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# **Analytic approaches for research priority-setting: issues, challenges and the way forward**

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## **Summary**

In the last two decades, growing demand for studies assessing health care interventions, coupled with the acknowledgment that limited public funds for research ought to be allocated efficiently, has led to increasing calls for the use of analytic approaches for research prioritisation. Two main approaches have been proposed—‘value of information’ and ‘prospective payback of research’—but none of them is used formally in the prioritisation process. This article discusses possible barriers to the formal adoption and use of analytic approaches. These include uncertainties around the appropriateness of using results of analytic approaches for priority-setting, questions around the validity of the results, and challenges in introducing and using analytic methods as part of the prioritisation process. It is argued that most of these challenges can be overcome, and ways of doing so are discussed. The author’s view on the direction and pace of future developments is provided, followed by recommendations for further research.

## **Introduction**

Health care research is seen as a key activity and a vital means of improving the health of the population. In recognition of this, public resources are made available to support a wide spectrum of research programmes, ranging from basic, ‘blue-skies’ research to studies that aim to evaluate health care interventions, such as randomised clinical trials and observational studies. Evaluative studies are a prime source of evidence on the effectiveness and cost-effectiveness of technologies, but they often require substantial sums of money. Thus, research funding organisations are typically called to make decisions around which of a number of competing proposals to prioritise and fund [1,2].

In a context of constrained resources, a decision to commit public resources to an activity ought to be informed by evidence on the activity’s potential benefit [3,4]. This is increasingly the case when

making treatment adoption decisions, in which economic evaluations are regularly requested to provide evidence on the costs and benefit of competing technologies [5-8]. However, decisions around research funding are typically made by inferring the potential benefits of research proposals through discussion and deliberations. Estimates of the potential value of research are not routinely requested or used as an explicit criterion in the prioritisation process, although a number of analytic methodologies aiming to quantify the value of research and inform research funding decisions are available in the literature [9-13]. Two key analytic frameworks are typically identified: 'value of information' (VoI) and 'prospective payback of research' (PPoR) [14-16].

Various obstacles may have hindered the formal use of analytic approaches in 'real world' priority-setting for studies assessing health care interventions, including methodological limitations and practical challenges. While methodological limitations have been the subject of much research, less attention has been drawn to challenges associated with introducing and using analytic approaches in research priority-setting [17]. Taking the viewpoint of research funding organisations as potential users of the methods, this paper identifies possible barriers to the use of analytic approaches in research priority-setting, discusses the degree to which such obstacles are surmountable, proposes ways by which existing challenges can be overcome, and gives the author's views on how the topic may evolve in the coming years.

The article begins by outlining the background to current research prioritisation processes and describing prominent analytic methods for research prioritisation. It then proceeds to identify and discuss challenges and uncertainties around the use of analytic approaches, paying particular attention to issues which are deemed pertinent to research funding organisations. The last part suggests ways of dealing with such challenges and points out changes and developments which are likely to take place in the next five years.

## **Current approach to research prioritisation**

In the context of health care research, the term priority-setting is commonly used to describe the process of selecting research programmes for funding [18-20]. Typically, priority-setting is required for specific proposals for applied evaluative research put forward by researchers, although, in some organisations, priorities are also set across topics for research. Prioritising specific proposals is relevant to 'researcher-led' (reactive) programmes, where teams of researchers submit proposals for applied research on topics of their choice [21-23]. On the other hand, priorities between topics are routinely set in 'commissioned' (proactive) programmes, where decisions are made about which of various topics to prioritise and commission research on [24,25].

Candidate proposals and topics are identified through different processes, including consultations with institutes involved in making recommendations on the use of health technologies, recommendations from organisations involved in the provision of health care services, direct consultation with patients and patient groups, as well as through 'horizon scanning' programmes [26-28]. Once potential topics and proposals have been identified, decisions are needed on which of them should be funded. Such decisions require determining, explicitly or implicitly, the value of carrying out a proposed piece of evaluative research. Identifying the value of a research project is a necessary but complex task, which, in principle, requires a comparison between the benefits that a research project is expected to bring about against its cost, and takes into account considerations pertinent to the funder's remit and objectives.

Currently, funding decisions are typically made through 'deliberative' (also termed 'subjective'[15], 'interpretive'[29], 'implicit'[18]) processes, which typically involve panels of experts assessing the desirability of a piece of research by discussing it in an interpretive way [18,20,29,30]. Panels deciding on research funding typically comprise representatives of groups that have expertise in the topic or a direct interest in the results of the funding process, including researchers, health care professionals, representatives of research funding bodies and, increasingly, members of the public [31-33].

