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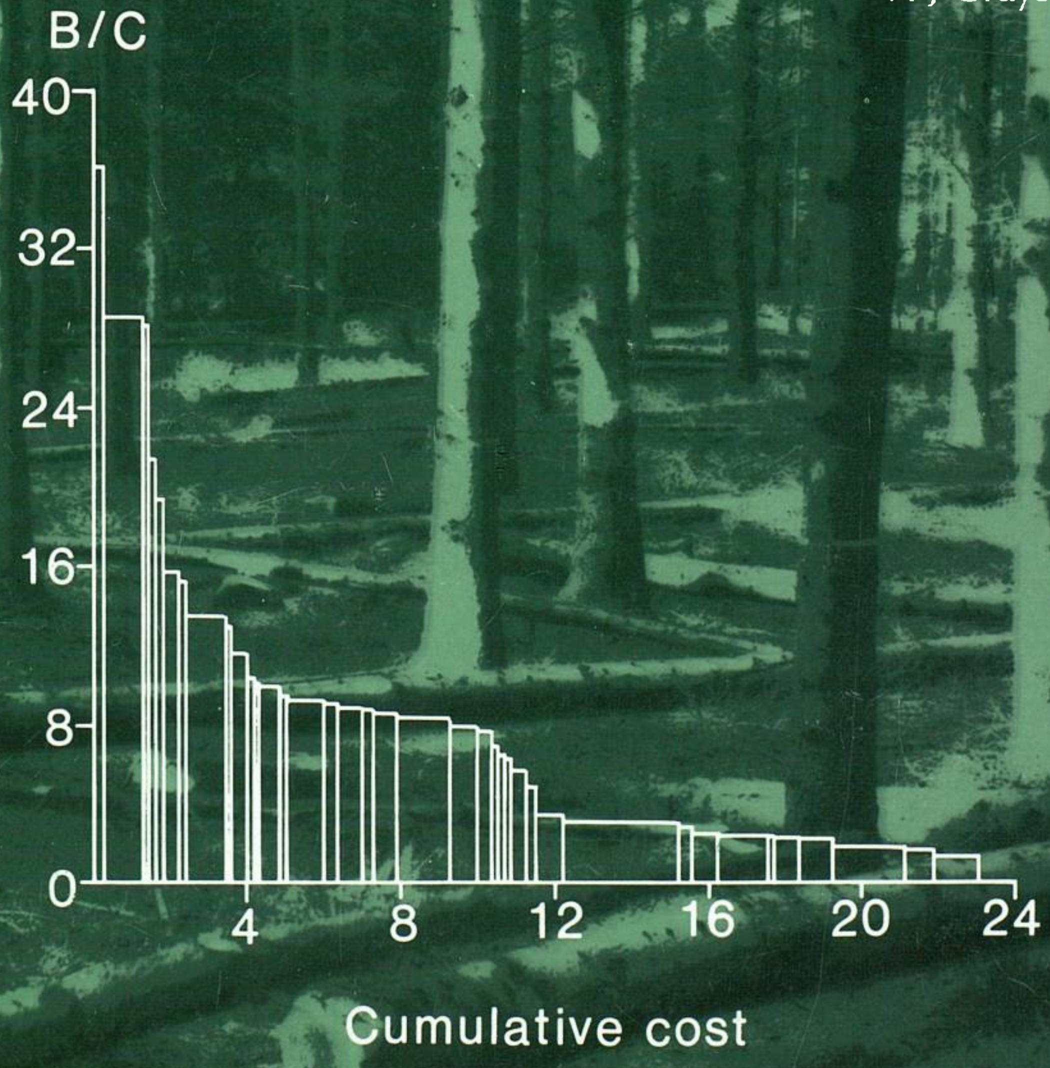
Evaluation of Forestry Research

A J Grayson



Evaluation of Forestry Research

by A.J. Grayson
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Foreword

The Forestry Research Co-ordination Committee was set up by Ministers following the 1980 report of the House of Lords Select Committee on Science and Technology entitled *Scientific aspects of forestry.* The second of the terms of reference of the Committee is "to advise on research requirements and priorities in relation to the needs and opportunities identified".

The evaluation of research has naturally concerned the Committee in its work, whether considering the whole field of forestry research or a particular subject area. As the discussion of existing techniques in this paper shows, there has been a considerable amount of effort devoted both to the theory and practice of research project evaluation in manufacturing industry and agriculture. From time to time there have been various initiatives within the Forestry Commission and the Institute of Terrestrial Ecology of the Natural Environment Research Council but no sustained attempt made to deal with the variety of problems encountered. The need for some quantitative measures both of the promise of new projects and the achievement of completed projects has stimulated this paper as a contribution to the work of the Forestry Research Co-ordination Committee and as a contribution to the discussion of quantitative evaluation of proposed and current projects. The Committee and the author will continue their consideration of the methods proposed in the present work and developments of it.

R.T. BRADLEY
Chairman,
Forestry Research Co-ordination Committee

Preface

Following earlier work in the 1970s initiated by the then Director of Research, Mr G.D. Holmes, the writer became interested in the twin matters of desirable expenditure on R&D in forestry and the evaluation of individual projects. In 1979 Dr W. Liese, Head of the Wood Technology Department of the Federal Forest Research Institute, West Germany and then President of the International Union of Forestry Research Organizations (IUFRO), invited the author to investigate the practicability of evaluating forestry research programmes with special reference to the needs of developing countries where the need for value for money was considered particularly great. Subsequent work was undertaken in a working group within Division 4 of IUFRO, supported by the Divisional Co-ordinator, Professor R. Plochmann of the Department of Forestry, University of Munich. The present work stems from these studies and the later stimulus provided by the Forestry Research Co-ordination Committee.

I wish to acknowledge the encouragement of three colleagues on the Committee, Dr Bowman, Professor Jeffers and Dr Mutch, while emphasising that the responsibility for the present paper remains mine alone. I am grateful to Mr A.J. Peace of the Statistics and Computing (South) Branch of the Research Division for his help in computation and for his inventiveness in developing ways of calculating B (Section VI).

A.J. GRAYSON
Director of Research

Summary

The subject of evaluation is important to research managers generally and especially to those concerned with the formulation and direction of programmes. Evaluation is normally impracticable at the programme level; instead attention must be concentrated at the level of the project. The purposes served by such evaluation are three: to increase the awareness of research managers about the likely impacts of their choices, to provide a more critical basis for varying or stopping projects and to improve the choice of new projects and their design. The lack of appropriate data limits the methods available for evaluating forestry research to cost-benefit analysis. The components of total benefits distinguished are: expected contributions to outputs which can be priced, a score for expected benefits in the field of environmental outputs which cannot currently be priced, and a score for scientific value which is defined as the contribution to knowledge not captured in the other two components of total benefits. While these three elements can stand alone, their combination in a single measure is useful and methods are described for calculation of weights to make them commensurable. The results of assessing 45 current projects in the Research Division's programme are presented together with observations on the effect of differing weights on project ranking. Needs for further work identified are: more applications of available techniques for valuing environmental benefits in such fields as wildlife conservation, practice in peer review and the estimation of scientific merit, and experience in the assessment of the probability of success of achieving project objectives.

Evaluation of Forestry Research

I. Introduction

Every trade and profession has its methods for assessing performance. Performance is used to refer to expected or achieved action: in economists' jargon one may be interested in either *ex ante* or *ex post* assessments. In some professions the test is adherence to a set protocol or drill, in still others financial management accounting provides the touchstone. In research management, there are marked contrasts between the treatment given to work at the applied and development end of the spectrum and at the other extreme that given to fundamental research or the more blue-sky end of strategic research. In industry, and particularly manufacturing but also including utilities such as power and water supply, the scope for applying traditional investment appraisal to applied research is wide and not substantially more difficult than that undertaken in the production operations of such enterprises.

In forestry[1] the pattern is different. In the first place much research serves objectives that are themselves not well defined and more poorly quantified. Examples are wildlife conservation and water quality. Secondly a wide range of work is concerned, from the most basic in physiology and genetics to such development work as the design of harvesting equipment. As with other industries, the objectives of the particular firm or agency may influence the values attributed to success in a particular research endeavour and accordingly the general emphasis of research that is undertaken.

The approach set out below is intended to be sufficiently general to allow its application to all cases of forestry research programmes. It calls for competence in a number of areas of applied economics and in this regard may serve the useful function of emphasising the need for further work on subjects that are necessarily of importance to forest managers. But in relation to projects in fundamental science, the approach can only carry the research manager a little way on from the systems ruling to date. It is hoped however that it will stimulate more critical appraisal using time-honoured techniques as well as the generation of new methods.

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(1) In this paper, attention is confined to forestry proper, by which is meant the cultivation and management of trees for the production of goods and services including harvesting. Forest industry research is excluded, though wood science as opposed to processing technology is in principle covered.

