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AGRICULTURAL ECONOMICS

SCHOOL OF AGRICULTURE

TOWARD A FRAMEWORK FOR
EVALUATING AGRICULTURAL ECONOMICS RESEARCH*

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Agricultural Economics
Discussion Paper 1/87

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ABSTRACT

Agricultural economists need to evaluate their own research priorities. The main difficulty in doing so is to value the types of information generated by economic research. Bayesian decision theory provides a framework for valuing information, and the results of selected studies using this methodology are collated. Most of the other determinants of research priorities can be encapsulated in a target return ratio measure. How such a framework might be used is illustrated by three "hypotheticals".

> It is easy to choose a subject for a distinguished lecture like this, before a large and critical audience with a wide range of interests. You need a topic that is absolutely contemporary, but somehow perennial. It should survey a broad field, without being superficial or vague. It should probably bear some relation to economic policy, but of course it must have some serious analytical foundations. It is nice if the topic has an important literature in the past of our subject--a literature which you can summarize brilliantly in about eleven minutes--but it better be something in which economists are interested today, and it should appropriately be a subject you have worked on yourself. The lecture should have some technical interest, because you can't waffle for a whole hour to a room full of professionals, but it is hardly the occasion to use a blackboard (Solow, 1974, p.1).

Unlike Solow (1974), I did not find it easy to choose a topic for this address, but I did find his criteria helpful in narrowing the choice. What I would like to do is to explore whether the framework we have developed to analyse research priorities for our colleagues in agricultural science can be adopted to the evaluation of research priorities for our own discipline. Before doing so, I want to record my gratitude to the members of this society for the opportunity and the stimulus to put down on paper some thoughts that have been slowly germinating in the back of my mind.

My interest in the scientific process was first stimulated by Professor W.K. Bryant, who introduced me to some of the literature on the philosophy of science while I was studying at the University of Minnesota. This provided the foundation for an appreciation of two remarkable books by Koestler (1959, 1964), the first being <u>The Sleepwalkers: The History of Man's Changing Vision of the Universe</u> (1959) and the second being <u>The Act of Creation</u> (1964). The main theme of both these books is how subconscious 'bisociative' thinking (i.e., the connection of previously unrelated mental frames of reference) provides the basis for creative activity, including the process of scientific discovery. As a corollary, the human factor, and in particular personal motivation, is an important determinant of research productivity, so that individual interests should influence research priorities.

Some time later I became involved in the assessment of priorities for agricultural research. More recently I have come to the view that all of the essential building blocks needed for the objective evaluation of agricultural economics research priorities are already present in the literature. I would like to single out three particular papers which were instrumental in the development of my ideas. They are a paper by Norton and Schuh (1981) which I will discuss shortly, the paper by Ruttan (1984) on the supply and demand for agricultural policy research, and the assessment by Edwards (1985) of Fisher's (1985) policy research prejudices, most of which I share. In this address, I propose to sketch out some of the foundations of a conceptual framework to evaluate our professional activities in a more objective manner.

In doing so, I am going to bypass the particular problems of so-called basic research, which presents one of the great challenges to the evaluation of all types of research. It is not a challenge which I intend to take up today. Instead I am going to restrict my discussion to 'mission-oriented' agricultural economics research, defined to include all professional activities undertaken by agricultural economists which generated information of direct relevance to consumption, production, investment or policy choices. At one extreme it includes quite straightforward data-gathering exercises involving little or no creative or analytical component, such as the BAE's crop forecasting program. At the other end of the spectrum are conceptually difficult and highly creative efforts to push back the frontiers of knowledge of our discipline. An example is the attempt by Bardsley and Harris (1986) to econometrically estimate average attitudes to risk of Australian farmers.

Although somewhat dated by now, the classification of Australian agricultural economics publications by Phillips (1975) suggests that about two-thirds of all research meets the criterion for 'mission-oriented' research.[1] Casual appraisal of Table 2 in Richardson (1986) indicates the proportions have not changed much in the intervening decade.

The plan of the address is as follows. First, I propose to discuss the ramifications of the premise that the typical product of social science research is information. Next, I will review a selective sample of the literature dealing with the estimation of the value of information. From this review I will attempt to delineate some guidelines which could be used by agricultural economists in the assessment of their activities. I then integrate this suggested approach for information valuation into the general, and conventional, framework for the assessment of research priorities. Finally, I will illustrate my proposed approach by discussing three selected areas of agricultural economics research.

