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# PRIORITY SETTING IN AGRICULTURAL RESEARCH: BEYOND ECONOMIC SURPLUS METHODS

PETER SMITH*

*Centre for Agricultural Strategy, Reading University, UK*

## SUMMARY

The article examines the issue of priority setting in Third World agricultural research organizations, for those research sectors and topics which—mainly because of the difficulty of quantifying benefits—are outside the domain of economic surplus and benefit/cost techniques. The issue is redefined as one of processing ill-structured decisions, i.e., decisions in which there is no unique, identifiable, objectively optimal solution. The implications of this for the concept of rational action in this field are explored, and the results used to define a practical approach. This centres on a radical review of the possible options and criteria in priority setting, using four techniques (challenge groups, repertory grid, creativity techniques and flow charting). The final section of the article examines the decision-making aspects of these techniques in three priority-setting contexts of increasing difficulty: within a department, where the issue is essentially one of technical choice between competing research lines; within a major institute or research sector, where there is substantial competition for resources; and in an institute or sector which is facing a crisis of resources or relevance. Copyright © 2001 John Wiley & Sons, Ltd.

## INTRODUCTION

Given both the scale of agricultural research in and for the Third World[1] and increasing pressure on research funds, the priority setting issue is an important one. During the 1980s and the 1990s, economic surplus methods (ESM) were developed for priority setting; these were undoubtedly an improvement on the earlier attempts to incorporate economic benefits into priority setting, at least in terms of the quality of the micro-economics involved.[2] Alston *et al.* (1998) review ESM in depth; these methods have been strongly advocated by World Bank staff (Byerlee and Alex, 1998).

Alston *et al.* acknowledge that ESM are best suited to choices among research proposals directed at 'embodied technology'—improved germplasm, and innovations in inputs, hardware and husbandry practices—and are most relevant to sectors producing traded commodities. They are also primarily intended for allocating funds between major research sectors. In general, ESM are not suitable where costs and benefits cannot be accurately quantified. This includes: policy research; socio-economic research; problem exploration and definition; and applied/adaptive research where there is no recent history of successful technology generation from which impact estimates may be derived.

This article focuses on these excluded areas. Their importance raises three questions: can they be brought within the scope of ESM; can other existing approaches work in these areas; and—if both answers are 'no'—what is the alternative?

*Correspondence to: Dr P. Smith, Springcott, Chittlehamholt, Devon EX37 9PD, UK.

[1]In the late 1990s, the budget of the CGIAR international research centres was US $340 million p.a., and for the Indian national agricultural research service, almost twice that.

[2]However, the method uses the simpler, uncompensated version of the consumer surplus element of total economic surplus; this version has been extensively criticized; see Cullis and Jones (1998, p. 24).

## THE LIMITATIONS OF ESM

The limitations are: the problem of qualitative and numerically vague data; the nature of decision-making in research priority setting; and the model of institutions implicit in ESM. In each case, the limitations are fundamental ones; the answer to the first question is 'no'.

### *The problem of qualitative and numerically vague data*

In the excluded areas, research outputs have no direct monetary value, and priority setting depends on assessing qualitative assertions about proposals, such as

> *the justification of the work is the impact on crop yields in the Indo-Gangetic plains of an improved understanding of the factors limiting the uptake of high yield varieties and modern technology.*

In such a situation, the data ESM requires is simply not available. This is not an unusual situation; qualitative variables are part of reality. Some are inherently qualitative, such as the heuristic promise of a particular line of investigation, or the morale of a research institution. Others (ostensibly numeric) can only be placed in a broad band within which a point estimate cannot be meaningfully singled out; this may be true, for example, of variables with fat-tailed probability distributions, or generated by processes which are markedly non-stationary, or give rise to long, complex lag structures (see Feder, 1988, p. 149 ff.). Variables—such as the scientific merit of a proposal—may also be ambiguous (but not necessarily meaningless). Such variables are referred to as *fuzzy*, in contrast to point estimates on interval scales, ordinal rankings and assignments to precisely defined sets, which are all *crisp* (Ross, 1998).

### *The nature of decision-making in research priority setting*

This has two aspects: problem structure and information flow.