II. Basic considerations

It is necessary to be clear about the target of the approach described here. It is not, except by summation, all research, or even whole research programmes in a number of separable fields. It is essentially concerned with projects or groupings of projects for which likely costs and likely benefits can be identified. Forestry research and development programmes, unlike aircraft and weapon systems programmes, do not have single goals in which success of the whole depends quite critically on all component parts working together. A typical programme consists of a set of projects, each of which serves a given operational requirement, plus some more general strategic research expected to be of general application. The programme does not stand or fall by success in all, or even a majority, of its parts. In this it is more parallel in the league of big science to CERN than, say, a space station. In an important regard the atomistic nature of forestry research has a further distinguishing feature from the single large project, namely that in allocating resources the research manager is faced with what is in principle a smooth distribution of projects of varying worth rather than a lumpy distribution calling for the exercise of greater courage in judging among competing claims. Courage and judgment are called for in the latter case because a 'wrong' decision is likely to be far more costly than can be true of errors of selection among a large number of small projects.

As is made clear below, there is no upper limit to the grouping of projects considered given the possibility of evaluating each and every project within the set. This is a fundamental point to grasp and emphasises a major restriction of the method outlined which has important implications for its mode of application. In other words the method offers no easy way to deciding how much expenditure in R&D a given activity may justifiably attract. This implication arises because it is believed that, even if there were some formula for determining the 'correct' level of expenditure, project selection remains a question. Additionally, but independently, it is doubtful whether, except by crude analogy, there is any such formula to guide those allocating money to R&D expenditure for a given activity. Before describing the method it is necessary to establish the purposes of evaluation and then to review the techniques developed to date in order to identify their strengths and limitations and, in turn, to recognise the strengths and weaknesses of the proposed method.

The identification of the range of benefits covered differs according to the evaluation approach used. For example some commonly used methods have no way of dealing with the bulk of environmental values simply because market prices do not exist for them. It must be regarded a failure if an evaluation system for forestry research makes no provision for an essential ingredient of so much management and research activity.

The ordering adopted is in Section III to consider the purposes of evaluation in terms of who is served and what use can be made of results, including discussion of *ex ante* and *ex post* approaches, in IV to survey existing methods of evaluation and their applicability to forestry research, in V to set out the methods proposed for use, including problems of economics, scoring and weighting, in VI to review worked examples and the results revealed for a particular institution, and in VII to indicate further development and possible applications of the method.

III. Purposes of evaluation

Although the immediate aims of research evaluation are diverse, ultimately the concern of research managers and policy makers is to have critical guides to action, whether at programme or project level[1]. Two main kinds of questions are raised in research management discussions. One concerns the desirable level of research activity serving an industry or the scale of a particular research programme. The other centres on the promise of a project or a closely related set of projects. In both cases, a frequent appeal in argument is to past performance of research although critical assessments of expectations are the more relevant guides.

Ex post evaluations have a particular role in providing a general idea of the productivity of research. They serve an interesting but hardly decisive purpose since there is no necessary link, however much some features of peer review (see below) depend on it, between past and future performance. In the United States, a large publicly supported programme such as that in agriculture has clearly gained support from demonstrations of the link between R&D and economic success in this sector. It has been argued (Fedkiw, 1986) that *ex post* evaluation has no role at the project level because individual projects will not be repeated and the results, that is their profitabilities, are so variable. This seems too sweeping a condemnation. One does not have to be an extreme reductionist to appreciate that understanding of the way in which projects are formulated, evaluated, managed, terminated and their results transmitted can only come from close study. Gains in such understanding must help in the formulation of future projects.

A useful assessment of the aims of research evaluation, relevant to this point, is given by Skok (1986) and the discussion of his paper. It is not the purpose of this paper to provide tools by which results selected solely to impress those financing the activity may be obtained, effective though this may be. It is instead to review methods of guiding research managers and policy makers to better decisions on the scale, emphasis and approach needed to satisfy research requirements. This is a continuing activity which calls for continuing research evaluation.

Indeed evaluation is so fundamental to the initiation of projects that it is in any case an unavoidable activity *ex ante*. In relation to a decision on the total research budget, it is logically impossible, as with all economic issues, to approach the best overall result without considering the whole field: the fact that research in forest protection (say) shows a higher profitability than research in silviculture does not mean that some other field such as harvesting research may not be even more profitable and deserve a higher allocation before more resources are devoted to protection research. The process may properly be carried wider still: the research budget of an enterprise has itself to be decided in the light of the returns from all the enterprise's operations.

It will be clear from the foregoing that whatever the technique used to assess the value of research there is no way of avoiding an economic approach nor is there cause to. Research and, within a given research budget, individual projects compete for resources and it is rational to adopt a strategy which is expected to yield the maximum output. Secondly, it is not helpful or responsible to suggest that because different projects produce benefits of such different kinds as gains to wildlife conservation and extra wood that the two sorts of value cannot or should not be brought together. Appeals to certain values being 'transcendental' have no operational meaning. What is however relevant is to recognise that different agencies may because of different remits place different relative values on the putative benefits of research, difficult though this may apparently be to justify.

A useful discussion of the use of different ethical systems affecting real-life decisions is provided by Kneese and Schulze (1985). Their analysis invokes a dramatic issue, that of nuclear waste storage with

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(1) Lundgren (1986) provides a useful guide to forestry research evaluation in the US context.

unkown long-term consequences of a (potentially) dire nature, in order to illustrate the idea that in certain circumstances a different ethic from that underlying conventional welfare economics may be appropriate. Such extreme cases are most unlikely to arise as forestry issues, and so become the subject of possible research. Finite resources and unbounded demands ensure that this discipline of making the fullest use of resources is imposed on all those responsible for decision making, whether the objectives of the programmme relate only to economic measures or, as more usually, to economic, scientific and political purposes.

An important consideration in research evaluation is education of those in the system, namely policy makers, research managers, researchers and forest managers. This function is a valuable one and especially relevant when changes in the conventional system of appraisal are being introduced. Managers and scientists have different goals and this can engender conflict and waste. With a good system of evaluation, greater collaboration may be expected to maintain high morale and enhance research productivity[1].

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(1) I am indebted to Dr Grant Milne, Canadian Forestry Service for this point.

IV. Existing evaluation techniques

Current methods may be grouped into three classes. One is internal to the scientific world, the others are economic.

Peer review and developments from it

The traditional technique applied *ex ante* is peer review. In the course of a project or programme, and certainly *ex post,* two other techniques of assessment, namely number of publications and citation analysis (see Herman, 1985), can be used. In a useful review of these techniques, Irvine and Martin (1985) illustrate the potential for combining techniques of interview of scientists close to the subject area with bibliometric analysis. This system avoids possible deficiencies of traditional peer review such as the tendency for members of decision taking bodies to represent the attitudes of past periods and so to freeze earlier established priorities into the system. The interviews may be designed to elicit characteristics of the competing institutes and their programmes so that a more critical and informed appraisal is achieved. In order to gain a view on future prospects of a number of competing institutes, the interviews conducted by Irvine and Martin were centred on questions regarded as important for success in the field concerned, and the ranking of contending facilities then compared. The purpose of these activities was to test more critically the attitudes and opinions of those in the field instead of relying on peer review pure, if so it can be, and simple. In this there are similarities with the Delphi technique which searches for the reasons for and confidence expressed on certain opinions by taking respondents through a number of rounds of enquiry. A further illustration of a structured approach to peer review in a forestry context is provided by Davis and Shafer (1984). This used a series of stages of assessment starting with paired comparisons of all the research proposals concerned.

All such methods are open to the criticism that they remain too far independent of the final consumer. Their application must lie principally in fundamental research. Inevitably a problem exists of relating rankings with the costs of the alternatives. The rankings by the 'partial' indicators illustrated by Irvine and Martin are ordinal, whereas costs of alternative programmes are cardinal. In the final analysis these investigators resort to scoring the relative advantages and disadvantages of particular projects by reference to such features as construction requirements and the ability to mount a variety of experiments. For certain partial measures such as publication numbers, unit cost calculations are feasible (see Boyce and Evenson, 1975) but their interpretation remains problematical.

In strategic or applied research, the possibility of approaching more nearly a money assessment of pay-off radically alters the character of the problem and the method outlined below offers a tentative solution to the difficulty. Insofar as comparisons have to be made with other projects the outputs of which are easily evaluated in money terms, there is a further problem of making outputs commensurable. In this regard institutions conducting forestry research are unlike those which have one principal goal and one major facility: the techniques required to serve the needs of diverse programmes must be more comprehensive.

General comment on economic approaches

The internal nature of peer review, even when questioned and tested as indicated above, opens the method up to the criticism that the paymaster has to take too much on trust. In fundamental science it is difficult to see what the non-expert can offer although scientists often emphasise the importance of asking the difficult, or absurd, question.