Before I proceed though, I want to add that I do not under-estimate the problems involved in evaluating our research activities. As a profession, we need to guard against the onset of diminishing returns which would set in if we devoted too much of our efforts to the choice of what to do, rather than getting on and doing something. By the same token, to never evaluate our alternatives before deciding what to do is the antithesis of the theoretical core of our

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1. Phillips classified 269 articles, equal to 70 per cent of the 383 total, as either policy, empirical problem solving, or descriptive. The other types were theory, methodology, deductive problem solving, and review articles.

discipline. Because of the insights gained from reading Koestler's (1959, 1964) books, I also would prefer to see any evaluation take the form of self assessment by all members of the profession of how best to allocate their own time between alternative professional activities. However, I am not arguing for any formal, quantitative, complicated or time-consuming analysis. Instead, what is needed is a conceptual framework which need only be used in an informal, subjective manner from time to time when mapping out career plans and making similar pivotal choices.

Furthermore, there is the issue of our professional credibility. For some years, Australian agricultural economists have been in the forefront of international work on the evaluation of priorities for agricultural science research. Some of this work has been highly sophisticated, formal and analytically rigorous. However, much of it resulted from the application of basic economic principles mixed with some hard facts, a bit of logical reasoning and more than the odd gut-feeling. If we are not willing to apply similar methods in trying to improve the efficiency of our own actions, then we can scarcely expect our attempts to do so with respect to agricultural science research to have any credibility.

What Benefit Agricultural Economics Research?

Certain types of farm management and production economics research are directly analogous to agricultural science research in the sense that the aim is to develop innovations in the form of new decision-making strategies with the potential, if adopted, to lower average costs of production. Examples include research on management strategies for integrated pest management, the application of linear programming to whole farm planning, and the development of optimal marketing rules. Comparable procedures to those used to estimate the benefits from scientific research can be applied to these cases. However, it is clear that most social science research does not generate innovations in the above sense, and that a different conceptual framework is needed if the benefits of such research are to be valued.

A pioneering attempt to provide such a framework was made by Norton and Schuh (1981). Unfortunately their work is not very accessible and has not received the publicity it deserves, so first let me briefly outline their contribution.

Norton and Schuh (1981) start from the proposition that the output from social science research is information rather than a new or improved product.[2] In their view, even if this information leads to someone producing a better product, it is not the research *per se* that produces the product. The types of information provided by social science research are classified by Norton and Schuh (1981) into seven basic categories as follows: (a) management information, (b) price information, (c) institutional information, (d) product and environmental quality information, (e) human nutritional information, (f) information to aid in adjusting to disequilibria, (g) information to aid in the reduction of rural poverty.

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2. Presumably they define a new product broadly enough to encompass process innovations.

Thus the crux of the problem in evaluating social science research is how to value the information that it produces. According to Eisgruber (1978, p.901) 'neither theory nor methodology exist to address adequately the economics of information, and, until recently, little effort was made to overcome this deficiency'. Factors which Eisgruber believed contributed to this deficiency include the lack of a market price to use as a basis for valuing information, the fact that information is not a physical good and that its impact is not easily observable or measurable (so that existing econometric techniques are of little assistance in valuing information) and that public and private values of information may differ significantly.

If induction cannot be used to value research information, then it is necessary to rely on deduction. To do so, first consider the following list of economically significant attributes of information identified by Hirshleifer (1973): (a) applicability, referring to the number of decisions, or decision makers, to which the information is applicable; (b) content, referring to the uncertain state to which the information is applicable (eg, consumer(s) tastes, endowments or resources, technology, and market characteristics such as price or quality of traded goods); (c) certainty, referring to the degree of concentration of posterior belief distributions dictated by the information (fully certain information assigns 100 per cent probability to a single value of the variable being predicted); (d) decision relevance, referring to information quality (sometimes referred to as reliability, or degree of informativeness).

As an aside, I would add to this list the fact that information quality is of particular importance in a way not matched by any other good. There are at least two dimensions to information quality, one being degree of reliability or informativeness and the other being veracity or falsity. At one extreme, information reliability can eliminate all uncertainty (that is, be perfectly informative) and at the other extreme it can be totally uninformative (that is, is disregarded totally by the decision maker). However, even if information is informative in the sense of reducing the spread of the decision maker's subjective beliefs, such 'reduction in uncertainty' might lead to worse rather than better decisions if the process generating the information is biased. In other words, apparent information may in fact be misinformation, in which case more is worse rather than better. As a profession we have barely scratched the surface of the economics of disinformation. To sum up, there are extremely difficult conceptual problems associated with the evaluation of information for which we do not yet have all of the answers. Consequently, at least for the time being the evaluation of social science research must remain an imperfect process.

