TECHNIQUES FOR GUIDING THE ALLOCATION OF RESOURCES AMONG RURAL RESEARCH PROJECTS: STATE OF THE ART

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Diverse methods are available for evaluating benefits and costs of rural research projects. They have been developed in response to a felt need for information in a highly uncertain environment. These formal evaluation schemes are compared in an attempt to show whether any offer useful guidelines for rational allocation of research funds. The outcome is a series of conjectures on the level of effort to devote to research evaluation, and hence on the techniques which should be used. In most circumstances encountered in Australian rural research the optimal type of evaluation would be relatively unsophisticated.

INTRODUCTION

The purpose in this paper is to assess the state of the art in the evaluation of agricultural research projects. This involves the examination of the various techniques of evaluation that are currently available, with a view to ascertaining their operability in guiding the allocation of resources among projects. To an extent, the study updates and extends that of Anderson[1], but it must be acknowledged at the outset that many of the comments made then about the problems inherent in research evaluation still seem to be valid. The broad objective is to examine what it is about research that makes its *ex ante* evaluation difficult, and whether the available techniques overcome the problems to provide useful answers to questions about the allocation of resources to agricultural research.

The research and innovation process is first outlined. This facilitates an examination of those of its characteristics that cause difficulties in evaluation, and also provides a basis for comparing the various techniques for research evaluation. These are considered in subsequent sections. For each method, there is a critique of its rationale and a commentary on the most significant studies in the area. The final part of the paper is a discussion about which evaluation techniques seem to be the most appropriate and how much effort in research evaluation might prove most fruitful.

THE RESEARCH AND INNOVATION PROCESS: CONSEQUENCES FOR EVALUATION

Conceptually, evaluation of research projects can be considered as a special case of investment appraisal, in which future flows of costs and benefits are both diverse and extremely uncertain. The need for research evaluation is akin to the need to appraise investment prospects in order to allocate funds efficiently and to achieve an optimal portfolio. Hence, the techniques of research evaluation are not unlike the techniques of investment appraisal, and involve evaluation of a process that transforms current costs into a flow of uncertain future benefits.

The differences between research and investment are at the practical level. Though the evaluation of both requires subjective assessment of future possibilities, which is performed by knowledgeable experts, research benefits are generally far more difficult to measure, are more diverse, and usually are of a public good nature in that the original investors may not be the eventual beneficiaries and additional beneficiaries do not necessarily detract from the benefit of others. The further difficulties in measuring research benefits are caused by the profound uncertainties combined with a typically multi-period process, to produce a diverse and complex system. A multi-stage appraisal inevitably is required to evaluate the effects of research through development and diffusion to final social impact.

Considering the research part of this process alone, it is possible to draw a decision tree representation like Figure 1. This might be considered a reasonable approach to investment appraisal. In research evaluation, uncertainty (except for serendipity) can be handled if all the events within the decision tree and their probabilities of occurrence can be elicited.[2] In theory, this is straightforward but, in practical applications, it would be stretching the capabilities of even the most creative and enthusiastic research reviewer to request information of this detail, even at the second stage of most research programs. The problem is that time and uncertainty combine to yield a dimensional problem which presents itself in terms of diversity of outputs from many research programs. In this example, covering only three time periods, there are i + xj + xyk possible innovations to consider, and xyz research paths. Even for applied research projects that would typically be characterised by small values of each of these variables, such a direct and comprehensive evaluation would probably be prohibitive.

In addition to these problems, there is the necessity of a multi-stage evaluation process because, once an innovation is suggested by a research program, it needs development and diffusion before it can result in market-level impacts. The value of each innovation can only be ascertained once its impacts along these dimensions are estimated. The assessment of these impacts adds further rounds of difficulties to an already complex process.

Hence, the research function produces potential innovations, each having a probability distribution of physical effects on agricultural production. In turn, each point of such distributions gives rise to a diffusion pattern for the innovation, and hence an impact through time on total production. Finally, there are the effects in agricultural product markets and eventually a net societal outcome. It is this multi-dimensionality combined with uncertainty at each stage of the process that makes research evaluation challenging — some would say impossible. Certainly, direct decision-tree evaluation is not possible and simplification, often some gross simplification, is necessary to achieve a tractable analysis.