#### ***Problem structure***

ESM implicitly assume that the priority-setting problem is well structured, i.e., belongs to the class of problems whose basic nature is not in dispute, and for which valid, generally accepted methods of solution exist (Mitroff, 1974). However, priority setting in the excluded areas is almost always ill structured, sharing none of these characteristics.

Ill-structure arises from two sets of considerations: first, whether the problem at hand suffers from (mere) risk, or from uncertainty. Risk implies a fixed list of objectives and options; and that the consequences of the latter (and their probabilities) are known. In this case, hazard arises solely from our ignorance of which consequence will result from our choice; given reasonably stable estimates of costs and benefits, the available options can be ranked, discounting for risk by using probability-weighted costs and benefits (Cooke and Slack, 1984, p. 195 ff.). Probability weighting (common in ESM in the broader sense; see for example, Janssen and Kissi, 1997) measures the merit of a research proposal as the product of its net benefit (if successful) with its subjectively estimated probability of success. This practice assumes such estimates are likely to cluster around the 'true' value, in a reasonably 'neat' way (e.g., as though normally distributed); however, a large body of evidence suggests that such estimates are unreliable, unless the estimator has first-hand experience of a large sample from the relevant population (Wright and Ayton, 1987). This will not be the case for a scientist estimating the likelihood of success of a novel line of research.

By contrast, under uncertainty, our objectives may not be fully known in advance (our vision of the possibilities of the material and processes may be evolving during the work); our list of options is not fixed (because of results from contemporaneous work and the possibility of creativity); and/or we do not know all the consequences of those options, because of our ignorance (e.g., in the area of climate change) or because of emergence. The latter phenomenon—of even moderately complex systems producing behaviour that cannot be predicted from a knowledge of the science of their components—typically makes forecasting the outcome of choices in socio-technical systems impossible; Holland (1998) presents an interesting analysis of mechanisms of emergence. Furthermore, different observers may be unable to agree on the ranking of the options, because of extreme vagueness in the estimates of benefits, costs and probabilities. In this case, a large element of hazard arises from the unanticipated consequences of choice.

A second set of considerations comes into play when the different groups of stakeholders apply different values or paradigms in evaluating outcomes, so that even the criteria of what is a good solution to the research resource allocation problem are in dispute; such situations are distinguished as *wickedly structured*.

In the narrow technical sense,[3] a paradigm is an image of the fundamental nature of the entities and causal links in a field of investigation, which determines what are significant questions, what is valid methodology and what counts as a serious anomaly. Paradigms direct the main thrusts of disciplines, but also tend to disqualify counter-evidence against the accepted foundations of their particular discipline or school (Kuhn, 1970). In a review of the subsequent development of the concept (chiefly by Lakatos, 1970; and by Laudan, 1977), the present author concluded that a continuous range of entities exists, from opaque Kuhnian paradigms, through professional imageries (accepted assumptions within a particular professional group, not as deeply embedded as the former, and relatively easily challenged), to systems of axioms and formal models (Smith, 1994). Paradigms are also what give sets of axioms and formal models their appearance of reasonableness and necessary truth; wherever parties with different paradigms are forced to collaborate, it is most unlikely they will agree about problem diagnosis, what options are realistic and feasible, and what criteria should be applied (Biggs and Farrington, 1991; and Smith, 1994, give examples of such disagreements). Consequently, inter-paradigm differences are a very significant complication both to devising priority-setting methodology and in operating it in groups of scientists from differing disciplines; often, this leads to managers and scientists being faced with multiple, conflicting sets of criteria—a decision constrained only by a single criterion or a set of consistent criteria is the exception rather than the rule.

Whether it arises from divergent paradigms, different values or uncertainty, ill structure and wicked structure have a profound effect on our concept of rationality. Rational action in well-structured situations consists of finding the optimal choice and executing it; in ill-structured situations, there is no identifiable optimal choice (which will be accepted as such by all), and here rationality consists of managing optimally that which we have chosen reflectively. This in turn depends on the quality of our search through the space of alternative diagnoses, options and criteria, and the quality of our decision processes (see below). These two paradigms of rationality contain different images of human nature: the first, that we are constrained optimizers of known effects; the second, that we are—to a significant degree—explorers of the unknown. Readers will draw their own conclusions as to which of these possibilities better explains the emergence of a technologically advanced society.