Just as a precondition for applied agricultural research to contribute to economic growth through greater efficiency is the adoption of the innovations generated by the research, the analogous condition for applied agricultural economics research is that the information so generated must be used as an input to decisions made by consumers, producers and/or government. Therefore a starting point for the evaluation of a particular area of economic research is to establish the content of the information produced by the research (that is, to delineate those uncertain states about which it is intended to provide information). Once established, this content can be used to identify those decisions which might be influenced by the research results. This step is equivalent to the identification of the group of potential adopters of an agricultural process innovation.

In identifying those decisions for which different types of research information might be valuable, farmers are the most obvious, but not the only potential users of agricultural economics research information. Other potential users of agricultural economics information include agribusiness and consumers, as well as the voters, politicians and bureaucrats that comprise government. Indeed, Ethridge (1985, p.1) argues that 'it is the farm sector that must be accessed to supply most of the information, but it is predominantly other sectors that use and interpret the information'. He goes on to suggest that non-farm agribusinesses are likely to utilise agricultural data for the following types of decisions: 'production and inventory, peak workloads, hiring practices, pricing strategies, magnitude and timing of investment in capital and research, . . . market development activities, etc'.

If we even consistently attempted to enumerate the number of susceptible decisions before initiating new research, I am sure that our research priority setting would benefit from the discipline and objectivity so imposed, not least because this measure provides a crude proxy for the all important scale effect.[3] To be useful though, evaluation needs to go beyond consideration of the scale effect to also identify any systematic differences in the 'per decision' value for various types of research information. While this value of information per unit of scale is likely to depend on firm specific variables as well as on the type of information and on the nature of decision, it will be convenient to ignore this inter-firm variability in order to concentrate in this lecture on the determinants of the value of information for an 'average' decision maker. Aggregate research benefits can then be treated as the product of the number of decisions (scale) and the average value per decision. Refinements to this admitted over-simplification are left to a later date.

Various methods of valuing information have been reported in the literature. For instance, Eisgruber (1978, p.903) identified the following three different schools of thought in his literature review: (a) decision theoretic approach, (b) net social benefit approach, and (c) scoring approach, but even he admitted that the latter scarcely deserves the title of an 'economic theory of information'. Freebairn (1978) also identified three alternative models for quantifying the benefits of outlook information, but included the 'information theory' model in lieu of the scoring approach.

Marschak (1968) argues that the basis of 'information theory' is an engineering-type concept which only permits the amount of information transmitted by a communication channel to be quantified in physical units. In his view this theory does not naturally lend itself to assigning economic values to 'inquiry' (that is, the production of data) nor to 'deciding' (that is, the use of data by decision makers). Theil (1967) has shown how the basic framework can be so extended, but as Leuthold (1971) demonstrates, when information theory is used to put an economic value on information, the approach is effectively the same as that of Bayesian decision theory. Later in this talk, I hope to be able to persuade you that, at least for market outlook research, this functional equivalence also extends to the net social benefit approach.

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3. In evaluation of agricultural research, the value of production typically proves to be the most important determinant of estimated net research returns.

The Bayesian Decision Theoretic Approach to Information Evaluation

To my mind, Bayesian decision theory provides the most logical and insightful conceptual framework for valuing information.[4] It is not hard to make Bayesian decision theory appear fiendishly difficult, but the essential concepts are really quite simple. To facilitate the exposition, consider the case of a potential adopter faced with a simple two-action decision problem of whether to adopt a newly developed process innovation on the one hand, or on the other to persist with previous practice.[5] The various consequences of this choice are depicted in Figure 1.

Despite the decision maker's uncertainty about whether innovation adoption is in fact beneficial or not, a choice must be, and is made each time an opportunity exists to alter the method of production. Hence the possibility exists that the chosen act will prove, ex post, to be suboptimal. In this event, there is an associated opportunity cost (or loss or regret) relative to that act which is in fact optimal. Of course, the possibility also exists that the chosen act is identical to the (truly) optimal act, in which case there would be zero opportunity loss. A priori, both possibilities need to be countenanced and the expected value of this opportunity loss is commonly referred to as the cost of uncertainty.

In addition, there is another conceptually distinct cost of imperfect knowledge which has been the subject of much greater attention by economists. This is the cost of risk, the size of which is a function both of perceived level of risk, and of attitudes to risk of decision makers.

Information derives its value from its potential to reduce either or both of these costs. This perspective is of considerable practical importance, because at best information can eliminate both of these costs. Consequently, if we can estimate the cost of uncertainty, and the cost of risk, however crudely, then immediately we have an upper bound on the possible value of the research being evaluated.