EVALUATION TECHNIQUES

(a) Introduction

The techniques for evaluation of research projects can be categorised into the following order of increasing data requirements: (i) rules of thumb, (ii) scoring models, (iii) production function, systems and mathematical programming models, and (iv) benefit-cost approaches. To an extent, the type of research project under consideration determines the type of appraisal that is possible. With basic research, the amount and reliability of data on the project are lower than with applied research. This prevents the use of data-using techniques, such as mathematical programming, in appraising such basic research projects. For applied research projects, on the other hand, the data position often permits a wider choice of appraisal techniques.

A question related to all this is that of determining the optimal level of research project appraisal itself. The usual marginal rules would require an inner appraisal to be observed that intends that

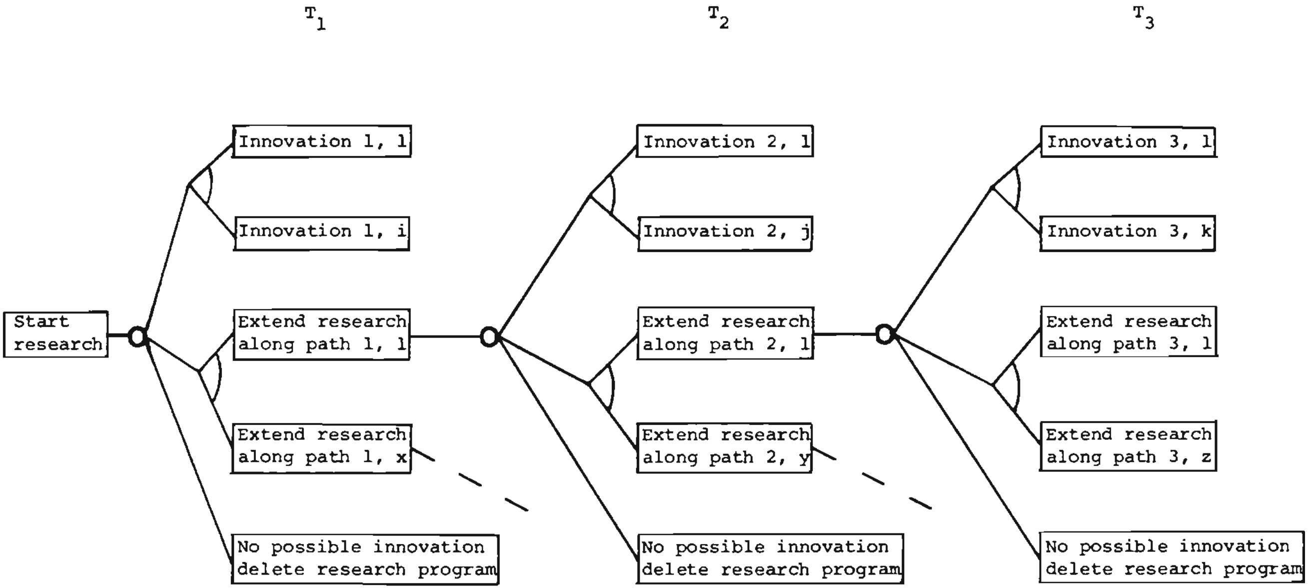

Figure 1 Decision Tree Representation of a Research Problem

the last dollar spent on both the research itself and the research project appraisal brings forth the same expected marginal return. The problem of determining this position in a risky environment is particularly difficult and has received scant attention.[3]

The general objectives of all the appraisal methods are to show which allocation of scarce research resources to alternative research projects will yield the greatest value. Previous reviews of research appraisal that form a basis for the current study are those by Anderson, Baker, Arnon, Baker and Freeland, Shumway, Schuh and Tollini, Greig, Norton and Davis, and Ruttan.[4] In addition, three major publications in agricultural research resource allocation that provide a broader outlook on the field are those by Fishel, Arndt, Dalrymple and Ruttan, and Ruttan.[5] Our review is restricted to *ex ante* evaluation techniques involving multiple attributes or criteria.