More often than not, research proposals are assessed against predetermined criteria, the use of which helps to ensure that research topics and proposals are judged against relevant considerations. Criteria typically vary with respect to their intended use; for decisions on prioritising topics for research, relevant criteria focus on the importance of the topic and usually include considerations around the burden of the disease or problem associated with the topic, the topic's relevance to patients and the health care system, and the extent of the existing uncertainty around the topic [34,35]. In assessing specific research proposals, criteria tend to concentrate on the proposal's merits, by looking at the scientific rigours of the proposal, including considerations about the robustness of the research plan, the feasibility of recruiting the necessary number of participants and the ability of the research team to complete the proposed piece of research.

### **Available analytic approaches**

The acknowledgement that decision-making would benefit from explicit estimates of the value of research has led to the development of different analytic approaches [11,12,18,36]. These aim to give numerical estimates of the expected benefits of research—typically by using statistical and mathematical techniques. In general, such models are based on the acknowledgement that the objective of evaluative research is to inform treatment adoption or recommendation decisions. Given this, the value of a piece of evaluative research is inferred from the additional benefit expected to accrue from improved treatment recommendations, made in light of new evidence obtained through research. Such benefit is typically expressed as health outcomes, as well as in terms of net monetary benefit, a measure that translates health outcomes into monetary values by using hypothetical estimates of the willingness to pay for a measure of outcome [37,38]. The focus of analytic approaches is on evaluative research for existing treatments which have already been proven safe and potentially effective, rather than for trials carried out to assess treatments' safety for regulatory purposes.

Both prospective and retrospective analytic methods exist in the literature. Retrospective methods estimate the benefit from research that has already been conducted, and may be useful in assessing the impact of research funding [39-41]. On the other hand, prospective methods aim to infer the value of a research proposal by estimating the benefit that the proposal is likely to bring about before the study is considered for funding, with a view to guiding funding decisions [36,42,43]. Given the above, the focus of this article is on prospective approaches. A number of analytic models are available in the literature; on the basis of the principles underpinning them, these have been categorised into one of two overarching frameworks: 'prospective payback of research' and 'value of information' (Vol).

### ***'Prospective payback of research'***

Models based on the principles laid out in the seminal work of Weisbrod [9] and Eddy [11] have been termed 'direct assessments of the cost-benefit of research' [19] or, more commonly, 'prospective payback of research' (PPoR) [14,15]. PPoR is based on the notion that research is worth conducting insofar as its results can trigger a beneficial change in clinical practice. According to this approach, the desirability of a research programme, such as a clinical trial, can be inferred from the additional benefit that the programme is expected to generate through informing a change in practice. The additional benefit of research is measured as the difference in the benefits expected to accrue under two 'states of the world': a) the 'factual' state, where research is conducted and informs a beneficial change in clinical practice (e.g. wider use of an effective and cost-effective treatment, limited use of an inferior treatment), and b) the counterfactual state, where research is not been conducted and clinical practice remains unchanged.

Different versions of PPoR are available, though most models follow a core set of methods. As a first step, PPoR models specify different hypothetical results (often called 'delta results' [11] or 'exemplar outcomes' [36] which could transpire if research was undertaken. Typically, it is assumed that research will produce two kinds of results: 'positive' (i.e. the treatment of interest is effective and/or

cost-effective), and 'negative', (i.e. the treatment is not effective and/or not cost-effective) [11,13,44], although newer models allow for the possibility of 'inconclusive' outcomes [36,43]. The next step in the process involves specifying how clinical practice would change following each of the hypothesised research outcome and calculating the stream of costs and benefit expected to arise from each research result and the change in clinical practice associated with it. The estimated costs and benefits are typically compared to those that are expected if research is not conducted and are extrapolated to the population expected to benefit from research over a specified time horizon.

### ***'Value of information'***

'Value of information' aims to offer decision makers facing a choice between options with uncertain payoffs an analytic way of estimating the value of obtaining further information to assist this choice. In health care, the approach is based on the notion that uncertainty around treatment adoption decisions is undesirable because it leaves room for erroneous decisions and loss of benefits in the population [45]. Given this, the usefulness of evaluative research, such as a clinical trial, can be inferred from its ability to eliminate or reduce uncertainty. In essence, the framework establishes the gains from—and thus the value of—obtaining further information through evaluative research by looking into the additional benefits expected from decision-making in the light of improved information.