Of course, information is rarely, if ever, perfect, and in practice the expected value of imperfect information will be some proportion of the potential expected value of perfect information. Precise quantification of the actual reduction in the costs of

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4. With some ingenuity, the output of most social science research can be treated as conforming to the decision-theoretical conceptual framework for valuing information even when the information is not a direct input to a productive unit. For instance, the avowed aim of some policy research is not to identify the best alternative policy instrument, but rather to inform members of the body politic of the costs to them of a particular policy option. However, presumably the ultimate intent in doing so is to influence the policy instrument selected by political processes. A classic case in point is some recent research by the BAE on the domestic costs of CAP to EEC voters. Such activities can be viewed as attempts to alter voters' choices of political parties, and thereby influence 'adoption' of 'desirable' policies. While it would be possible in principle to estimate the value of such information to voters, the practical difficulties of doing so are severe given our current knowledge of voter behaviour.
5. For the sake of simplicity, the possibility of partial adoption is ignored.

uncertainty and/or risk can be computationally difficult,[6] but guidelines for obtaining crude bounded 'guesstimates' can be derived from the basic principles of Bayesian decision theory, and/or from established findings in the literature.

An established result from both statistical sampling theory and business decision theory is that the value of information increases monotonically with extra information (that is, more messages), even if at a decreasing rate. Value also is a monotonic non-decreasing function of the accuracy, reliability, or precision of the information. Therefore the guesstimation of the value that a decision maker will place on information produced by social science research can be simplified by partitioning it into two stages as follows: (a) guesstimate the components of the expected value of perfect information (that is, the cost of uncertainty, and the cost of risk); (b) guesstimate the likely proportionate reduction in expected value of perfect information achievable by the particular type of research being evaluated.

This suggested partitioning is based on the view that expected value of perfect information is state specific, while the achievable degree of reduction in expected value of perfect information is information specific. In other words, expected value of perfect information is determined solely by the characteristics of those decisions likely to be affected by the results of the research. These decision characteristics include: (a) flexibility (that is, the range of alternative courses of action available); (b) payoff sensitivity (that is, sensitivity of outcomes to decision choices); (c) preference sensitivity (that is, sensitivity of choices and of returns to attitudes to risk, etc); (d) degree of prior uncertainty or imperfect knowledge (as indexed by variance of prior beliefs).

Thus expected value of perfect information is generally much simpler to compute than the actual value of a particular type of information, as the former is independent of the characteristics of the information, and underlying research process being evaluated. This raises the possibility of economising on computation costs by using a common set of guesstimates of expected value of perfect information in the evaluation of different types of agricultural economics research.

Conversely, the second stage, involving guesstimation of the proportionate reduction in expected value of perfect information, is determined principally by the characteristics of the information system in question, and so offers less scope for exploiting economies of size. In formal Bayesian terms, the two relevant characteristics of the information set are the number of messages, and the 'informativeness' of these messages as indexed by the likelihood function. In everyday terms, the greater the accuracy, or reliability, or precision of the information, the higher the proportionate reduction in expected value of perfect information.

For the simple two-action, two-state of the world type of problem discussed earlier in this talk, the perceived accuracy of information from research can be expressed in the form of likelihood

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6. For instance, see Byerlee and Anderson (1982) or, in different context, Campbell and Lindner (1985).

probabilities.[7] In an empirical study of the value of a weather forecast assuming multiple discrete states, Doll (1971) found the marginal value of information to be an increasing function of the likelihood probability. I have obtained a comparable result for a simple two-act/two-state exploration problem. In the latter case, though, the relationship is approximately linear. For the purpose of research evaluation, assuming a proportional relationship certainly simplifies the process and may well be the most reasonable approach given current knowledge.

Paradoxically, the development of simple guidelines to estimate the value of research information as a proportion of expected value of perfect information is actually easier for those more complicated problems where there are an infinite number of possible states, and where the decision maker is faced by an almost continuous range of possible courses of action. An example is the production of information about market prices. Fortunately, there is a small but instructive body of literature on the evaluation of market outlook research, including the pioneering article by Hayami and Peterson (1972), and a more recent set of articles by Freebairn (1976a, 1976b, 1978). To me, the intriguing aspect of Freebairn's work is that, even though he developed a modified version of the net social benefits approach to valuing outlook information pioneered by Hayami and Peterson (1972), his results are functionally equivalent to those obtainable from Bayesian decision theory.