(b) Rules of thumb

Intuitive methods based on rules of thumb are probably the set of techniques that have had by far the most use in the evaluation of research and allocation of funds to research. Their great advantage is their simplicity and low use of formal data, and hence low cost. Their most significant disadvantage seems to be a low level of scrutiny of projects, which leads to a certain degree of inflexibility over time in research resource allocation. Arnon and Ruttan review several rule-of-thumb techniques that have been applied.[6] The two most significant are perhaps precedence and congruence.

With the precedence model, the previous year's funding is regarded as a base for each project, and a proportion is augmented or removed. This has the advantage of permitting continuity in the funding of particular lines of research activity but, over time, has the disadvantage that research that has reached the limit of its productivity continues to be funded. As Arnon shows, it is easy to see how the social interaction within research organisations supports a precedence system.[7] Past emphasis tends to be perpetuated because research skills and experience have been developed in those areas and because the personal biases of research administrators and research workers tend to be in those areas. Tenure of specialists may also be a contributing factor.

Thus, changes in overall allocation to research are shared about equally by each project and a change in relative share occurs only as a result of an upheaval in the system. It must be questioned whether research resource allocation based on the precedence model is sufficiently sensitive to promote a set of research activities that adequately reflects the pressing problems of agriculture.

The congruence model is another rule-of-thumb technique that, however, induces a little more flexibility into the allocation system. The general principle of the technique is to allocate funds across research areas in proportion to the value of the agricultural product of such areas. For example, if the output of beef has a value twice that of wool, then beef research receives twice as much finance as wool research. The model lends itself easily to a system of industry levies to finance research, like the system used, for example, by the Australian Wool Corporation, and the notion of matching funds provided by governments.

The main drawback of the congruence model is that new lines of activity (e.g. oilseeds) that are small in relation to the whole, but potentially highly productive, tend to receive low levels of research funding. In addition, the model may not be as straightforward as it initially appears because, as Ruttan points out, the process of allocating research resources involves a four-way allocation of resources: (i) among commodities, (ii) among resource categories, (iii) among stages of the production system, and (iv) among disciplines.[8] A congruence model could be applied to each of these dimensions singly or in some combination.

(c) Scoring Models

In the scoring-model approach, a set of attributes, $a_1, \ldots, a_K$, is defined, over which each of J projects is to be assessed. Reviewers are asked to give each project a discrete value ($=1, \ldots, 7$, say) on each attribute. By averaging across reviewers, scores, S_{jk}, for each project on each attribute are determined. Then, given a weighting scheme for the attributes, $w_1, \ldots, w_K$, determined by research administrators, a global score for each project is $S_j = \sum_k w_k S_{jk}$. Alternatively, if a multiplicative system is preferred, the global score is $S_j = \prod_k w_k S_{jk}$. The scores obtained are used to rank projects.

Selection of projects then proceeds with the objective of maximising:

$$(1) \qquad \sum_{j=1}^{J} s_j x_j,$$

$$(2) \qquad \text{subject to} \quad \sum_{j=1}^{J} c_j x_j \leq B,$$

where B is the available budget, $s_j = 1$ if the project is selected, otherwise zero and c_j is the cost of funding project j.

It is generally acknowledged[9] that the three most significant practical agricultural research appraisal exercises using scoring models were performed by the USDA,[10] Iowa State University,[11] and North Carolina Agricultural Experimental Station.[12] All used a fairly conventional scoring-model approach as a basis for later panel discussion (and presumably decision) on the financing of broad research areas. The Iowa study used ten criteria and a uniform weighting scheme, USDA eight criteria and variable weighting, and the North Carolina study a variable number of criteria, depending on the particular project being appraised, together with a variable weighting scheme. A novel aspect reported by Shumway and McCracken was to adjust the weighted average ranking of the projects according to the variability of rankings of the individual reviewers.[13]

