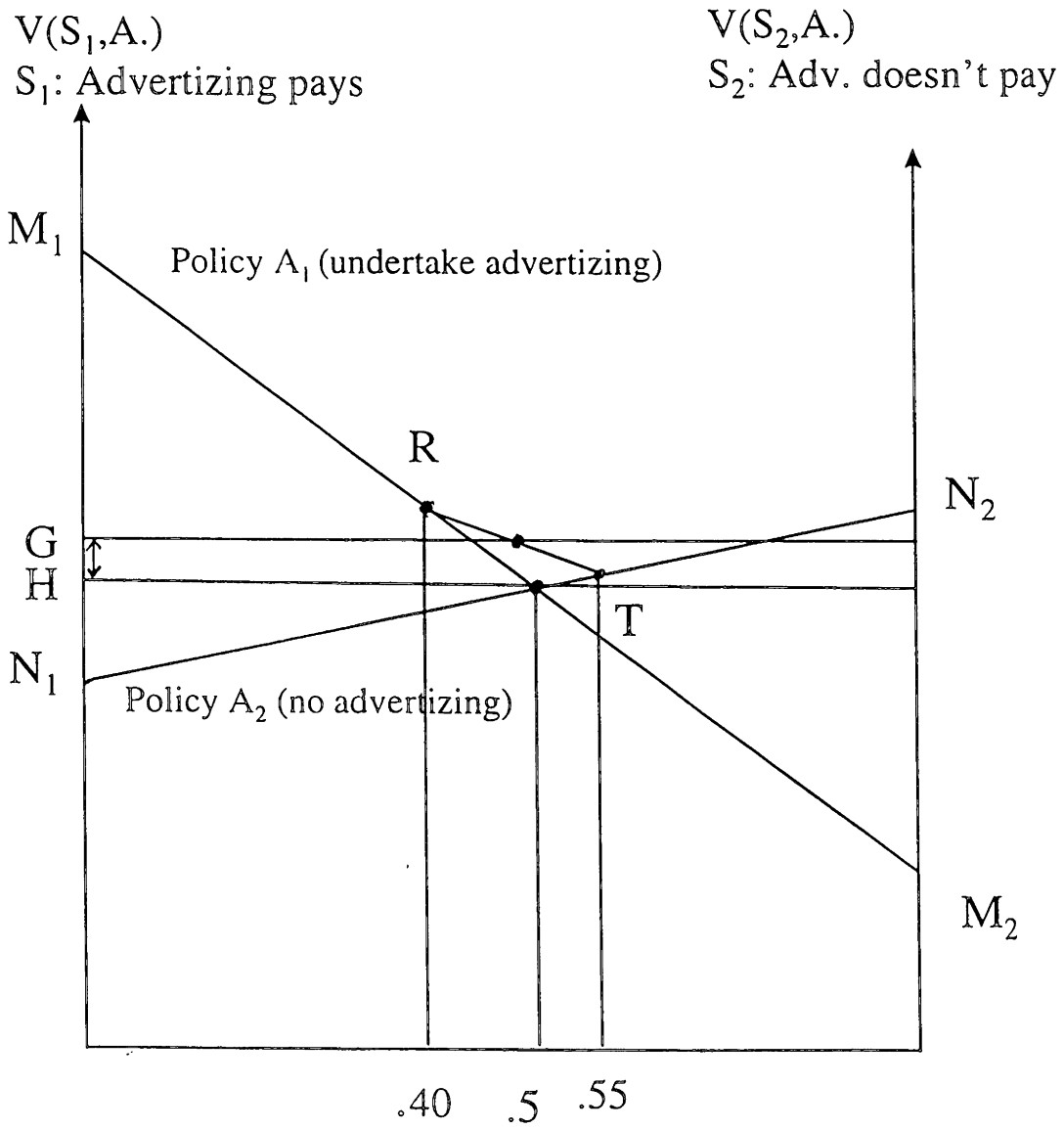


Figure 2. Value of a Research Program to Estimate Whether Advertizing Pays for an Industry



Ex ante value of the research program: distance between G and H