The appeal of economic approaches lies in the fact that, assuming such methods can be applied, the point of reference is a price system that reflects, or should reflect, society's wants. To the extent that the research is not strategic or applied, future outputs such as new processes or materials cannot be imagined, and still less their prices estimated, and economics has little if any role to play on the demand side through its contribution to calculation of value. But the extent to which economics, including social cost-benefit analysis (that is analysis of costs and benefits to society as a whole), can be applied elsewhere is considerable.

Production function method

This technique relies on the identification of the factors controlling change in output in a given sector of economic activity and the ascription of that element of change in output which cannot be ascribed to changes in the quantity and quality of labour and capital employed to the application of research. This technique has been applied in broad fields such as the economy at large (see for example Solow (1957) and Denison (1974)), by Ruttan (1982) for agriculture, and workers such as Mansfield (1964), Mansfield *et al.* (1977) for particular manufacturing industries. Unfortunately while good statistics are available for the contribution of most industries to national income and these can be disaggregated to individual sectors, such as crops in agriculture or quite narrow groupings in industry, comparable statistics are not compiled for forestry except in a few countries such as Finland. Even if the requisite data are available, the time pattern of gains from research applied has to be identified. Much ingenuity has been displayed in selecting appropriate lags (see especially Mansfield, 1964; Bengtson, 1985).

The estimation of surplus

The second economic method also relies on market prices and works by explicit estimation of shifts in the supply curve for a given commodity. The purpose of research, as of other investment, is to shift the supply curve for a commodity to the right. The supply-curve relates price to the quantity of the commodity supplied (Figure 1). A rightward shift implies that at a given offer price a greater quantity will be supplied (Figure 2). Given knowledge of the supply shift the object is to estimate change in social 'profit' by adding the gains in consumer and producer surplus and ascribing the gain, other things being equal, to the research element responsible for the supply shift. In practice identification of the causes of the shift is not so unambiguous a process as with the production function route. The classic illustration of the technique is that on the return to research on hybrid corn (maize) by Griliches (1958) and a substantial literature has grown out of that seminal paper. The experience gained has been useful in identifying the pitfalls of appraisal and some of the relevant lessons learned are noted in the following presentation. It is important to note that such calculations of change in surplus require the estimation of supply and demand curves for the output or outputs that research influences. Unfortunately not all the expected benefits of forestry research can be so handled.

Extension of the surplus estimation: cost-benefit analysis

The estimation of surplus is easier where gains in output do not materially influence price. In contrast to the circumstances investigated by Griliches, the position with wood production in Britain is that the research programme under review has insignificant effects on total world supply and hence on world price, itself the determinant of British wood product and hence tree values. This implies that the whole of the gain is to producers rather than to consumers. The simplifying assumption commonly and not too erroneously adopted by those evaluating forestry research projects which are expected to shift the

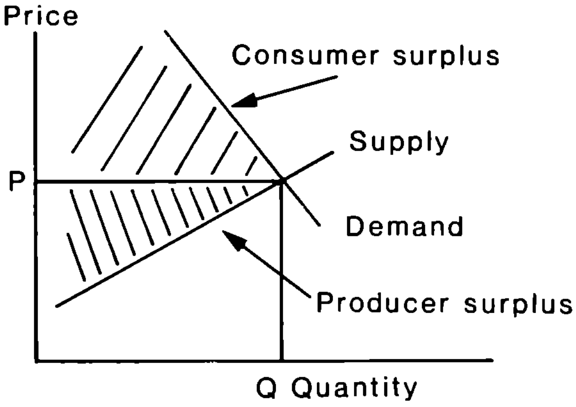


Figure 1. Consumer surplus and producer surplus.

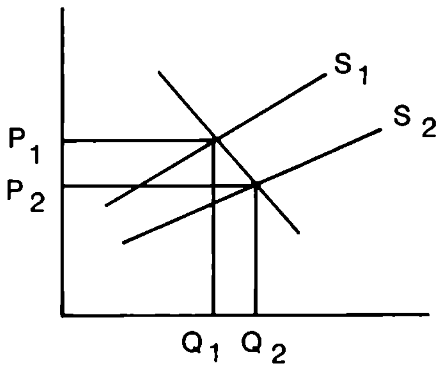


Figure 2. Effects of shifts in supply curve from S_1 to S_2.

supply curve rightward is that producer surplus increases by the increase in output times the price. As Lund *et al.* (1980) point out, this is a serviceable approach and although more precise estimates can be made on different assumptions about the slopes of supply curves, a point also reviewed by Norton and Davis (1981), Kingma (1984) and Wise (1984), these issues are simply noted here. The general point that has to be remembered is that conventional cost-benefit analysis (C/B-A) of, for example, public works, explicitly assesses dynamic elements of change where major supply shifts are engendered by the project. The situation in British forestry, however, is that analysts remain ignorant of the future structure of the wood using industry, the pace of change in demand for non-market goods and services and other relevant factors. The C/ B analyst of research is thus working comparatively blind: it is to be hoped that the exercise of project evaluation may itself encourage more of the needed research in forest economics itself.

Most of the evaluations of research in manufacturing (where the production function approach has been most commonly applied) and in agriculture (where the surplus approach to estimation of benefits to a particular crop has been the commonest method) assess the contribution of research in terms of gains that can be measured on the market. The market may be a free or, as with agriculture in Britain, an administered one[(1)]. However there are many research programmes, particularly in the public sector, which concern values in the environmental field for which markets do not exist. The method described here aims to extend the investment appraisal made by such analysts as Griliches and developed more generally by Lund *et al.* to take account of non-market benefits in such areas as water, recreation and wildlife conservation, subjects which have not as yet excited the attention of agricultural researchers or agricultural economists helping to evaluate their projects. While cost-benefit analysis is difficult, if only because it aims to be comprehensive, there is no blinking the fact that many research projects involve expected gains in activities which do not produce traded goods, which compete with other projects and which accordingly fall to be considered along with other investments in R&D.

In addition to effects of research findings on environmental factors, there will often be impacts on values so far removed from practical applications as to defy evaluation by reference to their possible impact on identifiable physical realities. This aspect of scientific value cannot be ignored, difficult though its incorporation into a cost-benefit calculus must be. Forestry research programmes contain examples of projects that span the whole range from those influencing the production of marketed goods through those producing gains in the environmental field and those at the fundamental end of the research spectrum to those containing elements of all three. In these circumstances it is highly desirable to adopt an evaluation technique that is appropriate to the whole span of project types.

A further consideration concerns the commonly-voiced, though less commonly criticised, notion that spurious accuracy will be conveyed by the quantification that is implicit in a C/B-A. As already noted, decisions about starting projects, running them at a particular rate and stopping them, are made on the basis of a number of factors among which possible gains from application, probabilities of success and future cost to completion all have to be weighed. That this is an essentially quantitative process can hardly be denied. Indeed as Kellert (1984) has argued in relation to decisions concerning marketable goods on the one hand and environmental values on the other: "The primary assertion is that, until such specification and measurement occur, decision-makers and the public will continue to be biased towards those values that can be quantified, especially in dollar terms. The environmental trade-offs confronting this nation are simply too important to permit the depreciation of intangible values presently not subject to empirical estimation. The challenge is to establish numerical standards for all environmental values that are applicable across diverse settings, and can be used to assign relative quantitative weights to various land use decisions." Though the author of this view writes from a different standpoint, his concerns echo those of a keen advocate of C/B-A (Pearce, 1985) who confirms in a short, sharp summary the views of a recent OECD conference on environment which favoured continuing attempts to apply the analysis to decision making in the environmental field. A valuable survey of applications of C/B-A to research is provided by Greig (1981) who refers to over 36 such studies in agriculture and manufacturing industry.

One aspect which is not covered in the present paper but which has excited the attention of analysts is the matter of the distribution of benefits between producers and consumers. In the circumstances of British forestry this question is almost meaningless in the field of forest products where British research on growing can only have a small effect on prices and growers gain the bulk of the benefit, but it may be relevant in recreation and conservation where improved provision can be expected to reduce prices of enjoying such services. In practice there

(1) As Schultz (1979) points out, agricultural research is not immune to the pricing distortions current in the world: he mentions sugar beet in Western Europe and the USA as examples. The position has become growingly more absurd with recent CAP developments.

will be projects for which not all the expected benefits will be positive: a development which favours fish may cost some wood or vice versa. The groups of people affected may not be distinct but if they are there are commonly accepted techniques for balancing one's advantage against the other's disadvantage: losses can be compensated. Thompson (1972) provides a perceptive illustration of the principles involved by reference to the Roskill Commission's work on a third London airport. It would be wrong for research managers to ignore such considerations; research is not, as one is constantly reminded, value free and this topic of benefits to one group and costs to another is liable to be the one that enshrines the only substantial distributional problem in project evaluation. Where income distribution effects are felt worthy of investigation, it is generally agreed that the way to handle them is to attach calculations of the incidence effects to each evaluation so that the research manager or other decision taker may see the price paid in terms of efficiency (or highest benefit/cost ratio) by each shift in the provision of incremental distributional gain (Blaug, 1985; Bengtson and Gregersen, 1986): a similar procedure is recommended here. It should be noted that the factors that determine the distributional impacts of research are highly relevant to the issue of private/public responsibility for research (Hyde and Seldon, 1986).