In Bayesian decision theory terms, price prediction is an example of the class of infinite-action problems known as point estimation. For this class of problem, Winkler (1972) shows that determination of expected value of perfect information as well as of the actual value of information is especially simple as long as the loss function is quadratic. Furthermore, the optimal point estimate is the mean of the belief distribution, and expected value of perfect information is directly proportional to degree of uncertainty as measured by the variance of normally distributed beliefs. Likewise, the proportionate reduction in expected value of perfect information due to extra information simply equals the ratio of the number of observations embodied in this extra information to the total number of observations embodied in posterior beliefs.

Freebairn (1976a, 1976b) also found that the value of price predictions derived from outlook research is proportional to the reduction in variance of forecasting error. This functional equivalence to the Bayesian results is explained by Freebairn's demonstration that the net loss of social welfare from price forecasting error is a quadratic function of the size of the error as long as farmers' forecast prices are unbiased estimates of realised price. Freebairn posited a rational-expectations justification for this assumption though, as noted above, such an assumption also is consistent with the decision-theoretic approach as long as their prior beliefs are unbiased. Either way, the result suggests a simple procedure for estimating the value of information from agricultural economics research for those cases where the prior degree of uncertainty can be expressed as a variance, and/or where the effect of research information can be estimated in the form of a proportionate reduction in prior variance.

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7. That is, the probability of predicting a state of nature conditional on it in fact being the true state.

Some idea of the likely magnitude of expected value of perfect information for some types of decision, and/or of the possible proportionate reductions in expected value of perfect information for some types of information, can be gauged from the results of empirical studies in which the value of specific types of information has been estimated. A selection of these estimates is summarised in Table 1.

It would be dangerous to attempt to draw too many general conclusions from such fragmentary evidence. One noteworthy aspect is the fact that, with few exceptions, the proportionate reduction in expected value of perfect information is greater than 10 per cent, and

Table 1: Selected Empirical Estimates of Value of Information per Annum to Farmers

Author	Risk attitude	$EVPI*	Unit	Type of Information	$EVSI**	% Reduction	Product	Decision Type
Eidman et al(1967)	Neut		Head	Price Outlook	0.06	?	Turkeys	Selling Strategy
Bullock & Logan(1970)	Neut		Head	Price Outlook	1.81	?	Cattle	Selling Strategy
Freebairn (1976b)	Neut		Aust	Price Outlook	1.10m		Wool	Production Level
	Neut		Aust		0.09m		Wheat	
	Neut		Aust		0.004m		Barley	
	Neut		Aust		0.16m		Potatoes	
Norton & Schuh(1981)	Neut	0.71	Bu	Price Outlook	0.51	72	Soybeans	Selling Strategy
	Neut	0.71	Bu		0.21	30		
Ryan & Perrin(1974)	Neut	200†	Ha	Response Function		?	Potatoes	Fertilizer Applicn
Doll (1981)	Neut	2.50	Acre	Weather Forecast	1.30	52	Corn	Growing Strategy
	Neut	2.50	Acre		0.05	2		
Byerlee & Anderson (1982)	Pref	480	Farm	Weather Forecast	52	11	Wool	Drought Strategy
	Neut	520	Farm		59	11		
	AVERS	550	Farm		58	11		
Bosch & Eidman (1985)	Pref	4.20	Ha	Soil Moisture	1.00	24	Corn & Soybeans	Irrigation Scheduling
	Pref	4.20	Ha		1.40	33		
	Neut	6.98	Ha		3.68	53		
	Neut	6.98	Ha		5.04	75		
	Avers	16.30	Ha		14.40	88		
	Avers	16.30	Ha		15.00	92		
	Pref	4.20	Ha	Weather Forecast	1.00	24	Corn & Soybeans	Irrigation Scheduling
	Neut	6.98	Ha		1.08	15		
	Avers	16.30	Ha		0.70	4		

* Expected value of perfect information.
** Expected value of 'sample' information.
† Not based on Bayesian decision theory.

often greater than 50 per cent. As for guidelines to the value of information relative to gross value of production, Freebairn (1976b) found that even with a 50 per cent reduction in forecast errors, net benefits to society would increase by less than one per cent of gross value of output. On the one hand, this estimate is biased upwards by the implicit assumption of complete adoption. On the other, it is biased downwards because it ignores benefits to other economic agents besides farmers. On balance, I suspect that it is an upper bound estimate of realised benefits.