Two measures of the Vol are commonly distinguished: the expected value of perfect information (EVPI), which shows the maximum expected benefit from eliminating uncertainty, and the expected value of sample information (EVSI), which reflects the marginal benefit from limiting uncertainty through a study of a specific sample size and design [38,46]. Vol has been often advocated as an analytic method for identifying research priorities [47-49], assisting with research funding decisions [50,51] and determining the optimal design of further research [10,52,53]. Key characteristics of PPoR and Vol are given in Table 1.



## **Existing uncertainties**

Although there exists a fair amount of literature looking into methodological aspects of analytic approaches—especially in relation to VoI [54-60]—less focus has been given on uncertainties and challenges that may be particularly pertinent to potential users of the methods. Such issues are discussed below.

### ***Validity of results***

A first pertinent question is whether the results of analytic approaches are valid. In principle, assessments of validity require a comparison between generated results and actual, post-research observations. To establish that the frameworks produce valid results, the benefit predicted in the pre-research analysis must agree with the actual results which will be realised after research has taken place [14,43]. However, such a task is hindered by both practical and methodological difficulties. First, a long time horizon would be needed before the actual post-research benefits were observed [14]. Secondly, care would be required to isolate the benefits which can be attributed to the specific research programme taking place, so that these could be compared against the prospective estimates [43]. In addition, given the fact that calculations of the potential value of research employ a number of assumptions, disagreement (or agreement) between *ex post* predictions and actual results may be due to the assumptions used, and will not necessarily confirm or dismiss the validity of analytic methods.

### ***Fit for purpose***

A further pertinent consideration relates to the extent to which the notions underpinning analytic approaches allow them to provide unbiased answers for research prioritisation. An implicit notion in analytic approaches is that resources should be allocated with efficiency in mind, so that the greater the likely benefits associated with a research programme, the greater the value of the programme. This notion is in agreement with currently used decision rules based on the utilitarian view that

resources should be allocated to achieve ‘the greatest benefits for the greatest numbers’ [61-63]. However, just as the public may have preferences for health care resources to be used in pursuing objectives other than economic efficiency—for example equity [64-66]—society may prefer to give priority to research programmes that may not necessarily result in the greatest benefit. Objectives beyond maximisation of benefit—for example, pursue of equity, research capacity building and potential for further methodological advances— are not reflected explicitly on the results of analytic methods, and thus in situations where they are perceived relevant, these objectives will need to be taken into account as additional considerations.

Related to the above is the notion that the payoff from research—and thus, the inferred value of research—is directly proportional to the population that stands to benefit from improved information. As a result, proposals affecting larger eligible populations are more likely to result in greater benefit. In such cases, the payoff from research is more likely to exceed the cost of research, making proposals in such areas good candidates for funding. This, however, may be undesirable when it comes to evaluating research proposals for rare conditions, where the fact that less people stand to benefit—and consequently a lesser payoff is expected from investing in research—means that evaluative research in such areas may appear to be inefficient and may be overlooked.

Other issues, related to the rationale underpinning the approaches, may also lead to biased answers. For example, PPO approaches are based on the notion that the desirability of a research programme can be inferred from the additional benefit generated through a subsequent change in practice. This is an intuitive idea, which seeks to account for the ‘real’, tangible benefit that may accrue to the population due to policy changes. However, this notion has led to criticisms that, in some cases, the framework may advocate prioritising research in areas where there is greater scope for gains from a beneficial change in clinical practice, over areas where there is much uncertainty about the appropriate use of treatments, but a lesser scope for change in practice [15]. VoI is based on a ‘decision-theoretic’ stance, according to which research, such as a clinical trial, should be seen

as a source of evidence for decision-making [45,67]. This viewpoint stipulates that a clinical trial is worth conducting as long as it is expected to add to the existing evidence base and provide input for decision-making, no matter whether the generated results will reach statistical significance, or whether the trial has been powered to do so in the first place. According to this viewpoint, a trial of a few participants would be worth conducting if it produces evidence that reduces uncertainty and can be used for decision-making. Although this viewpoint has gained popularity [17,68], it is considered to be at odds with the established view that clinical trials should be carried out to test hypotheses about treatments and should be designed and powered for this purpose.