Although all three of these exercises were extensive, their success seems to have been tentative at best. Shumway and McCracken report that, although considerable resources were devoted to the appraisal, the usefulness of this type of ranking exercise remained unproven.[14] Williamson regarded the scoring model used by USDA as a 'window dressing' device, and considered that the planning process would have produced a similar ranking of alternatives without the model.[15] He further considered that a long period of experimentation with the methods would be required before such techniques would become acceptable. Finally, Mahlstede emphasised the amount of time and effort that was required by review panels, and recommended that future appraisal should guard against unwittingly imposing constraints on the creative process.[16]

The usefulness of scoring models is restricted to situations where there are a small number of independent attributes (say, less than 10) to be assessed.[17] Hence, the research assessment problem must be capable of being construed meaningfully in terms of a small number of attributes. Second, the discrete scale over which each attribute is scored must have a distance between units that is capable of distinguishing between different alternatives, yet have the minimum number of intervals to distinguish between alternatives which differ significantly. These characteristics of scoring models make them particularly useful in appraising basic research, and hence in comparing basic research projects with more applied projects.

There is some debate in the literature over the issue of whether additive or multiplicative procedures should be used in scoring models. Mottley and Newton in an early scoring model used a multiplicative approach with equally-weighted attributes.[18] The grounds for using a multiplicative model were arbitrary. In contrast, additive models have been favoured almost universally since then. Moore and Baker conducted an intensive study of scoring models and concluded that additive models were preferable to multiplicative models on grounds of consistency of results.[19] Cardus, Fuhrer, Martin and Thrall argued that the multiplicative model is conceptually correct as long as the scores that are multiplied represent attributes that follow each other in sequence.[20] Their model included indexes for research success, adoption and benefit to the funding body; the product of which is the project's score. They proceeded to compare the performance of multiplicative and additive models, but obtained the same project ranking using each.

As a final point on the multiplicative/additive issue, Silverman argued that no aggregation across the attributes to obtain a project score should take place because this "obscures the project's impact on each of the criteria".[21] Instead, research administrators are presented with the rating of the project on each attribute, and presumably they provide an intuitive weighting scheme. Ruttan tends to concur, viewing aggregation across attributes as the most serious problem of scoring models.[22] Various methods have been devised to overcome the need to aggregate across attributes. However, they merely amount to another method of aggregation. The 'ordinal intersection method' devised by Cook and Seiford is representative of such efforts to avoid the aggregation problem.[23]

A basic element of scoring models is the avoidance of explicit construction of a research production function. This is achieved by using research reviewers to estimate broad parameters of projects (scores), presumably based on their own individual intuitive notions of the research production function. Cartwright has criticised this lack of structure, claiming that such models place little discipline on reviewers to understand the structure of the problem.[24] Despite this, however, Moore and Baker, and Baker and Freeland report substantial evidence that scoring models are rank-order consistent with more complex mathematical programming and benefit-cost models.[25]

(d) Mathematical programming I

The first category of programming model is somewhat similar to scoring models. The study by Russell is fairly typical of the class.[26] The basic structure of his model is to maximise:

(3) $U = \sum_k w_k G_k,$

(4) subject to $G_k = \sum_j (G_{jk}/C_j) C_j$ for $k = 1, \ldots, K,$

(5) $C \geq \sum_j C_j,$

(6) $\hat{C}_j \geq C_j,$

where U is total utility of the research program, G_k terms are the different criteria, w_k values are the units of utility derived from one unit of criterion G_k, C_j is the level of funding of project j, G_{jk} values are expected units of goal dimensions G_k supplied by the project if funded at level C_j, $\hat{C}_j$ is the level of funding specified in the original proposal from researchers, and C is the total available research expenditure. Thus, the w_k values are similar to weights in the scoring model. An additional feature of this type of approach over scoring models is that the level of funding of each project can easily be considered variable. Russell achieved this through equation (4), which is a linear relationship between the level of funding and the benefit derived.[27]

Russell's model is directed towards general use in appraising individual research projects (e.g. hybrid swedes) in the U.K. Other similar models are addressed to allocating funds optimally within a research organisation,[28] or form the basis of a large-scale decision model for allocation of funds in hierarchical organisations.[29] This form of mathematical programming appraisal of research projects has drawbacks similar to those noted for scoring models, and they need not be elucidated further here. Mathematical programming does have some advantages, termed by Russell 'post-optimal information'.[30] This includes sensitivity analysis applied to the weighting system, and extension to non-linear weighting schemes.