There are four possible combinations of actual and assumed problem structure:

| | Situation is well structured | Situation is ill structured |
|---|---|---|
| Approach assumes problems are well structured | The classic decision situation: finding the optimal solution | Potentially disastrous, because of the distortion needed to fit the problem to the method |
| Approach assumes problems are ill structured | This combination does not usually arise | Highest likelihood of finding a good solution in a difficult situation |

An example of the kind of distortion referred to in the top right quadrant of the matrix is provided by the tendency of ESM to focus on technology generation and near-to-market research output, at the expense of strategic and curiosity-driven research.[4] The Martin and Irvine (1989) study suggests that this carries a real danger of allowing the pipeline of latent technology to run dry, by focusing on those areas which are amenable to one particular priority-setting methodology.

[3]The interpretation used here is the second of Kuhn's (1970, p. 85) two main senses.
[4]The terminology used here is that of Martin and Irvine (1989): mission research is directed at the production of applications from known properties and effects; strategic research is directed at exploring fields from which future applications (or other benefits) are expected, but which cannot yet be identified. Curiosity-driven means what it says.

 

### *Information flow in research decision processes*

Decisions typically involve a number of phases: recognition; finding an heuristically potent diagnosis; establishing the criteria that should apply; generating options; and selection among them. ESMs have focused exclusively on the last of these, taking the options as given, and diagnosis and the issue of which criteria should be applied as a well-structured problem, with an evident and known answer.

Any attempt to set up an alternative to existing methods of priority setting has to address the issue of decision-making in ill- and wickedly structured situations; similarly, it has to address issues of diagnosis, generation of options and setting of criteria.

### *The implicit model of institutions which underpins ESM*

ESM relies on the neoclassical model of institutions and motivation, centred on the rational actor model of organizations; according to this, organizations are single-minded entities, each carrying out programmes set by its managers. These managers determine objectives and the most efficient means of achieving them; in doing this, they loyally implement the organization's charter, which has legitimacy because it embodies the desires of the organization's owners or sponsors. Management has sufficient power (from formal authority and control of resources) to ensure that its instructions will be carried out.

This model has appeared under a number of guises (see Morgan, 1997). However, it is certainly not applicable to organizations with complex, ill-structured taskloads (Mintzberg, 1983). In reality, the charter may be extremely vague; the owners/sponsors may have abdicated responsibility; and their requirements may be contradictory or impossible to fulfil (for example, the 'mission statements' of research institutions often commit them to objectives that cannot be attained by the generation of knowledge). There may be serious conflicts regarding strategy or overall direction amongst powerful actors, both within the institutions, and in what Mintzberg calls the 'external coalition'—its sources of funds, regulators, activist groups, etc. Further, management usually does not have a monopoly of power: expertise, skills in organizational politics and 'negative power' (Handy, 1985, p. 118 ff.) all act to distribute *de facto* control of the action far and wide through most organizations.

Defining priority setting as the process of generating a single, ordered list of items for management to implement only makes sense if one does believe in organizations with total top-down control, and a well-structured taskload. Such a belief seems to underlie Byerlee and Alex's (1998) prescription for ESM, backed by controls and sanctions.

Because of the dispersal of power in organizations, priority setting has to influence choice at many levels, from choices among specific investigations within departments, to macro-level allocations between major research sectors. If the key actors have faith in the methodology, as a means of producing good research agendas—which are powerful personal and professional assets—they will use and maintain it; otherwise, they will bypass or neutralize it, and the management annals are full of instances of such action (see, for example, Mintzberg, 1983, p. 171 ff.).

At the same time, priority-setting methods should (but typically do not) recognize and address the political issues that will inevitably arise when decisions involve major reallocations of funds and resources, precisely because of the opportunities for subverting, sabotaging or walling-off of unpopular decisions, which can make priority setting irrelevant to what actually happens. Priority setting can only lead to a complete, 'rational' allocation of the research budget in the exceptionally unusual case of no organizational politics, in which case zero-based budgeting is feasible (for an interesting analysis of a major failure of ZBB; see Wildavsky, 1975).