### ***Robustness of results and reliance on assumptions***

The aim of analytic approaches is to inform research funding decisions by generating estimates of the value of research programmes prospectively, before research takes place. Inevitably, such estimates are expected to be surrounded by uncertainty. The degree to which they are sensitivity to assumptions is likely to be an important consideration for potential users. PPoR methods require a series of assumptions and hypotheses around the possible outcomes of research and the likely change in clinical practice in light of these outcomes, while Vol results are greatly affected by the degree of uncertainty surrounding parameters which have a bearing on adoption-related decisions, with greater uncertainty being associated with higher estimates of the expected value of research [69]. Further, any attempt to estimate the current and future benefits of research will be sensitive to assumptions about the future incidence of the disease and the time horizon over which the produced evidence is expected to be useful. Long time horizons will inevitably inflate the number of patients that are expected to be affected by the availability of improved information, increase the expected benefit in the population and make further research appear more desirable.

### ***Practical challenges related to the use of analytic approaches***

If analytic approaches were to be used routinely, a number of practical questions around their exact role and use would be expected to arise. A first, important question relates to how estimates of the value of research should be used in the prioritisation process. Different options may be available. Under one option, results could be used in a 'prescriptive' manner, as the only determinant of funding decisions. Alternatively, results could be used as additional input in the prioritisation process, alongside other relevant considerations in a similar way that evidence from analytic methodologies, such as cost-effectiveness analyses is typically used in the process of making decisions for treatment recommendations [70]. In this case, decisions would be needed on how results of analytic approaches can be combined with other considerations in the decision-making process, and what 'weight' should be placed on prospective estimates of the value of research.

A further question relates to the additional time needed to carry out preliminary assessments of the value of research and, thus, the extent to which these could cause delays in the prioritisation process. Specific components of the analyses—for example, systematic reviews and decision analytic modelling, which are typically a prerequisite to undertaking VoI—take time to carry out. Previous work has looked into the time frames for carrying out VoI and PPOr analyses in the context of NIHR Health Technology Assessment programme in the UK. In their studies, Claxton and colleagues [47] found that decision modelling and VoI would take a team of researchers with different levels of experience approximately 10 to 12 weeks to carry out. Townsend et al [43] that PPOr analysis based on their Preliminary Assessment of Technology for Health Services model can be undertaken within one to four weeks, depending on the complexity of the project. In addition, calculations of measures of VoI, such as the EVPI for specific uncertain parameters and the EVSI for different study designs, are often computationally challenging [69,71]. In recognition of this, there has been a steady stream of research into ways of easing the computational burden of VoI calculations, including research aiming to identify conditions under which calculations can be simplified [72,73], as well as work that

employs regression-based methods to speed up the required computations [74-78]. Undertaking analytic assessments requires some degree of expertise, which may be in short supply. The lack of trained researchers to undertake the required analysis, should Vol analyses become part of the prioritisation process, was highlighted by Claxton and colleagues in 2004 [47]. While, since then, the growing interest in Vol, as reflected in the number of published Vol analyses [17,79], may have led to increases in the number of researchers who are able to undertake such analyses, it is likely that, at least in the short term, capacity limitations may continue to be an important consideration.

As limitations in capacity make it difficult to carry out prospective assessments of the value of research for every proposal and topic considered by a funding organisation, some judgement would be needed to determine which proposals should be subjected to analytic assessment [43,47]. An obvious selection criterion may relate to the cost of the proposal [43], with proposals requiring funds above a certain threshold expected to undergo analytic assessment. However, as it is argued in the next section, establishing a cut-off value is expected to be a challenging task. Similarly, questions are expected to arise in relation to who should be tasked with undertaking these additional analyses. An obvious option is for these analyses to be undertaken by independent teams of researchers, commissioned by research funders. Alternatively, the analyses could be carried out by researchers requesting funding, and could be presented alongside their proposal. However, care must be taken to ensure that these additional requirements do not end up posing a substantial burden which may discourage the submission of research proposals.

## **Expert commentary**

The acknowledgement that public funds for research need to be allocated efficiently have given rise to increasing calls for the use of analytic approaches. Despite this, analytic approaches are yet to become an integral part of 'real world' prioritisation processes. Uncertainties and challenges which are thought to hinder the use of analytic approaches were described in the previous section. This

section discusses whether these challenges are insurmountable, and suggests ways by which these difficulties may be overcome.