(e) Production function/systems/mathematical programming II approaches

These three types of appraisal technique are treated together because conceptually they are similar. Typically, a three-stage process is involved. A model is constructed to represent the agricultural production sector, agricultural productivity is estimated as a function of research inputs, and the influence of varying these inputs on agricultural productivity and hence on production, farm incomes, etc. is observed. The differences in the three types of appraisal result from the different emphases of these three elements.

The production function models concentrate on the relationship between agricultural productivity and research, and generally include only a rudimentary representation of an aggregate farm production sector.[31] The approach is, first to estimate an econometric relationship based on time-series data:

(7) $Y = f\,(C,N,Z)$,

where Y is an index of agricultural productivity, C and N are research and extension expenditures, respectively, and Z are other (often stochastic) exogenous influences. Second, the effect on Y of varying research expenditure is computed, and third this productivity change is translated into a supply curve shift, and market level consequences are assessed.

Pinstrup-Andersen and Franklin describe several research appraisal models developed at the International Centre for Tropical Agriculture at Cali in Colombia, one of which is a systems model.[32] A detailed model of the agricultural production system of small farms is developed to represent the principal structural relationships in the physical, biological and economic environment. New technologies are introduced systematically into the model, and five-year simulations show the effects of uptake of the technologies on aggregates such as maize output, and net income. By this means, the benefits from investment in broad research areas (e.g. herbicides) can be revealed.

The second mathematical programming approach is similar to the systems approach, emphasising a detailed model of the agricultural production process, this time using linear or quadratic programming.[33] Again the model is used to show the production and market effects of the introduction of new technology, and hence indicates the relative potential benefits from investment in broad research areas (e.g. research to enhance labour productivity). Both the mathematical programming and systems approaches depend on subjective estimation of the productivity effects of research projects. The production function approach is, on the surface, based on objective data, but must rely on a subjective estimate of the future continuation of estimated relationships between research inputs and agricultural productivity.

The production function/systems/mathematical programming approaches operate at a more highly aggregated level than does the scoring model. They are not generally used to analyse a specific research project (though some exceptions are noted below), but are used in a conditionally normative manner to show what should occur given the pursuit of particular forms of research. Also, they depend on rather detailed data and are only applicable to appraising applied research. As explained by Shumway, these models suffer from the problem that it is not possible (with much confidence) to correlate historical research performance with future research payoff at a very disaggregated level.[34] Nevertheless, several project-specific appraisals have been performed.[35] Finally, one advantage of these models is that they do yield measures of the marginal productivity of research investments.

(f) The benefit-cost approach

Greig provides a useful review of major benefit-cost analyses of research.[36] He divides benefit-cost models into four types, viz. benefit-cost, benefit cost with probability of success measures, discrete probabilistic benefit-cost, and probabilistic benefit-cost.

For the purposes of this paper, only the probabilistic benefit-cost model will be considered, as comments about it are generally applicable to the others. It is the most realistic in its treatment of probabilistic features.

The benefit-cost approach to the appraisal of research projects generally involves the use of panels of scientific reviewers who estimate, for each project under review, the time pattern of future benefits and costs, and probabilities of success in the research, development and adoption stages. The benefits and costs are ideally estimated as probability distributions rather than single-value estimates in order to reflect the inherent uncertainty in subjective estimation. In addition, market-level consequences following adoption can be estimated as the consumer and producer surplus changes resulting from supply and/or demand curve shifts.

Hence, the application of the basic benefit-cost model to a particular research project can be regarded as a four-stage process (though each application has its own variations): (i) either specify annual research expenditures (C_t) to the reviewers or ask them to estimate reasonable annual research expenditures, (ii) obtain from reviewers a probability distribution of times to complete the project ($P(T)$) given C_t, (iii) for each C_t and expected value[37] of T ($E[T]$) reviewers estimate a probability distribution of benefits $P(G_t)$, and (iv) discount and simulate from the resulting cost and benefit distributions to obtain cost-benefit ratios or net present values of projects.