V. Proposed method

The method proposed divides the benefits of research into four:

a. expected value of gains arising from application of likely research result in terms of marketed output, principally wood;

b. expected value of gains arising from reductions in costs of operations;

c. expected gain in environmental benefit arising from application of likely research result; and

d. score for scientific value, representing gains not captured in a. to c.

In order to carry out the evaluation it is necessary to identify project objectives and to assign a probability of success of their achievement. The benefits expected from a given project will vary over time depending on the speed and scale of application of results and the speed and scale with which other developments will overtake the results of the project under review. The subjects of assessing type c and d benefits and of assigning money values to them are discussed later in this section and aspects of calculating each of the four components noted in Section VI. The usual discounting procedures, with due attention to changes in real costs or prices over time, are applied.

Wood production

The first component includes expected changes in the value of wood output through quantity or quality change or both. The major difficulty in this calculation is estimation of the average increase in volume. Price in the British wood market is regarded as a good measure of timber's value to society though formal statements of the view are difficult to find, with Jackson (1974) one of the few writers alluding to the point. In view of the concern that arises in agriculture on this matter, the following points require to be made: first, an insignificant segment of British imports of forest products is subject to tariffs or quotas; secondly, there are no subsidies on harvesting wood though there are some on the establishment of new pulp and paper mills and particle boardmills; thirdly, there is free entry to firms undertaking harvesting and processing; fourthly, the methods of sale favoured by the one very large seller are not such as to limit competition; and fifthly, there is free access to market information. This position, so rare in the wood market of many countries, accords well with the conditions applying in perfect competition, though it should be noted that rates of return on some wood processing investments may be higher than normal as a result of market power deriving from the control of technology in an oligopolistic market. The study of wood prices, which in the present context means standing tree prices, is limited, though Mitlin (1987) derives useful relationships between prices received in sales of standing coniferous timber from Forestry Commission forests and tree size and broad location. Quality differentials are not easy to value because price statistics are weak on such characteristics as log grade or internal features of the wood. Where such measures are needed recourse must be made to conversion or utilisation studies on wood exhibiting variation in the relevant quality variable.

Cost reduction

Expected cost reductions are usually more readily quantified than yield increases. By cost is meant the value of resources required to perform such operations as fencing, planting and harvesting. For certain elements, notably relatively unskilled labour, market values of labour may exaggerate the social costs of these inputs. Adjustment should be made for this difference. In addition it is important to ensure that variable costs only are considered: fixed costs such as supervisory and administrative overheads can normally not be counted. Westgate (1986) provides a useful worked example. Finally, the effect of a cost reduction on the appropriate intensity of an operation (such as drainage) cannot be considered in isolation; the effects on benefits such as wood output per unit area cannot be ignored. This effect is commonly quite significant. On the other hand it is less likely that a change in practice resulting from research will influence the scale of forestry programme, although it must be admitted that estimation of this elasticity is lacking.

Environmental outputs

Gains in output of goods and services usually labelled environmental are difficult to evaluate because most of such commodities are not traded. There is only one feasible course, namely to value those which yield to cost-benefit analysis and to estimate scores for those that, in the current state of the art, do not. The reason for favouring this course so firmly is that careful evaluation of benefits in the wood and cost fields show the prospective values of different projects to differ far more than might be estimated intuitively. There is no reason to believe that the same does not apply to other types of benefits and therefore it is important to diminish the extent of the subjective in research evaluation, easy though the scoring option may be. In addition, to be worthwhile, reasonably careful C/B-A of environmental benefits is not lightly undertaken and therefore, where important, values requiring such derivation are likely to be well based. The relevant benefits identifiable today are set out below.

Water

Quantity effects assessed in terms of gross annual consequences can readily be evaluated (HM Treasury, 1972; Grayson, 1974) and, given enough information on the system of management applied, the analysis can be adjusted to account for differential seasonal or other effects associated with forest practices. Effects of forestry on water quality have recently become both more noticeable and more intensively studied. To the extent that assessments can be made of the physical effects a direct approach is feasible. Thus if by law quality has to be restored and the least cost route is by some physical amelioration such as special water treatment, this cost may be used to assess the cost of quality reduction. Or if the effect is not through some statutorily determined character, but through a marketed service such as fishing, then data on rents and licences should be used as estimators.

Shelter

A common forest influence is shelter of neighbouring properties. Little is known of the physical effects of forests on agricultural output and costs but, where available, appropriately adjusted values of, for example, sheep and cattle output can readily be inserted.

Amenity and conservation

The distinctions between the three principal 'environmental' benefits of recreation, amenity and wildlife conservation are blurred. All these services are or may be enjoyed simultaneously by those visiting, or, if we are not to omit 'option demand', those wishing to ensure that they may visit, forests. There are, however, as with any audience, degrees of intensity of the recreational experience gained by a particular individual's visit. The practice has developed of planning for provision of more and better enjoyment in each separate field, sometimes without considering the whole. Clearly there are sites where one feature's enhancement is of little importance to another feature. An example is the 'flow' country of Caithness where quite considerable value is attached to protection and enhancement of wildlife but serving this goal has negligible impact on recreation values because visit frequency is so low. By contrast, measures to enhance the landscape amenity of south Wales spruce forests may have substantial effects on the visual scene but little impact on conservation values which may not be highly valued to start with. These examples serve to emphasise that the distinction between at least major groups of benefits is real, despite the difficulties that may arise in valuation and the dangers of double-counting.

In the case of recreation, Clawson (1960) led to the creation of an objective method of valuation based on the calculation of a demand schedule linking visit rates of populations at different distances with the cost of travel to the recreation attraction. Despite concerns over the applicability of his method, the central thesis remains and adjustments can be made for the various elements that a careful application requires. For example variation in income distribution of visitors to different sites and the question whether travel cost is all cost can be analysed: Pearse (1970) and Grayson *et al.* (1975) respectively illustrate the effects of the necessary corrections. In landscape evaluation useful progress has been made in establishing a measurement system of visual amenity (for example in a British context, Robinson *et al.*, 1970) but very little work done on carrying this through to money evaluation. This is probably more a matter of lack of will than of practicability although it must be admitted that the small numbers of case-studies available for adequate testing of parameters creates difficulties. An obvious route is to use trade-offs as in studies of health care (e.g. Hurst, 1984; Williams, 1985).

It is perhaps in the field of wildlife conservation values that the greatest difficulties arise. Because a substantial amount of research is undertaken in this field by such agencies as the Institute of Terrestrial Ecology, the Royal Society for the Protection of Birds, the Nature Conservancy Council and the Forestry Commission, and such efforts are likely to increase, and also because forestry practices often have significant effects on these values, it is important to consider them separately. Some pointers are given in Appendix I where the possibilities of contingent valuation, as it is termed, are noted. It should be remembered that complications may arise because of the widespread use of the system in Britain of designating sites for habitat protection. This must have a marked influence on the way in which those interviewed by the contingent valuation approach respond because alternative areas for recreation and study to the particular site may be available. Pending empirical tests of the available methodologies recourse is had here and in the worked examples of the following Section to the use of scoring. As this has a bearing on the other feature of research output referred to above, namely scientific value, it is appropriate to set out some points for consideration at this stage.

Scoring

Scoring methods are attractive because they can be applied quickly and are comprehensible. They are also cheap. These features are good ones but this does not mean however that where inability to carry out the usual economic surplus calculation or fuller C/B-A limits the evaluator, he should not devote care to the construction of the scoring system. Ruttan (1982) reviews a number of scoring systems employed in agricultural research evaluation in the United States, some of which such as the 1966 National Program of Research for Agricultural Study cover all research goals among which many could be specified by alternative direct routes. This is to carry scoring to a needlessly all-embracing extent. By contrast, and in relation to issues similar to those under discussion here, Oltremari (1985) describes a scoring technique for allocation of public lands to national parks, recreational areas and the like. And in forestry Papanek (1981) has developed a method which is operationally applied to problems of forest management in a centrally planned economy. In research evaluation generally, Greig (1981) draws attention to some tests of scoring systems where other independent monetary allocation methods (C/B-A and linear programming) have been applied. The unsurprising general view is that scoring systems lack a sound structure: but in using the word 'sound' there is a risk of denying scoring any virtue. Some ideas from utility theory concerning many attributes should, as Greig suggests, help.