## OTHER CURRENT METHODOLOGIES

Arnon (1975, 1989) reviews the existing range:

1. *Congruence* produces benchmarks for spending in the various research sectors by dividing the overall budget in the same proportions as the contributions of the sectors to agricultural GDP (presumably to optimize allocation by equalizing marginal returns across sectors). However, research in some sectors may be inherently more costly; in others, it may be stalled, waiting for a potent reformulation of intractable problems; and

 

some sectors are not separately represented in national accounts. Surprisingly, this approach retains some advocates.

2. *Weighted scoring methods* are popular for prioritizing individual research initiatives. They normally combine scores on compliance with policy, prospective economic impact, scientific merit and technical feasibility (Arnon, 1989; Norton and Pardey, 1987). They can function without the wealth of high-quality data required by ESM, and are applicable to research choice at all levels; they can also handle important non-quantitative issues. However, the weights used are often highly subjective, and the method ignores the important effect of differences between paradigms and/or professional imageries.
3. *Delphi* aims—through successive rounds of anonymous comments by experts on earlier proposals, ratings and definitions—to produce a consensus, by eliminating the excessive influence of senior or aggressive individuals that may occur in face-to-face discussions. It appears to have worked well for exploring the ramifications of complex decisions (Linstone and Turoff, 1975), but has made little impact on priority setting, because it too ignores the effect of paradigms and professional imagery.

## AN ALTERNATIVE: PRIORITY SETTING AS AN ILL-STRUCTURED PROBLEM

Any alternative approach has to provide for the exploration of options and criteria, and for decision-making in ill-structured (and wickedly structured) situations.

### *Exploration*

The effectiveness of four techniques—challenge groups, repertory grid, creativity-supporting methods and flow charting—is considered below.

*Challenge groups*, which grew from work by Mason and Mitroff (1985) and Churchman (1971, p. 181 ff.), are effective where a set of professional imagery (PI) is limiting the range of possible diagnoses and options being considered. Challenge groups are small, professionally homogeneous syndicates, selected from a larger group which represents a wide spectrum of interests in the arena; each syndicate is asked to identify the current PI and at least one potent alternative; these findings are pooled for plenary review.

A challenge group exercise for a national forestry research institute (Smith, unpublished consultancy report for ODA, Manchester University, 1993) identified the efficient production of large timber suitable for industrial use as a dominant element of the PI. This had heavily influenced the research programme, orienting it away from the reality of an industry with an important non-timber component (browse, firewood, edible fungi and honey), and most of whose timber went to small-scale carpentry and craft uses, where small logs, with a variety of colour, hardness and curvature, were in demand.

*Repertory grid* uses a card-sorting technique, in which actual examples of research initiatives are sorted by scientists and research managers, to reveal the dimensions they use in classifying research proposals (Stewart and Stewart, 1981). Because classifications and categories are intimately connected with paradigms, this exercise provides a powerful way into the latter (Spradley, 1979; Bannister and Fransella, 1971). An exercise of this kind was conducted with the staff of a group of research institutes in north-west India (Smith, 1996). It revealed profound but unsuspected differences within a group of senior scientists who normally worked together on issues of policy and strategy; these differences divided the group into two subsets, A and B (described in Table 1), between which there was little effective communication.

Table 1. Characteristics of hidden subsets of research policy-makers

| Subset A prefers | Subset B prefers |
|---|---|
| Development of technology which is known to be feasible | Speculative, strategic research |
| Studies with high probability of ‘success’ | High-risk, high-promise initiatives |
| Well-established areas of research | Work unique to the institute/researcher |
| Front end (germplasm, reproduction and disease control) research | Back end (management, marketing and processing) research |

 

*Creativity techniques* generally aim to disrupt preconceptions about the form of solutions to particular problems (Adams, 1979). An example application concerns a successful marine mussel fishery, which was established by one of the Indian Council of Agricultural Research institutes (Smith, 1996). Mussels are reared on ropes, attached to anchored wooden rafts. The problem was to identify alternative technologies—worth testing—for raising a second crop of mussels each year during the storm season, when production was impossible because of the high costs of wave protection for the rafts, the difficulty of avoiding wave action by using submerged rafts (because of damage by wood-boring worms) and the cost of permanent moorings for them. The use of a standard creativity tool (synectics) enabled the institute's scientists to produce suitable options, including: plastic or glass-fibre for deeply submerged rafts; cheap, dead-weight permanent moorings (using recycled materials); and simple shock absorbers in the mooring chains.