Perhaps the most comprehensive model of this type is the MARRAIS model (Minnesota agricultural research resource allocation and information system) developed by Fishel.[38] Ruttan regards the MARRAIS model as "one of the most logically thought out and procedurally sophisticated research-planning models available".[39] However, he also notes that its "high cost to users has been an obstacle to its routine application". In Fishel's experimental application of the approach, nine projects were sent out for review by 170 scientists. Only 69 of these reviewers completed the task, which involved three surveys.

Additional interesting features of the MARRAIS model were the method for estimating benefits generally and the method for estimating benefits of basic research projects. The overall approach to estimating benefits was to ask reviewers to give a probability distribution of the value G_{max}, the value of the research project if it is 100 per cent successful. Subjective estimates of F, the technological feasibility, were also obtained, so that the final distribution of benefits is $P(G) = P(F)P(G_{max})$, assuming that F and G are independent.

Second, when estimating the benefits of basic research projects, a ranking procedure is used, and no direct benefit estimation is involved. All the projects under consideration are ranked. Then reviewers are asked to express the relative worth of adjacent projects in percentage terms. Given that a number of these projects are applied in nature, dollar values of benefits are already estimated for them, and hence dollar values of the benefits of the basic research projects can be imputed.

A significant study using the benefit-cost with probability of success measure is that of Araji, Sim and Gardiner.[40] Greig argues that caution should be applied when using this alternative because it only provides an expected value of benefits and not a probability distribution.[41] Resolution of this issue (at least for public research), however, depends on acceptance or otherwise of the controversial (but generally accepted) arguments of Arrow and Lind as to the relevant criterion for public investment appraisal.[42] It does seem that, under most circumstances imaginable for public research work, society (through its wide sharing of the net benefits of research) should properly be neutral in its attitude towards risky projects and, accordingly, should (through its publicly sponsored administrators) seek only to maximise expected (i.e. average or mean) social return. Risky research projects should not be discriminated against because of the risk *per se*.

Allied to the benefit-cost evaluation of agricultural research is the notion that social benefits of research can be estimated through consumer and producer surplus changes resulting from shifts in supply and/or demand curves. This approach was developed for use in research evaluation by Griliches, Peterson, and Duncan, and more recently by Lindner and Jarrett, Rose, and Wise and Fell.[43] Applications of the approach include de Castro and Schuh to an assessment of the distribution of benefits from research into six crops in Brazil, Edwards and Freebairn to the total agriculturally oriented research of Australia, and Johnston to individual research projects within a national research organisation.[44] Davis examines the relationships between the economic surplus and production function approaches in *ex post* evaluation of the returns to agricultural research.[45] He shows that most production function models imply a pivotal-divergent supply shift, which is not common in economic surplus models.

Technology assessment can be considered as an extension of this net social benefit model.[46] Its objective is to determine *ex ante* the broad social, economic and environmental consequences of investments in various lines of research. In their assessment of agricultural economists' contributions to agricultural research, Phillips and Dalrymple suggested that there were benefits from such a movement in research evaluation.[47]

Although Fishel applied the benefit-cost model to both basic and applied research, its data-using characteristics generally restrict its usefulness to evaluating more applied types of research project.[48] It has been applied mostly at the individual project level, and there would be a difficulty of finding suitably qualified experts to provide the necessary subjective estimates for analyses at higher levels of aggregation. The benefit-cost approach is the closest to the real-world system which it models. Hence it is the most extensive, most systematic but, at the same time, the most costly of the research evaluation techniques.