In the present context a more fundamental appraisal appears to be needed of conservation values than of landscape values where empirical methods offer an acceptable method. In wildlife conservation, goals require first to be clarified and the weights to be ascribed them decided. Examples of goals favoured are minimising the risk of extinction of a given species or habitat, and encouraging diversity. It is hoped that more discussion such as that set out in Usher (1985) will take place and thus allow more general agreement on relative valuations of different outputs. Scores are additive so that strong assumptions about relative weights are implicit in such summations.

Scientific value

Scientific value is defined here as a money sum representing the contribution to science not already captured in other elements of the benefit of the particular project. Part of this value lies in the expectation that the work may, in some unknown corner of some future development in the science concerned, contribute understanding so leading to applied research and thus application. But apart from this, there are what may reasonably be termed, *pace* C.P. Snow, cultural values. Publicly financed studies of astronomy or archaeology provide good examples of activities which are hardly carried on for their direct economic return in some market but, it must be assumed, because the values placed by society on their pursuit are judged sufficient by taxpayers to justify their continuing support. It is worth noting at this point (as Peacock and Godfrey, 1975, have pointed out in relation to museums and art galleries, and as the senior author has more recently stated in connection with his review of broadcasting (Peacock, 1986)), that there is nothing to preclude the operation of research institutes as ordinary commercial concerns.

The problem about relying on the market to determine price and the quantity of output at which demand is in balance with supply is that the market may fail to serve society efficiently. A major reason for this in forestry is likely to be the long gestation period for much research as well as the risk of failure. Apart from this, it is quite possible that though the gains to producers and consumers combined may exceed research costs, those to producers may not and may even, as Hyde and Seldon (1986) point out, be negative. There are other considerations such as the case where patenting does not prevent the rapid copying of research results or where patenting delays or dilutes benefits to consumers. These arguments tend to justify public funding of research. Some difficulties associated with reliance on competing firms to carry out research, such as the fact that an individual firm's work may benefit the whole industry but not provide a net profit to the firm, may however be overcome through an industry research association. In many circumstances it is apparent that only a public sector organisation which shares its findings is able to carry through the level of research work that is close to the socially optimal level. Strong evidence for this view arises from the work of, for example, Mansfield *et al.* (1977) who showed that the social rate of return on the R&D behind 17 innovations was double the private rate, that is the return accruing to the firms responsible.

By way of illustration of the concept of scientific value as defined here, consider two quite separate projects: F′ (F for Florey) and F″. The first, F′, leads to the identification of the potential of penicillin, while F″ is concerned with establishing the means for large-scale production of penicillin. Viewed as two separate projects one has, using X(1) to represent the value of penicillin produced, X(3) scientific value and C cost:

	X(1)	X(3)	C
F′	negligible	high	low
F″	very high	high	high

X(3)′ is the value of the science that enables F″ (and other antibiotic programmes) to be undertaken. X(3)″ is the further gain to science associated with debugging, if the phrase may be allowed in this context, the process, providing more insight into the biology of *Penicillium* and developing the chemical engineering techniques that remain as significant advances in knowledge over and above the gain in F″ itself captured in the value of X(1)″.

It is important to note in this example that F″ could not occur without F′ preceding it. Not only is this true but collapsing the two projects into one is a manifest impossibility. F″ is literally inconceivable without F′ which by itself could hardly have been established with a production programme in mind. Experience of research projects would suggest that the same pattern applies here. For example the notion that *Heterobasidion (Fomes)* might be controlled by the prior colonisation of surfaces of cut stumps by other fungi preceded the development of the means of providing that inhibiting agent.

It is also relevant to note that X(3)′ is not wholly independent of C′, though it is of course independent of the scale of F″ operations. There is no linear relationship between C′ and X(3)′ in fundamental science: one suspects a big leap for mankind is the nature of the response curve for some areas of science, though not so obviously perhaps with biological sciences. For more mundane research and especially the more applied work illustrated by F″, the contribution to scientific value may increase more smoothly with effort C″.

Translating the aim of assessing scientific value into action is the most difficult aspect of the appraisal. Two features may be noted. The first is that the scientific ability of the project leader, rather like the managerial ability of the leader of the production process in a development or other non-research project, may be the most significant determinant of the score assigned. It is that which informs the work of the laboratory assistants and foresters who work in the project leader's team. The idea is well recognised by Medawar (1985) when he refers to Shelley's classification of scientific creativity - 'poetry comprehends all science' - along with the form of creativity more usually associated with imaginative literature and the fine arts. It is true as Shelley put it that "a man cannot say I *will* write poetry - the greatest poet even cannot say it", and as Medawar reads across to natural science "no more can a scientist say I *will* make a scientific discovery; the greatest scientist even cannot say it". But the greatest scientist is, one assumes, great because he has excelled more than once in producing acts of imagination that turn out to be of importance: we have regard to a person's 'form'. The second point to note is that it is assumed that the probability of success of the project in achieving material goals does not influence its scientific value in the sense defined here because this does not embrace any of the measured benefits and cutting back the scale of the project because such benefits seem unlikely to accrue will automatically limit any possible exaggeration of the scientific value component.

The practical application of these ideas is for the evaluator to ascribe a score to scientfic merit and to apply this score to the resources devoted to the project on the basis that the merit of the project leader animates the project team and assures it the stimulus required to produce ideas and papers. In the assessment of scientific merit, it is suggested that some of the techniques, including bibliometrics, described by Irvine and Martin (1985) and referred to earlier should be applied.

Weighting

It is of course open to the evaluator to list for each project:

X(1) (the sum of benefits in wood output and cost reduction)

X(2) (the score for environmental benefits in fields for which valuations are not available),

X(3) (the product of the scientific merit score and cost of the project), and

C (the project cost).

This number of variables can be reduced by standardising X(1), X(2) and X(3) per unit of C. This is of some help in grasping the main features of a project's promise but still leaves the evaluator with the possibility of having to grapple with three numbers for each project. An alternative approach is to attempt an aggregation of projected project benefits by introducing weights applied to X(2) and X(3) and adding the results to yield a total benefit in money terms, B. Though it may be objected that such a construct relies on too many risky assumptions being brought together it is important to recognise that the two weights 'a' (on X(2)) and 'b' (on X(3)) can be varied and varied independently of one another to test the robustness of decisions among competing projects.

Two approaches to the determination of 'a' and 'b' are set out in Appendix 2. In the cases of the

Forestry Commission projects which form the basis of the Appendix' workings the results obtained by the two routes happen not to be wildly different; this is not altogether unexpected. It is improbable that the same set of figures will be found between agencies because although similar standards may be used in rating the X(2) and X(3) elements, the concern of the agency may be to favour projects strong in X(2) or X(3) rather than X(1) and the degree of 'play' introduced into the calculation may be so large as to create high variance of 'a' and 'b' across agencies. Although it is clear that the goals, including the emphasis on different subjects, of different public agencies vary, this does not mean that certain disciplines of public accountability should not of course apply.

So far as public sector research is concerned, the process of weighting expected money values in one field differently from those in another is difficult to defend. Apples and pears can be and indeed are made commensurate in money terms through price. But this need not mean that £1's worth of fruit is the whole story: those concerned with the production of (or research on) pears can still go their way and give pear production (or research) greater emphasis. This may be sub-optimal but is understandable. It is clearly desirable to search for a common mensuration and valuation of research outputs in all the fields touched so that each research manager can know the implications of his decisions and the opportunity costs of ones that cause a portfolio of projects to offer less than the maximum benefits. But if the forest service wishes to emphasise wood and wildlife, while a university research team favours recreation and contributions to pure science, that is still feasible.

Recognising this, it remains true that research organisations are not always explicit about their objectives and so managers and researchers remain unclear about exactly what emphases they are to give to different kinds of project. The assumption here is that objectives are clear. Babcock (1975) emphasises the necessity for this pre-condition and refers to inter-agency differences and the Canadian Forestry Service's outlook. Comparisons of results are clearly of value in this context and discussion is required to elucidate major differences. It would be unreasonable to expect harmony or high degrees of confidence until further experience is gained: as in econometric modelling one learns by doing and the more doing, costs allowing, the better.

Costs

The relevant costs of a project to set against expected benefits depend on whether an evaluation covers the whole period of the project from initiation to its planned end or only part of a project's life. In the latter circumstances it is possible to argue that certain elements of expenditure of the nature of general services or overheads remain fixed whether or not the particular project continues or not. This marginal approach has however to be handled with great caution; instead it is recommended that both for single project evaluations and for reviews of whole institute programmes all overheads including administration should be included. Table 1 of Appendix 3 sets out the cost composition of Forestry Commission Research Division expenditure as an illustration and indicates the high labour content of that expenditure. A convenient route to the assessment of a project's cost is to use the estimated number of man-years of project worker time and to multiply this by a unit cost multiplier. Table 2 of Appendix 3 shows the value of this multiplier for different groups of projects in a particular Branch of the Forestry Commission's Research Division.