In order to extrapolate to other potential demands for information from such empirical estimates of expected value of perfect information as do exist, some knowledge of the nature of the relationship between information value and its determinants is needed. In a comprehensive review of findings to that time, Hilton (1981) concluded that in general there is no monotonic relationship between the value of information and any of the determinants of expected value of perfect information. While the nihilism of this conclusion rivals that of Arrow's Impossibility Theorem, Hilton provides partial redress by cataloguing the findings from a number of studies of particular cases which provide some evidence of fairly consistent relationships between expected value of perfect information and its determinants. His summary has been supplemented by the inclusion in Table 2 of some additional results of which I am aware. All of us could benefit from a systematic effort to augment the information contained in this table.

Research Evaluation Guidelines

In principle, agricultural economics research is no different from other forms of research, so the general framework used to assess priorities for agricultural research can also be applied to agricultural economics research as long as the information output can be valued.

There has been a trend in the literature over recent years to develop increasingly complicated formulae to calculate the expected net present value of alternative avenues for research. Time will tell whether all of these recent refinements improve our ability to select high payoff areas of research. Mindful of Kamarck's (1983) stricture to strive more for accuracy, and less for precision, I intend to use a much simpler formula in order to concentrate on those determinants of net research returns which I believe to be more important.

Like any other form of research, the criterion for investment in social science research should be that the expected net present value E(NPV) > 0, where

$$(1) \qquad E(NPV) = \sum_{t=1}^{\infty} [-C_t + p_t d_t G_t](1 + i)^{-t}$$

where C_t = Cost of Research in year t

G_t = potential gross annual research benefits in year t

p_t = probability that research output is available by year t

d_t = proportionate level of adoption of research output by year t.

Table 2: Summary of Studies of Relation Between Value of Information and its Determinants in Special Cases

	Information Value is:			
	increasing	non-decreasing	decreasing	indeterminate
Decision Flexibility	Merkhofer (1977)*	Hilton(1979)*		
Decision Maker's Risk Aversion		Bosch & Eidman(1985)	Ohlson (1965)*	Campbell & Lindner(1985)
Decision Maker's Wealth	Kihlstrom (1974)* Ohlson(1975)*			
Decision Maker's Prior Risk	Hilton(1979)* Doll(1971)	Marschak & Radner(1972)* Itami(1977)*		
Information Accuracy	Wilson(1975)* Ijiri & Itami (1973)* Hilton(1979)* Doll(1971) Byerlee & Anderson(1982) Freebairn(1978)			
Information Amount	Various†	Kihlstrom (1974)*		

* From Hilton (1981).
† For instance, see Winkler (1972), Degroot (1970).

This expression simplifies to:

(2) $$E(NPV) = C[p.G.C^{-1}((1+i)^{n}-1)(1+i)^{-(h+m+n)}+(1+i)^{-h}-1]/i.$$

if C_t = C for years 1 to h, and zero thereafter

d_t = 1 for years (h+m) to (h+m+n)

0 for other years

p_t = p for years h to (h+m+n)

0 for other years

G_t = G = (RVI) for years h to (h+m+n)

where V = gross value of production affected by research
I = expected value of perfect information per unit value of production
R = proportionate reduction in expected value of perfect information due to research information.

Expressing the formula in this way permits exploration of the sensitivity of E(NPV) to variation in the following variables:[8] (a) annual research costs, C; (b) the discount rate, i; (c) implementation delays, m; (d) realisation period, n; and (e) the ratio of expected gross annual research benefits to costs pRVI/C.

Note that E(NPV) is perfectly linear in C. This is especially convenient as it permits average annual research costs to be treated as the numeraire, thereby reducing the number of variables requiring explicit discussion. In contrast, E(NPV) is non-linear in terms of research duration, h.

Of the other parameters in the equation, the implementation delay encompasses both the discovery lag while potential users of the research results become aware of their existence, and the average time lag to adoption by potential users. This implementation time lag, and the length of time over which potential research benefits are likely to be realised, are both hypothesised to differ systematically between different research areas.

Finally, there is the composite variable, pRVI/C, which I will refer to as the target return ratio. Note that this ratio is akin to an elasticity in the sense that it is dimensionless. It subsumes many of the crucial and difficult to measure determinants of net research benefits, such as the probability that the project will yield useful results, the likely ceiling level of adoption by potential users (that is, the so-called 'scale' effect), the potential reduction in the costs of uncertainty and of risk per unit of scale, and the proportionate reduction in these costs actually realised.[9] Given estimates of the number of years needed to complete the research, the number of years' delay before implementation, and the number of years before the results become obsolete for each case, it is a simple matter to calculate the target return ratio for expected net present value to be positive. The computed ratios for selected parameter values used in the remainder of this talk are presented in Table 3. In summary, the most striking feature of Table 3 is the low values (for example, less than 2) of target return ratios required for E(NPV)>0 for modest values of the more easily predicted parameters of duration of the research project, implementation delays, and likely duration of benefits.