The degree to which validation of the results of prospective assessments is a strict requirement before they can be used in practice is unclear. While confirming the validity of results generated through analytic models is likely to enhance the credibility of such methods and strengthen the case for their use, unconfirmed validity does not necessarily imply that the produced results are biased or flawed. Despite the fact that results of cost-effectiveness and cost-benefit analyses are rarely validated, these are widely accepted and used, as they are generated through methods which are thought to have a robust theoretical and methodological basis [80].

Arguably, the fact that estimates of the value of research generated from analytic approaches do not take into account additional considerations that society—and, by extension, public funding organisations—may deem important (e.g. the potential of a proposed research to reduce inequalities) [30,43] may be seen as a limitation. However, this limitation can be overcome by thinking carefully how the results of analytic approaches should be used in priority-setting. If results were to prescribe—rather than inform—research funding decisions, additional considerations may, inevitably, be missed out. If, on the other hand, results of analytic approaches were to be used as an additional, rather than the sole, criterion for decision making, funding organisation would be able to ensure that any potentially relevant considerations (e.g. societal preferences and equity concerns) can be taken into account as appropriate.

Assumptions in the evaluation of costs and benefits of different activities, be it health care programmes or projects of public infrastructure, are, to a large extent, unavoidable [4,81]. This would be expected to hold true for assessments of the value of future research, especially because such research is yet to take place and estimating its payoff requires guesses and speculation. It is thought that, rather than discarding findings which, to some extent, are based on assumptions, the

effort should be towards making sure that employed assumptions are made explicit, are plausible, and are based on the best of the available knowledge.

Time and expertise needed for undertaking preliminary assessments of the value of research may indeed pose a barrier to a widespread use of analytic approaches. Nonetheless, there is now a steady stream of research looking into ways of reducing the burden of undertaking Vol analyses. Such research has led to establishing situations in which Vol can be conducted with minimal modelling, as well as 'shortcuts' and efficient computational methods in calculating Vol measures [60,73,75,76,82,83]. This, coupled with explicit efforts to increase research capacity [84,85], imply that challenges related to time and expertise requirements are now less prohibiting than before.

Questions around who should be tasked with undertaking these extra analyses and in what cases analytic assessments should be undertaken appear to be more contentious. In the context of 'proactive' funding streams, in which institutions commission research on proposed topics, analyses such as Vol and PPOr could be undertaken either internally, within the research funding organisation, or externally, by academic teams commissioned to carry out this work on behalf of the funder. The latter would be in agreement with current arrangements under which primary and secondary research commissioned on behalf of different stakeholders is undertaken by contracted teams of researchers [86]. The option of funding organisations undertaking in-house analyses for their own use may also be feasible. However, this would require significant increases in organisations' capacity to deal with the additional work, which may be more plausible as a long-term goal. Additionally, there may be opportunities for funding organisations to use results of analytic assessments which may have been carried out by other institutions. This is particularly the case when evidence on the value of further research may be available from analyses undertaken to inform joint treatment adoption and research funding recommendations [87]. In such cases, potential synergies may be exploited by using the results of available analyses to underpin decisions to commission further research, or to specify optimal design characteristics [87,88].

In 'researcher-led' streams, where teams of researchers put forward proposals for research on topics of their choice, these additional analyses can be undertaken by either the team of researchers who submit the proposal, or by commissioned academic institutions. In the former case, researchers can present results as part of their proposal, to substantiate the need for the proposed study. However, there may be notable advantages in commissioning independent academic institutions to carry out this work, instead of asking researchers who are involved in the specific proposal to undertake the analysis: this would remove the burden from researchers who may be lacking the expertise to carry out these additional tasks, while it would provide assurance that the analyses would not come from teams of researchers who have a vested interest in showing that research is worthwhile and should be funded. As a downside, such an option would inevitably imply that the costs of undertaking the analysis would be fully borne by the funding organisation.