Owing to the high labour content of research and the fact that the labour productivity of research appears to rise only slowly, unit cost per researcher might be expected to rise in real terms, as appears to be the case for other service activities such as education and health. With these activities constancy of expenditure in real terms, that is after deflating expenditure by a general index of prices in the whole economy, implies fewer, not unchanged, resources. The effect does not appear to have applied in R&D in recent periods. Mansfield (1984) found the price of R&D inputs doubled in the United States between 1969 and 1979 compared with an increase of some 90 per cent in the general level of prices as measured by the implicit gross domestic product (GDP) price deflator. A similar calculation for Forestry Commission research shows the results set out in Table 1. The reason for the slow real increase in this price index in recent years lies in the decline in real wages paid to government scientists.

In principle, explicit account should be taken of any real price change in research inputs in calculating discounted project expenditure and the price index should take account of gains in total factor productivity (labour, materials, services). In practice the small change shown in row 4 of Table 1 hardly warrants such adjustment.

Table 1. Price index of research inputs, Forestry Commission. **1980 = 100**

	1975	1980	1981	1982	1983	1984	1985
1. European Community price index for UK research	76.0	100	105.4	110.3	114		
2. Price index of FC Research Division inputs	51.2	100	107.7	115.3	121.9	128.0	135.1
3. GDP implicit price deflator	51.8	100	110.5	118.0	124.8	130.4	138.3
4. Real price index of FC research	98.8	100	97.4	97.7	97.7	98.2	97.7

Sources:

1. Government financing of R&D, 1975–82. Eurostat, 1983.
2. Forestry Commission calculation based on weighted index: salaries (46%), wages (17%), materials (17%), other (20%).
3. National Income Blue Book, Central Statistical Office.
4. Row 2 values divided by row 3.

VI. Results

Summary

The results of applying the foregoing methodology to 45 current projects are summarized here. Evaluations were made in each case for the whole project as viewed in 1986, whether new, mid-way through or nearly complete, and embrace total expected research benefits and costs from these relatively different vantage points. Clearly more confidence can be placed on the evaluations of nearly completed projects. Scores for X(2) and scientific merit (leading to X(3)) were ascribed by a single evaluator. Table 2 shows the main features of the projects concerned which accounted for approximately 48 per cent of the Division's expenditure in 1986. The selection of 1986 as the year to which all benefits and costs are calculated is arbitrary in the extreme. For projects which are close to completion, components of benefit as well as costs will, other things being equal, both be higher than for those only recently begun because benefits of application have to be discounted less while past costs are compounded over a longer interval. In order to minimise the distortions introduced, all project evaluations have been adjusted by discounting to the year in which the project was started. With a 5 per cent discount rate, multiplication by 1.05 to the power of −(1986 − T), where T is the starting year of the project, changes all values in a given project consistently and leaves X(1)/C and B/C ratios unaltered.

The Table sets out results for the 45 projects in terms of ranges and mean values of X(1), X(2), X(3) and C. It will be seen that the ranges of X(1) and C are substantial. Table 3 shows correlations between components of research output and C. It will be observed that those between X(1) and X(2) and X(3) are negative and non-significant with only those between X(1) and C and between X(3) and C being significant.

The values set out in Table 2 of Appendix 2 for weights to be assigned to X(2) and X(3) have been used to generate values of total benefit, B. Figures 3 and 4 show values calculated at 5 per cent to each project's start of X(1)/C and B/C respectively for all projects plotted against cost and ranked in order of decreasing value by these two ratios. The rank correlation coefficent of the two orderings is 0.55 ($p<0.01$).

Table 2. Summary of results of evaluations of 45 Forestry Commission research projects

£(83/84) million

	X(1) (wood)	X(2) (environment)	X(3) (science)	C
	Values calculated at 3%			
Range (min-max)	−0.05−47.23	−0.05−1.0	0.0−18.0	0.015−7.1
Total				38.835
Mean	5.246	0.070	1.135	0.863
	Values calculated at 5%			
Range (min-max)	−0.305−22.93	−0.05−1.0	0.0−20.3	0.015−7.95
Total				43.425
Mean	2.660	0.076	1.250	0.965
	Values calculated at 5% to start of each project			
Range (min-max)	−0.228−14.08	−0.02−0.8	0.0−10.3	0.012−3.0
Total				23.058
Mean	1.670	0.046	0.711	0.512

Table 3. Correlation matrix for values calculated at 5% to start

X(1)	1.000			
X(2)	−0.120	1.000		
X(3)	−0.066	−0.028	1.000	
C	0.354	−0.060	0.304	1.000

It is obviously important to appreciate how far the ordering of projects would be altered with different weights a and b. Figure 5 provides a visual presentation of these effects. At the calculated 'optimal' levels of a and b derived in Appendix 2, Table 2, there is a certain ordering of projects in terms of B/C. With a different a,b set, the ordering is altered and the closeness of this ordering can be assessed using Spearman's rank correlation coefficient. The values of this coefficient through a, b space are plotted as contours and show that the influence of relative change in b, the coefficient on X(3), is greater but that quite marked shifts, by a factor of 2 up or down, change the rank ordering only a small amount.

Conclusions from the results presented

In all the applications of this methodology, it is implicit that implementation reflects the policy or policies of the forest industry. The general point arising is that the estimation of benefits of a given application of research is properly determined by the agency, not the research project evaluator. A particularly sensitive point of valuation arises in forestry because of the overwhelming influence of discount rate. As an example one may consider improved plant material. In a perfect market not suffering from myopia, the fruit of such research would be valued by the price paid for the improved seed or cutting. In the absence of such a market, one is obliged to carry out a valuation of the discounted net benefit of the improved plant, and this calls for the nomination of a discount rate. In the numerical examples quoted in this paper a common discount rate is used in the calculation and in the further discounting involved in the research project itself. This is at least consistent for the case where the same agency conducts the research and applies its result. It is inappropriate otherwise and research benefits are thus underestimated where the discount rate of the user of research results is lower than those of the research agency and vice versa.

For 39 projects with non-zero X(1) type benefits (wood and cost gains) all but eight are expected to yield over 5 per cent. It is however exceedingly unlikely that the whole set yields more than 7 per cent in real terms and 10 per cent for the best where the same discount rate is used in calculating the results of applying research outputs. At the weights a and b used to calculate B (Figure 4), B/C ratios suggest an overall internal rate of 9 per cent. These results provide dramatic illustrations of the effect that long gestation periods have on internal rates. The figures are very different from the internal rates, commonly in the range of 30 to 70 per cent, computed for agricultural projects worldwide and reported by, for example, Evenson *et al.* (1979) and reflect the effect of long production cycles in forestry, as well as the misleading nature of the internal rate criterion.

One value of concentrating on evaluation of total benefits and costs of projects is to discover whether there is any systematic variation in B/C ratios across subjects, project sizes or other features. The results of an analysis across the subject areas distinguished by the Forestry Research Co-ordination Committee (FRCC) are set out in Table 4. There is no evidence of any marked imbalance in these results indicating the desirability of a shift of resources, apart from the figures in the first column which yield the familiar results for tree breeding projects, namely their sensitivity to discount rate.

The common managerial question is not whether a particular field of work is a profitable one to invest in but whether it is correct to continue to expend money on the particular project. It is clear that if there are difficulties in estimating B and C for a whole project, the hazards of attempting to assess the value of incremental B with incremental C from the present position are far greater. A critical, but hardly operational, approach to the problem is given by Roberts and Weitzman (1981). It is suggested that if this is the purpose of the evaluation, a useful starting point is to carry out the assessment of B and C values to the current position in the project as a first step.

Application of the methodology brings to light a number of administrative and managerial matters. One critical issue is the definition of boundaries between projects. There is a risk of double-counting of benefits, in part or whole, in two separate projects conducted by different agencies. This requires a discipline on the side of the evaluator such that costs are properly attributed to the particular set of outputs identified.