Views range considerably on the applicability of the benefit-cost method, but even the practitioners are guarded in their enthusiasm about its extensive use. Fishel recognised that the cost of the procedure is the time taken by research scientisits to make the required subjective estimates — time they could productively spend on research itself.[49] He concluded that blanket use of the approach would be an unrealistic drain on research talents. Slightly more optimistically, Easter and Norton concluded that, although benefit-cost evaluation of research is a major task, it has the advantage that the level of analysis can be kept relatively simple.[50] The key to the approach is generally considered to be the co-operation of scientists, because their estimates of potential outcomes are critical.[51] Ruttan regards consistency of the approach as the main advantage, but recognises that any method, simple or sophisticated, is no better than the judgments made by the research-scientist reviewers.[52] Schuh and Tollini argue that the reasons why the approach has not had more widespread use include its costs and time-consuming nature, and the problem that the pooling of a large number of opinions may do little more than pool ignorance.[53] Perhaps the most pessimistic views on the applicability of the benefit-cost evaluation method are those of Arnon.[54] He considered that, for many kinds of research project, it is not possible to measure with reasonable accuracy the costs and social and economic benefits.

(g) Information systems

Judgment of which of these techniques is better than others is meaningful only given a detailed specification of the situation in which the technique is to be applied. Even then, the choice of technique is extremely difficult. Souder outlined the vast array of attributes to be taken into consideration when selecting an evaluation technique.[55]

As Baker and Freeland pointed out, there has been a shift in emphasis from the techniques themselves towards concentration on information systems within which the techniques are used.[56] One reason for the shift is that the existing techniques seem incomplete, because they do not include all relevant aspects of the research environment. The quest has been to establish information systems which will enable good research-allocation decisions to be made using various evaluation techniques. Examples of this work include studies by Baker and Freeland, by Geoffrion, Dyer and Feinberg, by Baker, Shumway, Maher, Souder and Rubenstein, Baker and Sweeney, and by Winkofsky, Baker and Sweeney.[57] Another aspect of this shift is the recognition that subjectivity is an intrinsic part of any *ex ante* evaluation of agricultural research, and that quantification is not synonymous with objectivity.[58] Thus the shift in emphasis towards models of information systems represents an attempt to handle better the subjective aspects of evaluation.

THE AMOUNT OF RESOURCES TO DEVOTE TO EVALUATION

The diverse methods available for quantifying over time ($t = 1, \ldots, T$) the benefits (G_t) and costs (C_t), or surrogates for these, associated with alternative research projects ($j = 1, \ldots, J$) and programs ($m = 1, \ldots, M$) have been outlined above. Let a program (X_{mt}) consist of a selected portfolio of projects (x_{jt}). For any given program at time t_1, there will be a stochastic stream of net social benefits (β_t, $t = t_1, \ldots, T | X_{mt_1}$) and the appraiser/allocator/decision maker of the research budget presumably manipulates X_{mt} over time in order to maximise some intertemporal welfare function of these benefits, say $U(\beta)$. In addressing this task, various levels and intensities of evaluation may be used, ranging from informal holistic judgment and decision by an individual through the more complex and data-demanding formal models, perhaps implemented with committee-based judgmental inputs and decisions. Let such resources devoted to appraisal be encapsulated in a cost index of the (opportunity) cost of all the resources so involved (A_t). Then the appraisal question can be posed as choosing:

$$(8) \qquad U^* = \max_{A_t} E[U(\beta(A_t))].$$

The nature of the key relationship $\beta(A_t)$ is quite obscure and is thus open to considerable speculation. Intuitively, it seems likely to feature an initial range of increasing marginal returns, followed by probably a wide range of diminishing marginal returns and, inevitably, negative marginal returns — such as where the creative energies of research workers are totally preoccupied (or destroyed!) by attempts to meet (or outwit) the demands of the assessment system. As the evaluation possibilities do not follow a smooth continuum, both A_t and $\beta(A_t)$ will be irregular discontinuous functions. For further complication, not only is β inherently stochastic through the intrinsic uncertainties of the research process, but $\beta(A_t)$ involves additional uncertainties as to the impact of different A_t inputs on β. For instance, research appraisal personnel must surely vary greatly in their capacities to cast judgments on research planning and the impacts thus depend on the idiosyncracies of individuals.

This type of work is very undeveloped, but there are a few pointers in the literature. Pioneering conceptualisations were provided by Matheson, and by Smallwood.[59] The most cogent and developed advancement towards an operational framework for assessment of decision analysis modelling is that of Nickerson and Boyd.[60] Their approach encompasses decision problems of the research planning type, although they have not considered optimal levels of evaluation *per se*.