Subjective, even qualitative estimates of the probability of success, the scale effect, expected value of perfect information per unit of scale, proportionate reduction in expected value of perfect

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8. The partial derivatives of E(NPV) are attached as Appendix A. Great caution needs to be exercised in interpreting such sensitivity measures, because the <u>ceteris paribus</u> assumption almost certainly breaks down. For instance, any attempt to maximise E(NPV) by substitution between annual research costs, C, and research duration, h, is likely to also induce changes in one or more of p, G, m, and n.

9. It is not uncommon to avoid the problems inherent in estimating some of these parameters in studies evaluating research priorities for agricultural science by assuming standard values (for instance, see Edwards and Freebairn 1981). Despite the difficulty of doing so, I believe that assessments of this type will be of little value until possible variation in all of these determinants can be predicted with some degree of confidence.

information, and annual research costs can then be combined to assess whether the target return ratio is likely to be exceeded or not.

Research Evaluation--Three Hypotheticals

To illustrate how the suggested framework can be used to guide the evaluation of agricultural economics research, I will use three examples. The first concerns choice of technique decisions by farmers, the second utilises the literature on the ex ante evaluation of outlook research, while the third is a more speculative attempt to

Table 3: Target Return Ratio of Research Required for E(NPV)* = 0

h	n \ m =	1	2	4	8	16	32
1	1	1.1	1.2	1.3	1.6	2.3	5.0
2	2	1.2	1.2	1.3	1.6	2.4	5.3
4	4	1.3	1.3	1.4	1.8	2.7	5.8
8	8	1.6	1.6	1.8	2.2	3.2	7.0
16	16	2.3	2.4	2.7	3.2	4.8	10.4
1	4	0.3	0.3	0.3	0.4	0.6	1.3
2	8	0.3	0.3	0.4	0.5	0.7	1.5
4	16	0.4	0.4	0.5	0.6	0.9	1.9
8	21	0.6	0.7	0.7	0.9	1.3	2.9
16	64	1.3	1.4	1.5	1.8	2.7	5.9
1	16	0.1	0.1	0.1	0.1	0.2	0.4
2	32	0.1	0.1	0.2	0.2	0.3	0.6
4	64	0.2	0.2	0.3	0.3	0.5	1.1
8	128	0.5	0.5	0.6	0.7	1.0	2.3
16	256	1.2	1.3	1.4	1.7	2.6	5.6

m = Implementation delay (in years)
h = Duration of research expenditure (in years)
n = Duration of research benefits (in years)
* = Calculated using real discount rate = 0.05.

apply the same principles to the evaluation of agricultural policy research.

Consider first linear programming studies designed to identify a more profitable allocation of fixed farm resources utilising established technology. My gut feeling is that such studies can be completed quite quickly, that implementation delays will be relatively short, but that benefits also will be short-lived because of the volatility of some parameter values, and because some knowledge encoded in the model soon becomes obsolete. It can be seen from Table 3 that if all three lags equal two years, then the target return ratio for research will be slightly more than one. Other calculations show it could be as low as 0.5 for research lasting only one year but yielding benefits for at least four.

Furthermore, given that the requirement for creative effort is modest, the likely chances of success are quite high (almost certainly greater than 0.5 provided that the research staff are competent). Now if annual research costs are about $50,000 per annum, a conservative estimate of the potential annual research benefits necessary might not exceed $50,000 per annum, but could easily rise to $0.5 million. Unfortunately, the information generated by this type of research is quite location specific, and even firm specific, so the scale effect will not be large. Thus the value of research information per farm would need to be large, perhaps as large as $5,000 per farm. It is unlikely that the cost of uncertainty plus the cost of risk would be this large even if existing farm plans are based solely on tradition. Any lack of credibility concerning the validity of the results would exacerbate the difficulty of providing a positive net return.

A somewhat different picture emerges if the LP models are developed to evaluate new farm practices. The target return ratio required is likely to be somewhat smaller than that in the previous case because the time period over which benefits are realised should be longer. For most innovations, the scale effect also is likely to be appreciably larger than for the previous case. Furthermore, considerable uncertainty exists about potential performance of any new innovation, so expected value of perfect information per farm also is likely to be substantial. One moot point is whether advice by 'academic' economists will have much credibility with potential adopters. If it does not, then the research results will not reduce expected value of perfect information very much, if at all. On the other hand, if farmers regard the results of such research as reliable, then actual information value should justify the research. Bio- and information technology are now 'flavours of the month', and some agriculturalists are predicting that they will generate a further revolution in farming practice. If these predictions are fulfilled, then relatively pedestrian research investigating the profitability of the seeds of this revolution should be accorded more status by the profession than it currently enjoys.