The process of applying a research finding may have another important impact. An example is that of work on the identification of habitat requirements of the goshawk. Before the research it is supposed that no special action is taken to encourage the goshawk. Research may show that provision of nesting sites implies a loss of revenue through retention of stands beyond maturity judged by wood revenue considerations. Thus research benefits

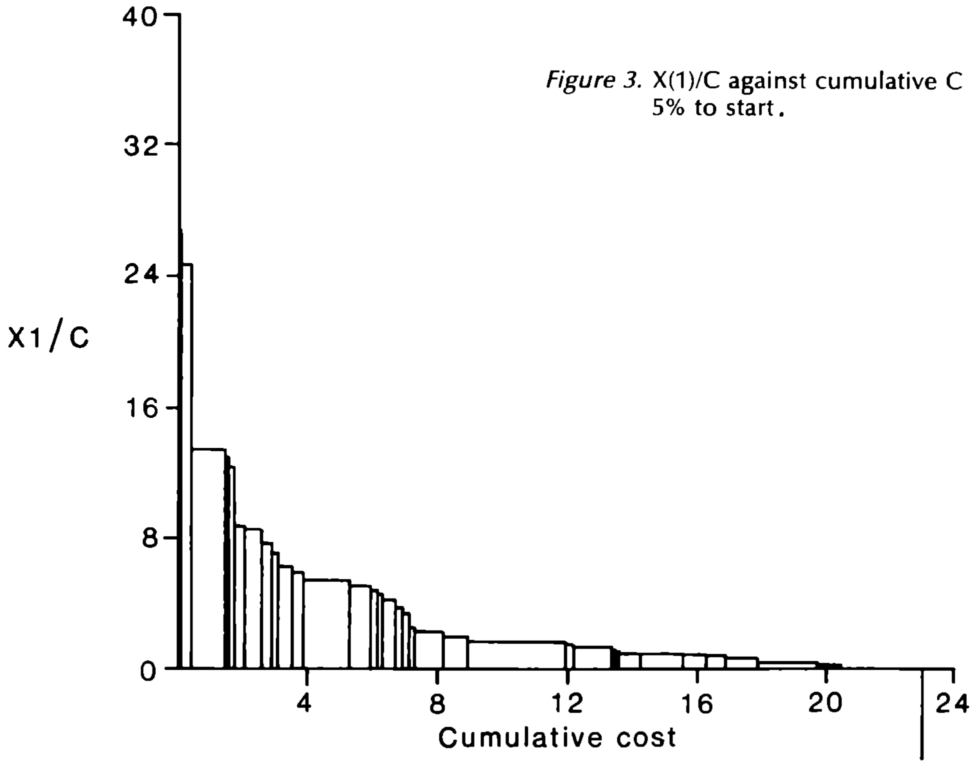


Figure 3. X(1)/C against cumulative C
5% to start.

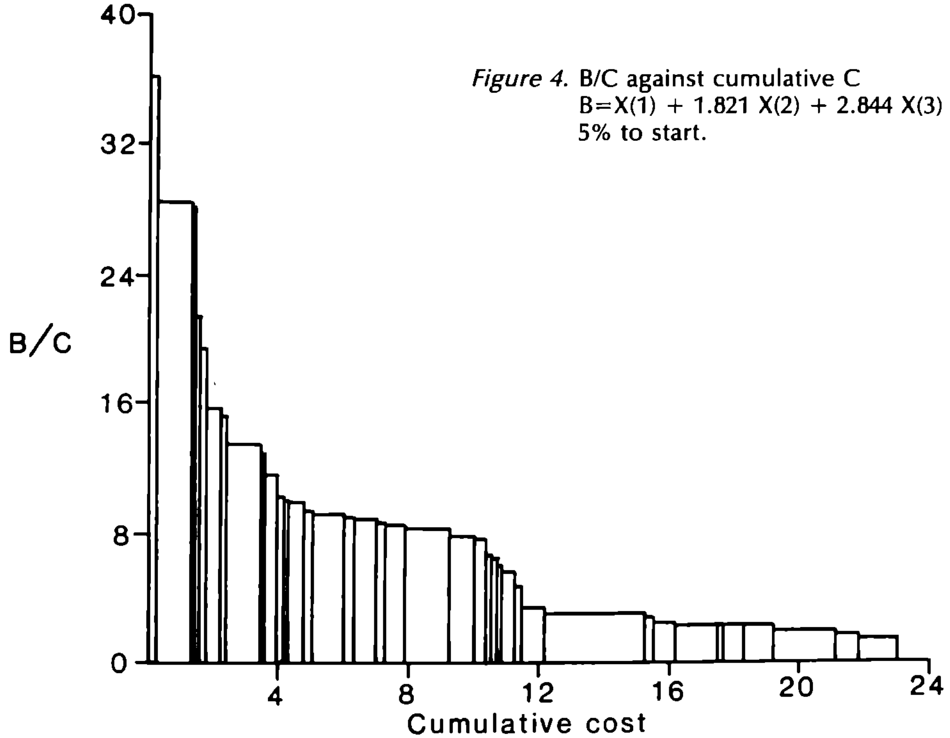
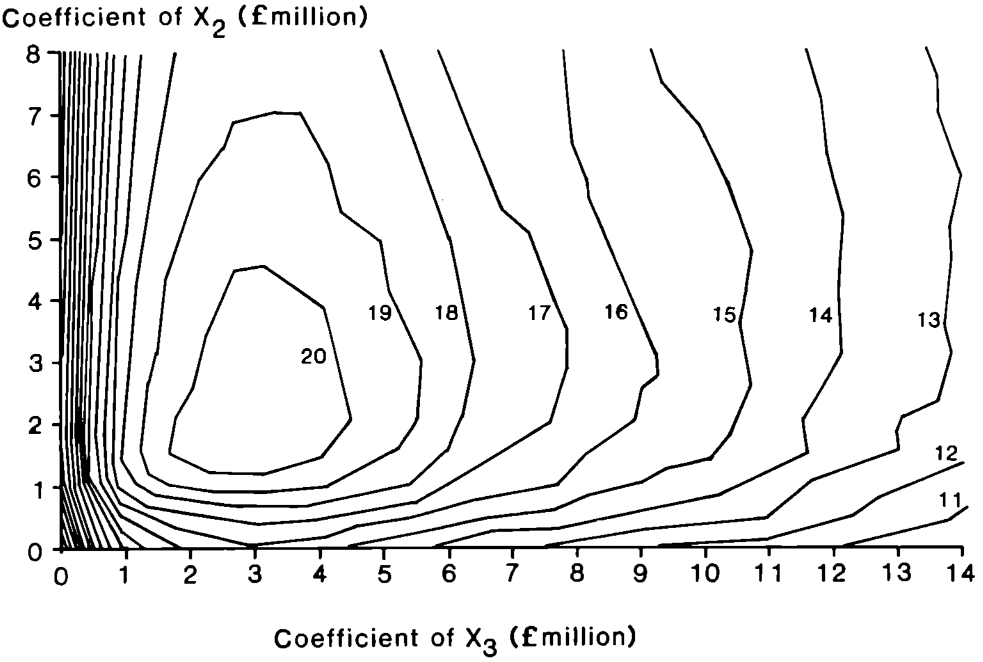


Figure 4. B/C against cumulative C
B=X(1) + 1.821 X(2) + 2.844 X(3)
5% to start.

Figure 5. Effect of changes in weights a and b on ordering of projects as measured by Spearman's rank correlation coefficient.



Contour Key:	
Index	Value
20	0.975
19	0.950
18	0.925
17	0.900
16	0.875
15	0.850
14	0.825
13	0.800
12	0.775
11	0.750
10	0.725
9	0.700
8	0.675
7	0.650
6	0.625
5	0.600
4	0.575
3	0.550
2	0.525
1	0.500

Table 4. Total X(1)/C and B/C values for 45 Forestry Commission projects according to subject areas

Subject area[1]	1	2	3	4	5	6	7	8	9	Total/ mean
Number of projects	3	8	15	15	0	0	1	2	1	45
Cost to start[2], £m	4.340	2.819	6.749	6.992			0.388	0.380	1.390	23.058
Total X1	6.797	6.797	28.279	20.953			2.430	2.372	7.540	75.168
Total B[3]	12.969	23.076	36.962	78.875			2.970	3.581	11.493	169.926
Mean X1/C	1.50	3.89	6.71	6.59			6.26	0.98	5.42	3.26
Mean B/C	2.92	11.65	8.00	13.31			7.66	10.75	8.27	7.37

Notes: (1). The subject areas are as follows:

1 Genetics and tree improvement
2 Tree biology
3 Silviculture
4 Biotic damage
5 Distribution, composition and properties of dry matter
6 Harvesting techniques
7 Wood science and processing
8 Environmental interactions
9 Forest planning

(2). Cost, X(1) and B calculated at 5% to project start.

(3). B = X(1) + 1.821 X(2) + 2.844 X(3).

would include a minus for X(1) and a plus for X(2) up to some limit. This limit is properly determined by the forest manager indicating the trade-off between X(1) and X(2) at the margin. In other words if this process were gone through critically one could transfer X(2) gains to the category of values determined by C/B-A which may be inserted into X(1). This process has in practice not been carried this far in the appraisal of this type of project.