*Flow charting*: constructing a flow chart of a key element or substance in a process is often a useful tool for identifying research entry points, and for identifying the need for inputs from a wider range of disciplines. Reviewing carefully the production and marketing chain of the mussel fishery referred to above in this way identified a number of potentially productive investigations, including: marketing studies of the possibility of wholesale buyers taking uncleaned shellfish (small producers found the flushing of grit, etc., from the catch expensive and difficult); and investigation of factors affecting the bacteriological quality, consumer acceptability and shelf-life of frozen mussel meat.

All these techniques contribute to the exploration phase, identified earlier as a keystone of rationality in ill- and wickedly structured situations. In each case, the work was carried out by those most closely involved, with guidance on the process only; this has positive benefits on the ownership of the revised diagnosis, solutions, etc.

Each of the techniques produced a sizeable change in some fundamental aspect of the definition of the research arena, the perceived constraints on solutions or the inputs required (by discipline). The repertory grid exercise exposed a major divergence on ideas on what constitutes 'good research' among important actors; such exposure may be a necessary preliminary to synthesis or the negotiation of a *modus vivendi*, but the differences can be so entrenched as to forbid either, in which case persistent conflict and paralysis are to be expected (Mintzberg, 1983, p. 420 ff.).

In some cases, the changed definition reflected a realization of the need to carry out a thorough literature search before committing time and physical resources. Very often, such search is limited by the inadequate (or non-existent) reference materials available to working scientists; it is tempting to suggest that information technology (IT) and the Internet have a role to play here, but the same factors that lead to poor conventional library facilities are likely to affect IT provision and maintenance. Also, the issue is not primarily one of information: the output of any information search is crucially dependent upon the way the problem has been framed (i.e., during the exploration phase referred to above).

### *Decision in ill-structured situations*

Three distinct cases are considered, with the methods appropriate to each.

#### *Case A. Ill-structured choice, at the level of individual research lines*

Decisions at this level—whose quality is fundamental to the performance of the whole research system—are mainly made within small, homogeneous groups of scientists, such as departments or programmes within a larger institute. Scoring might be an attractive option at this level, but a number of improvements are possible:

- Criteria and options should be generated by an appropriate process of exploration.
- The issue of correlations among the various scores (on different criteria) given to individual proposals by different judges should be addressed, e.g., by factor analysis or principal components analysis (Jackson, 1991).
- More use should be made of complex scoring systems, in which interactions among the criteria are set up to reflect policy preferences, so that, for example, a given level of (prospective) economic benefits attracts a higher priority if these benefits are directed towards a favoured group or sector.

 

- Attention needs to be given to the estimation of weights. One possibility (which is widely applied in other decisional situations; see Terano *et al.*, 1994) is to use fuzzy logic; this is a major topic, with implications for other aspects of priority setting.

*Fuzzy logic*. The proposition:

IF < economic benefits are moderate > AND < distribution of benefits is good > THEN < priority is high >

is a fuzzy proposition, the terms *moderate*, *good*, and *high* each being a fuzzy value of a (qualitative) variable. Fuzzy logic (FL) deals with the problem of vagueness or qualitativeness of the variables by using membership values, rather than truth values. Truth values indicate whether a particular case belongs to a well-defined set (such as 'research proposals with an estimated EIRR of 13.5% or more'), whereas membership values indicate how good a member a particular case is of a vague set (such as 'proposals with high expected economic impact'). Truth values can take the values of 0 or 1, only; membership values can take any value from 0.0 to 1.0 inclusive. FL is a robust calculus, enabling rigorous reasoning with valid, fuzzy propositions and data. Normally, it is applied as a fuzzy system, a set of rules similar to the above, which cover all the combinations of levels of the variables which are of interest.