Given current capacity constraints, it may be infeasible to carry out such analyses for every proposal submitted for funding. Current processes, where research proposals are selected through different prioritisation stages, can be used to filter specific proposals which are considered to be good candidates for research, but for which further evidence would be needed to support a funding decision. An obvious option would involve carrying out prospective assessments only for proposals which have reached the last stages in the prioritisation process. In this way, it will be assured that time and effort is not spent on proposals which are out-of-remit, or for which experts and reviewers agree that they do not represent good candidates for research. Alternatively, Townsend et al. [43] have suggested that analyses could be restricted to proposals which are deemed particularly costly by employing a monetary cut-off point could be used, above which all proposed trials should be evaluated using analytic approaches. Using a monetary threshold to identify costly trials is an unambiguous and straightforward criterion, although complications would be expected to arise if a universal threshold was to be determined, given that the cost of research is expected to vary across disease areas. Indeed, clinical studies on topics which require long term investigations involving a



large numbers of participants would inevitably be more expensive than smaller and shorter studies which may be adequate for other topics. Owing to this, it may be preferable for any monetary cut-off points to be determined by taking into account particular aspects and characteristics of research in different disease areas.

It is perhaps surprising that, despite the increasing ‘academic’ interest in analytic approaches, key questions about their role and use have been relatively under-studied. Research around analytic approaches has focused predominately on addressing methodological limitations. While such research is undoubtedly beneficial, it is evident that future efforts should also be directed towards answering contentious questions around the role and use of analytic approaches in ‘real world’ research funding decisions. A stream of research, workshops and consultations would be needed to determine the most appropriate ways of introducing analytic methods into existing research funding processes. It is envisaged that potential users of the frameworks will need to establish ways for incorporating analytic methods which are efficient and acceptable to both researchers and decision makers. As a first step, this may involve looking into process-related issues that are specific to funding organisations, such as how the steps they currently follow may be modified to incorporate analytic approaches, and whether and to what extent current timelines should be extended to account for the additional analyses. Equally importantly, funding organisations will have to address questions on how results should be combined with other considerations deemed important to society (e.g. equity), and what ‘weight’ should be attached to estimates of the value of research obtained through analytic methods.

### **Five-year view**

Against this background, a relevant question is whether, in the coming years, analytic approaches are likely to find their way into the prioritisation process of public funding organisations. Past experience shows that, despite an enduring interest in the methods—predominately Vol—there has

been little progress towards such methods being formally employed in research prioritisation. Nonetheless, there exist a number of reasons why such a move may gain momentum in the future. First, the spread of evidence-based decision-making in health care has steadily led to an increase in the demand for evaluative studies. As the number of evaluative studies competing for a finite budget increases, the need for priority-setting is expected to be amplified and the interest in ways in which research is prioritised is expected to be rekindled. This interest, together with the acknowledgment that funding decisions ought to be made in light of evidence around the cost and potential benefit of competing activities, is likely to turn the spotlight on the current process of research prioritisation and place more emphasis on alternative, analytic approaches.

A second pertinent question relates to the direction that research on analytic approaches may take in the future. In the last 10 years, research on the topic has focused predominately on addressing methodological issues [17]. While this has led to advances, equally important questions around the role and implementation of analytic approaches have remained unanswered. It is thought that, as methodological issues are resolved and familiarity with the methods increases, the focus will naturally turn to answering questions related to the role and implementation of the approaches. For this, there will be a need for discussion and consultations between the research community and research funders to determine feasible and acceptable ways of incorporating analytic assessments into the prioritisation processes.

Clearly, addressing the challenges outlined in this article is a demanding task which will require the joint efforts of researchers and funding organisations. To a great extent, future obstacles to implementation are not expected to be too dissimilar to those encountered in promoting the use of economic evaluation for treatment adoption and coverage decisions. It is thought that a first, important step towards this aim is to acknowledge that grounding research funding decisions in explicit evidence, produced through systematic and analytic process is not an option, but a justified necessity.

## Key issues

- Research funding decisions represent investments of public resources and, in principle, they should be informed by evidence on the cost and benefits of competing research programmes.
- Currently, estimates of the potential value of research are neither requested nor taken into account in research funding decisions, despite the fact that analytic approaches to provide such estimates are available.
- A number of factors may have deterred the formal use of analytic approaches, including uncertainties around their validity, questions about the appropriateness of using results of analytic methods for priority-setting, and consideration around practical aspects of using analytic approaches as part of the prioritisation process.
- Addressing these challenges is likely to be a demanding task which will require the joint efforts of researchers and funding organisations.
- In the years to come, it is expected that greater emphasis on ensuring a transparent and efficient use of public funds, coupled with an increasing demand for health care research, will create a more receptive environment for the use of analytic approaches in the future.
- As methodological issues in the approaches are resolved, future research should gradually turn to challengers related to the role and actual use of the approaches in the research prioritisation context.

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