There are also several technical issues that deserve attention. The first concerns the estimation of the probability of success. Properly, a probability density function is required in order to provide the basis for a general, probabilistic statement. It has to be remembered that various other steps in the evaluation of X(1) require distributions, rather than single values, to be estimated. It is of course perfectly feasible to import these richer assumptions and to carry through the requisite simulations on the lines pioneered by Hertz (1964). Whether the results justify the effort in place of the simpler calculation of a single certainty equivalent is not known.

The second feature is that of rate of uptake and the length of time over which the research result continues to find application. Useful warnings in the need for care in assuming the pattern of uptake of research findings over time are given by Stone (1984) and Wise (1986). Wise points out that many assessments of rates of return on specific research projects have produced exaggeratedly high values owing to the adoption of unrealistic assumptions of the time path of uptake. The whole subject of innovation rates and longevity of project results deserves more attention as does the related subject of the extent to which uncertainty is reduced with increasing expenditure.

Several projects will be found to be concerned with improving the precision of a particular physical parameter. The value of more exact information will depend heavily on the management system's ability to respond usefully to increased or more precisely identified knowledge. In soil science, for example, Burrough (1983) points out that while soil variation has often been considered to be composed of 'functional' or 'systematic' variation that can be explained and random variation (noise) that is unresolved, this is not a meaningful distinction since it is entirely scale dependent: increasing the scale of observation almost always reveals structure in the noise.

In general the calculated values of costs and components of total benefit will all be subject to error and sequential valuation will give varying results. There is no avoiding this. As with all forecasting work, experience supported by analysis of past successes and errors is the only route to improving performance. The method of approach recommended here can never be perfect or exact, it remains a guide to what may be expected, and because each element is explicitly described and evaluated it cannot fail to provide better guidance than where elements in an assessment remain unidentified or obscure. This is true even where the principal reason for undertaking a project is political, rather than to provide the outputs considered here.

VII. Future developments and conclusions

The literature on R&D management is replete with worked examples of investment appraisal of research and more or less sophisticated models erected upon them (e.g. Roberts and Weitzman, 1981). The present paper attempts a fuller discussion of the more general approach of cost-benefit analysis to problems of project appraisal in research programmes serving the essentially applied field of forestry. Before considering the further implementation of the approach and the research requirements that inevitably arise, it is relevant to consider two recent papers.

The first paper noted centres discussion on the 'short cut' implied by identifying promising research fields instead of looking at programmes project by project and evaluating these. As a result of their review of different countries' methods of identifying emerging areas of strategic research that promised long term economic benefits, Irvine and Martin (1984) concluded that a continuing effort in the field of 'macro-level' strategic forecasting was desirable in order to guide government allocations of funds to in-house or commissioned work in promising areas. The whole of science must be covered in order to ensure that possible cross-field linkages may be recognized. Curiously their study, perhaps because of the body who commissioned their work, the Advisory Council for Applied Research and Development, paid no attention to agriculture where government involvement in strategic research has been as high as any in civil industrial research. The emphasis instead was on manufacturing industries where commercial companies' own efforts in many sectors have been considerable, though secret. But the level of detail to which proposals for increased emphasis on research may be attached is low, with, for example, 'core' or 'generic' technologies obvious candidates, or sometimes product groups. The Science Policy Research Unit at the University of Sussex has, in this and other studies, concentrated attention on basic research rather than applied research and development. It is therefore not surprising to find Irvine and Martin advocating the use of strategic forecasting by the Advisory Board for the Research Councils as a guide to their policy making 'while still preserving the requirement that detailed funding decisions of individual programmes and projects be made primarily according to intrinsic scientific merit' (*op.cit.,* pp. 153–154). As noted above in discussion of scientific value, the Sussex workers have been active in developing critical assessment of the performance of researchers, whether by individual, team or institute.

Another relevant recent publication in the management of publicly-funded research is that by Thornley and Doyle (1984). This is principally concerned with management matters but includes a discussion on the applicability of C/B-A to research. It is argued that while this is negligible in basic research, it is also low in applied research and because these two areas are important in the work of the Agriculture and Food Research Council it is concluded that one is unable to derive a measure of gains per unit of expenditure in research. The conclusion that because of this it is impossible to estimate the optimal level of research expenditure in agriculture follows logically but the writers take a conservative view of the practicability of improving our ability to carry out C/B-A in applied research projects. The writers' proposal for a concentration of effort on assessing promise on the basis of past performance, requiring evaluation of results of past work, is however strongly echoed here.

The ultimate value of the various approaches to research evaluation such as that set out here, or the techniques favoured by Irvine and Martin (1985) for curiosity-oriented and strategic research, is to encourage an improved flow of research results, better decisions on progress with current projects, as well as improved design of future projects. There is no reason why this should not be a feasible target while still maintaining an environment which encourages the creativity referred to earlier. Indeed the carrying into effect of the assessments reported here represents the logical extension to research of management precepts which have long been established in other fields of human endeavour.

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In order to avoid the disadvantages implied by the procedure of Table 1, an alternative approach is to derive values of a and b which at some mean value of X(1)/C minimise the coefficient of variation of B/C. This formulation avoids the difficulty associated with a regression approach which seeks to minimise absolute, as opposed to proportional, deviations from the mean. Table 3 shows the results of applying this approach.

The values shown against '5% to start' in Table 2 have been adopted in calculating the results shown in the text of Section VI.

Table 2. Weights calculated as set out in Table 1

Project outputs discounted at	Mean X(1)/C[1] Actual	Mean X(1)/C[1] Value at which stated weights occur[3]	Weight[2] on X(2) a	Weight[2] on X(3) b
3%	17.61	12.94	2.212	2.213
5%	10.50	10.08	1.764	2.806
5% to start[4]	10.50	10.12	1.821	2.844

Notes: (1) for the 10 projects with zero values of X(2) and X(3).
(2) a is scaled in £m while b is a pure number applied to X(3) which is itself measured in £m.
(3) the convergence value referred to on page 25.
(4) whereas the calculations identified as '3%' and '5%' refer to sums calculated at these discount rates to 1986, the last row refers to results found where sums are computed, using information available in 1986, to the actual initial year of each project. This removes the arbitrariness of a given calendar year which may be a few or many years after project initiation.

Table 3. Weights calculated so as to minimise the coefficient of variation of B/C

Project outputs discounted at	Mean X(1)/C[1]	Weights on X(2) a	Weights on X(3) b
3%	10.07	3.768	5.193
5%	5.64	3.168	3.961
5% to start	5.52	3.125	3.828

Note: (1) Mean X(1)/C values for all 45 projects in contrast to the (higher) values shown in Table 2 which relate to projects with zero environmental or scientific value scores.

Appendix 3

Illustrative costs: Forestry Commission Research Division

Table 1. Summary of 1987/88 basic budget for Research Division, Forestry Commission

					£ million
Staff related	non-industrial (NI) staff salaries (all)	2.453			
	NI employers' national insurance, pension and gratuity provision	.711			
	NI transfer expenses, excess rent allowance	.136			
			3.300		
	industrial staff wages, including loaned labour, contract wages	.970			
	industrial insurance, pension provision	.206			
	industrial protective clothing, sick benefit	.027			
			1.203	4.503	
Transport, travel and subsistence	non-industrials' travel and subsistence	.206			
	industrials' travel and subsistence	.040			
	80% of machine costs	.270			
	travel and subsistence of committees	.040			
			.556	.556	5.059
Office expenses	rent, maintenance, rates	.584			
	energy	.125			
	telecommunications and post	.104			
			.813		.813
Materials and supplies	general and miscellaneous	.748			
	non-transport machines and other equipment	.207			
	publications, computing	.270			
			1.225		1.225
Commissioned research	universities, government institutes	.450			
			.450		.450
Total					7.547

If grants for commissioned research contain a broadly similar composition of costs to in-house expenditure the totals become in £ million:

	In house	Grants	Total	Percentage
Staff related	4.503	.285	4.788	63.4
Transport, travel and subsistence.............	.556	.035	.591	7.8
Office expenses.............	.813	.052	.865	11.5
Materials.......................	1.225	.078	1.303	17.3
	7.097	.450	7.547	100.0

Table 2. 1983–84 expenditure, including costs of services, in total and per non-industrial staff member in Entomology Branch

Project group	£000	Man-years	£000 per man-year	Index (Branch mean = 100)
Dendroctonus	76.6	3.2	23.9	90
Panolis	60.2	2.2	27.4	103
Ecology	42.2	1.5	28.1	105
Host	47.5	1.7	27.9	105
Hylobius	30.2	1.3	23.2	87
Minor	48.2	1.6	30.1	113
Taxonomy	25.0	1.0	25.0	94
Help	43.7	1.5	29.1	109
	373.6	14.0	26.7	

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