The minimum requirement for fuzzy logic to be effective is a valid qualitative model of the situation, shared among members of a professional community in such a way that there is broad agreement about the meaning of fuzzy variables and values; without that, we are almost certainly in the situation described under Case C, below.

Fuzzy logic uses a consistent subset of the mathematical rules of classical/Boolean logic (McNeill and Freiburger, 1993). Membership functions can be derived from observing the language and practice of expert judges; in certain cases, by optimization methods; or—most important in this context—from historical data (Ross, 1998). The latter possibility, which reflects the success of past policies and decisions, is an attractive one, in that it eliminates the need for subjective estimation of the weights. Where historical data does not exist (either because it was not collected, because policy preferences have changed or because the current set of choices is of a radically different kind), this cannot be done, of course, and the fuzzy system would necessarily consist of policy judgements.

*Case B. Research choice in ill-structured, aggregate situations*

At the level of competing departments or research sectors, priority setting generally implies reallocation of resources, sometimes job losses, and often choices between mutually incompatible views of the particular research arena. It can never be safely assumed that such decisions are purely technical: organizational politics will play a part, often a major one. There are two possible approaches to decision-making here:

*B(i) A bidding system.* This approach has shown promise in a field trial in an institute with a small number of major programmes, each relatively homogeneous (Smith, 1992). The process starts with an exploration of professional imagery, criteria and options within programmes, from which provisional (programme) priorities are developed. Following this, management's criteria of what is good research (in terms of the institute's goals), and its version of the preferred professional imagery are also explored using the same techniques. The leaders of the programmes are then asked to state what additional work they would offer if their budget were increased by some percentage, and what sacrifices they would make if it were correspondingly reduced. (This was actually done by scoring, but a fuzzy system could have advantages.) Inter-programme budget reallocations are made by management in the light of the bids, making the process reasonably transparent. The critical factor in the use of this method is management's willingness to reflect on—and reveal—its own perceptions of what is 'good research'.

*B(ii) Incrementalism.* Historically, two distinct approaches have been taken to this sort of choice. The first attempts to force the problem to fit the methods available for well-structured, single-criterion decisions. This move has frequently been associated with the idea of zero-based budgeting (ZBB).

The second approach claims that incrementalism is, in fact, more sophisticated and realistic than ZBB, and that the former's major fault—inertia, resulting in the unjustified persistence of historical patterns of spending—can be dealt with. Incrementalism recognizes the desirability of change, but also recognizes that large, bold moves may have consequences which are difficult to evaluate in advance—that, in fact, the issue is an ill-structured one (Keen, 1977; McGrew and Wilson, 1982). There is also an argument that deciding which research sectors to promote, and which to discard, is analogous to the *K-Armed Bandit* (KAB) problem. Simplistic strategies of putting one's stake where the pay-off rate appears to be greatest perform poorly against a KAB (a gambling machine with *k* levers, each with a different, unknown pay-off rate and variance); it is advisable to move the share of the total stake incrementally, according to the evolving information on pay-off rates (Goldberg, 1989, p. 36 ff.).

While there is clearly justification for incrementalism, the danger of inertia is a real one, unless there is an agreed set of criteria governing changes in allocations (again, possibly in the form of a fuzzy system).

*Case C. Research choice in wickedly structured situations*

The classic case of a wickedly structured research decision arises where part of the research system is facing a crisis of resources or relevance, and the key actors are divided by paradigmatic or value differences. In this situation, the decision is a *consequential* one (Janis and Mann, 1977): the outcomes of a poor decision are likely to affect the decision-taker personally and seriously, in his/her professional reputation and prospects, job security, etc. There are two reasons for this. First, a wickedly structured problem implies a high degree of uncertainty, and the chance of any proactive group achieving the goal that initially attracts it—without expending substantial efforts on managing the flow of unexpected problems that is likely to develop during implementation—is small. Second, because of disunity among the stakeholders as a whole, any group that takes action will find that it has created for itself a hostile opposition, ready to exploit any failures (if only because the action group will be trying to capture and divert existing flows of resources).