My second example concerns outlook research. As this area of research has already been the subject of more investigations than most other areas, I intend to be brief and focus mainly on the probability of success. In the context of outlook research, success needs to be defined as achieving a reasonable reduction in forecasting error. The a priori likelihood of achieving such an aim varies widely depending on what uncertain state the research is attempting to predict.

Take wheat planting intention surveys and subsequent crop forecasts as a case in point. The probability that such research will succeed must be close to 100 per cent. In addition, research costs are comparatively modest, the period of research duration and the implementation delay are both very short (that is, less than one year), and the number of potential users of such information is extremely large. Depending on the marketing arrangements for the commodity in question, farmers may or may not use crop forecasts to 'fine tune' production and/or harvesting decisions, and/or selling strategies. Irrespective of farmers' demand for this information, it is likely to be used by input suppliers to estimate inventory needs, by transport firms to predict traffic loads, by processing and storage firms to plan labour requirements, by merchandising firms to re-assess price expectations, and by financial firms to calculate financing needs.

Thus even if benefits to individual decision makers are very small and 'adoption' levels correspondingly low, net research returns are likely to be positive notwithstanding the ephemeral value of the information produced. In contrast to wheat, the value of equivalent surveys for minor crops may be too low because the number of potential data users will be correspondingly smaller. For some crops, this may be compensated for by a less rigid marketing system which increases decision makers' flexibility of actions and with it the value of information per decision.

In contrast to the above case, some parameters are notoriously difficult to forecast. Tomorrow's exchange rate, and future 'futures prices' are cases in point. For such parameters, the probability of successful forecasts may be so close to zero as to ensure that E(NPV)<0 almost irrespective of the size of the potential value of more accurate forecasts.

Finally, I want to indulge in some brief speculation on policy research. In my opinion, this is an area where implementation delays are likely to be substantial. On the other hand, an extended duration of research benefits is problematical given the penchant of newly elected governments to do something, to do anything to convince voters (or maybe themselves?) that they are in charge. From Table 3, note that if the implementation lag is 32 years and if research benefits only last four times as long as it takes to complete the research, then the target return ratio can be as low as 1.0 for a short-lived project, and as high as 6.0 for an extended project.

Furthermore, this is an area of research where casual empiricism suggests that the probability of successful adoption of the research results is very low.[10] Consequently, the ratio of potential annual benefits to annual research costs could need to be greater than 50, or perhaps even 500, for investment in this area of research to be justifiable. Speculation about whether or not particular types of policy research can satisfy such a requirement is a question I prefer to leave to others to debate.

I expect that by now, most of you want to take issue with at least some of the assumptions and/or conclusions of these hypotheticals. At least I hope so. My response is to challenge you to make your own assumptions, to do your own sums, and to reach your own conclusions.

10. No doubt practitioners of the art will object that, without their efforts, agricultural policy would be in an even bigger mess. If so, the avoided incremental cost of even larger butter mountains, lakes of olive oil, grain pools, etc, form part of the benefits of such research.

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APPENDIX A

PARTIAL DERIVATIVES OF EXPECTED NET PRESENT VALUE OF RESEARCH

$$E(NPV) = i^{-1}pG[(1+i)^{n} - 1](1+i)^{-(h+m+n)} - i^{-1}C[1 - (1+i)^{-h}]$$

$$[\partial E(NPV)]/\partial C = i^{-1}[(1+i)^{-h} - 1]$$

$$[\partial E(NPV)]/\partial p = i^{-1}G[(1+i)^{n} - 1](1+i)^{-(h+m+n)}$$

$$[\partial E(NPV)]/\partial G = i^{-1}p[(1+i)^{n} - 1](1+i)^{-(h+m+n)}$$

$$[\partial E(NPV)]/\partial h = -hi^{-1}(1+i)^{-h-1}\{pG[(1+i)^{n} - 1](1+i)^{-(m+n)} + C\}$$

$$[\partial E(NPV)]/\partial m = -mi^{-1}pG[(1+i)^{n} - 1](1+i)^{-(h+m+n+1)}$$

$$[\partial E(NPV)]/\partial n = ni^{-1}pG[(1+i)^{-(h+m+n+1)}$$