In the absence of empirical data on $\beta(A_t)$ in the field of agricultural research, we are obliged to resort to conjecture about some of the relevant effects. This is undertaken by means of hypothesised generalisations about such quantities as the optimal research evaluation effort, A^*. Naturally, these parallel to some extent analogous hypotheses that could be assembled for optimal research investment itself.

(I) Industry aspects

(a) A^* increases with the average size of industry revenue.[61]
(b) A^* decreases with increasing total productivity of the industry.
(c) A^* increases with the geographical scope of the industry.

(II) Research aspects

(a) A^* increases with increasing number of projects considered.
(b) A^* increases with the number of potentially selectable programs.[62]
(c) A^* increases with the uncertainty of the research environment.[63]
(d) A^* decreases with increasing productivity of research.

(III) Evaluation aspects

(a) A* increases with the flexibility (e.g. via external review inspired redirections) of research allocation.[64]
(b) U* does not necessarily increase with such flexibility.[65]
(c) U* does not necessarily increase with A*.[66]

(IV) Assessor aspects

(a) A* decreases with increasing expertise of informal assessors.[67]
(b) A* increases with the number of voters in the decision unit.

(V) Trade and distribution of benefits effects

(a) For exporting industries, A* decreases the less is the downward pressure on world prices of cost-reducing research.[68]
(b) For import-competing industries, A* decreases the more is the downward pressure on world prices of cost-reducing research.[69]

These hypotheses are indicative of those on which more information is required before the state of the art of research evaluation can significantly be advanced.

CONCLUSION

The diversity of models that have been proposed for appraisal is brought out in the review. The base of experience and application is so slender that a definitive conclusion about 'best' appraisal schemes for particular circumstances cannot yet be drawn, and such a conclusion is clearly still a distant possibility. The tentative hypotheses drawn out reflect this fragility of knowledge. It behoves us, however, in the spirit of subjectivity that pervades most of the evaluation models and especially the scoring models, to attempt to distill some 'best guesses' as to desirable practices that Australian administrators of agricultural research resources might consider implementing.

Considering first the Rural Industry Research Funds (RIRFs), a 'representative' large fund (and industry) is wool, and a small one is, say, chick-peas or, indeed, any other of the minor industries swept together in the Commonwealth Special Research Grant (CSRG). It seems that for such a small industry, the potential gains from research, the shallow information base of the industry, and the limited number of competing potential research projects together conspire to make anything other than holistic, intuitive appraisal by knowledgeable authorities suboptimal. Even the scoring system employed by the *ad hoc* CSRG advisory committee probably represents an 'overkill', although it is doubtless resorted to in order to facilitate choice among diverse projects for disparate industries.

At the other extreme, the wool industry is large in Australian research and production terms, is well served by information systems, and features strong competition for its research support. Our guess here is that a fairly elaborate scoring system might be the best scheme to assist in evaluation. At an earlier and less experienced stage in our thinking, we might have speculated that a rather more comprehensive and information-intensive scheme of fully-fledged (including probabilistic) evaluation of benefits and costs for projects and programs would be desirable. However, the costs of assembling 'decent' data on projects that potentially affect various segments of the industry with differential impacts, time scales and innovation lives are seemingly so high as to dissuade us from this view. Just what sort of more modest scoring system is 'ideal' is a further and as yet unanswered question, but it seemingly should combine in a logical fashion scores for the key variables of: probability of success, profitability and life of contingent innovations, and cost of the research — all against a background of the opportunity cost of research funds in the industry over recent times.

By implication, industries of intermediate size should fall somewhere within this spectrum of scoring models, with very simple, probably crude, systems for small industries and more disaggregated and complex systems for the larger ones. Clearly, the personalities involved in each will determine the type of system judged best for each case. Parallel implications must hold for privately funded research although, since several transnational companies are involved, the portfolio problem for an internationally dispersed research program will tend to make the selection of more complex systems more profitable than for country-bound RIRFs.

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