To get any productive change, one such activist group has to be fostered, and supported in arriving at a sound, committed decision. Here, 'committed' means that the relevant actors are genuinely prepared to put the proposal into action, which in turn implies that: they have shared in analysing the situation, and developing the options; they have explored as far as possible the difficulties known to be involved in executing the chosen option; they have a feel for the directions from which further—as yet unknown—threats and opportunities might come; and they are mentally prepared for the business of managing such threats and opportunities as they arise. Any attempt to manipulate the commitment-building process to come to some predetermined conclusion is likely to fail, since the persons with action responsibility may not be able to deliver what the outsiders would like to see installed.

One of the most useful approaches is the Decisional Balance Sheet technique (DBS; Janis and Mann, 1977). This requires exploratory work of the kind referred to above, the creative generation of options, and role-play to explore the reactions of other key actors to the decisions made; it places emphasis on evaluating both the material and non-material (e.g., feelings, changes of status) considerations that bear on the decision. This leads on to an analysis of the leading options in matrix form, in which the columns are the options and the rows represent the considerations that affect them. DBS is an established method of analysing and displaying simultaneously all the implications of a complex choice—a process which may reveal a need to return to diagnosis, reviewing criteria, or generating additional options. Supporting the efforts of a group to escape from their difficulty by applying this technique requires a variety of personal skills and sensitivities which have not, until now, formed part of the normal repertoire of consultants engaged in priority-setting work. However, this—or something very like it—forms a standard part of the mainstream management consultancy toolkit (Nutt, 1990, for example, talks of 'tough' rather than ill-structured and consequential decisions).

## SUMMARY AND CONCLUSIONS

The focus of this article has been those research topics and styles which lie outside the scope of ESM. However, the scope of ESM may be narrower than generally accepted, because excessive uncertainty concerning costs and impacts may convert apparently well-structured to ill-structured decisions, or (because of the failure of the institutional assumptions which underlie ESM) even to wickedly structured ones.

Economic considerations should, of course, play an important part in determination research priorities; but there are also other important variables. The induced innovation effect (Binswanger and Ruttan, 1978) indicates that scientists have been responsive to economic pressures without specific external controls or encouragement. Upton's (1991) criticisms of earlier high estimates of economic returns to agricultural research (on the grounds of poor sampling and methodology) appear to be justified, but even his revised results, showing that *achieved* returns are commonly in the 10–18% range, is a tribute to the ability of scientists to spot potential economic impact (some of which may, of course, be research-pushed, rather than demand-pulled).

On the central issue of priority setting in the excluded areas, the argument has been presented for treating priority setting as a problem in ill-structured decision making; the small amount of case study material available on applications of this approach appears promising. If the adoption of this alternative is regarded as a paradigm shift, then it is one of the second kind which Lakatos (1970) identified: one whose adoption on an experimental basis can be justified in terms of its ability to address hitherto intractable problems. It would be unwise to claim more at this stage, before more field experience has accumulated (but it should also be noted that a *post facto* analysis of the effectiveness of any research priority setting approach is a rare bird indeed).

A major, closely related issue—which it has not been possible to deal with in depth here—is that of 'institutionalization': how changes in structures and practices can be made in real organizations, with internal conflicts, resource constraints and established cultures. There is a growing recognition (e.g., in the contents of the web site of reference material currently being developed in the Investment Centre at FAO) that current practice in institutional analysis and the design of measures for change is inadequate, but that a considerable body of useful theory and technique is available in mainstream management science.

Finally, both the existing approaches and the proposed alternative scheme suffer from a difficulty which does not seem to have been recognized in the literature: research priority setting is a particular case of the knapsack problem. This involves packing items (research proposals) into a multidimensional 'container' (whose dimensions are defined by the different non-interchangeable components of the research budget, and the inventory of specialized, single-purpose research assets which are available, and which are difficult to convert to other purposes in the short term). Methods have typically relied on the assumption that all resources are interchangeable, so that the knapsack is a one-dimensional one, and the problem collapses into a simple sequential choice of the most promising proposals, in declining order of (economic) merit; this is highly unrealistic. This issue merits further attention.

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