

# Evaluating the Impact of Research Programmes - Important issues to consider in evaluating a research programme (best practice)

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## Important issues to consider in evaluating a research programme (best practice)

The workshop discussion drew out a number of issues that those trying to evaluate the impact of research programmes need to consider before selecting the most appropriate tools or approaches. Many of these are not specific to evaluating research – but a number are made more challenging by the nature of academic research and its diffuse pathways to impact.

### Contribution/attribution

Attribution of impacts through comparison with the counterfactual (that is, the absence of the programme) is a rarity in the evaluation of research. A more realistic objective may be to identify ways in which a programme made direct contribution(s). Crucial factors, therefore, are the scope and robustness of the evidence collected for evaluation and the quality of analysis thereof. The validity of conclusions rests on the rigour of the evaluation process.

In order to understand how impacts (may) have occurred, evaluations need to assess the impact processes (i.e. what the researchers did or did not do to enhance the potential for their research to make a contribution). This strengthens the robustness of evidence supporting accounts of contribution, but even so it can still be extremely difficult to disentangle to what extent the research helped to cause (contributed to) the observed outcomes.

The realities of how research is commissioned and funded by multiple organisations can make it especially difficult to tease out individual funders 'share of the credit' for impacts. When donors joint-fund research programmes, each normally claims a proportionate share of the impacts. But when researchers receive multiple grants for closely related projects, it can be impossible to assess reliably how much each programme has contributed, and therefore what change is due to an individual funder.

See [John Young's slides](#) for the challenges of teasing out contribution, and examples of where research has directly impacted on policy.

See also this page on [Contribution Analysis](#).

### Timing

Academic research is often a slow process, and there can be a significant time lag (years or decades) between knowledge generation and impact, particularly through changes to public policy. Funders of research will need to decide on the most appropriate time(s) to evaluate a research programme, and plan and budget accordingly.

There is a need to balance the allowance of sufficient time for impacts to have occurred against the likely decrease in the availability of people, documentation and other sources for involvement or consideration in an evaluation. A longitudinal evaluation design may help in this respect, but may well appear costly.

Time-lags to impact are likely to be greater and less predictable in basic compared to applied research, which has implications for both the choice of evaluation approach and its timing.

## Independence and governance

For evaluation of a research programme to be reliable and therefore useful, the evaluators are normally expected to be independent (not stakeholders) of those managing and funding the research, and the evaluation itself should be well managed and governed. However, this can produce a sense that researchers and their users are 'subject to' evaluation. [Participatory](#) and recipient led evaluation methods may enhance the role of stakeholders, offering certain strengths (grounding in the context, knowledge of the key questions) and recognised weaknesses (validity of self-reporting).

## Gender

Organisations committed to gender mainstreaming should ensure that evaluations reflect this commitment, for example through highlighting impacts of research programmes for both women/girls and men/boys and engaging an appropriate balance of men and women throughout the evaluation process.

## Cost and proportionality

Evaluation, and the time required of those being evaluated, should be proportional to the scale of research investment and the likely value of the evaluation findings. One would generally expect only a small percentage of the programme budget to be devoted to evaluation. However, the data needed to trace and assess impacts are often not readily accessible (see [Potential challenges to more effective evaluation of research impact](#)), and collecting bespoke data from scratch can be very time consuming. Under-resourcing, therefore, is a key risk to rigour and robustness and hence the real worth of an evaluation study.

## Rigour

Whatever approach/methods are chosen (whether qualitative or quantitative), these need to be conducted with adequate rigour in order to meet international standards for evaluation (this contrasts, for example, with expert reviews, which may not have defined standards to meet). Meeting the required levels of rigour might be difficult to square with budget and proportionality considerations. So, even if motivation to evaluate is quite high, it might be better either not to proceed or to consider a less expensive form of external review.

## Extent of research organisations' influence on the outcome measures

Research for international development is designed to contribute to sustainable development, evidenced through improvements in the lives of poor. So, crop science research might aim to deliver more nutritious or more resilient crops, which should improve the health and wellbeing of people in a particular region or country. But, is it fair to evaluate the crop research programme against the proportion of under-nourished children in the country? Is that measure something over which the programme has enough influence to be evaluated against?

Similarly, is it fair to measure a research institute against changes in policy on which they may have little control? Perhaps assessing them against the levels of evidence in the debate would be more appropriate.

Understanding the spectrum of impact (conceptual, instrumental and capacity building – see [Why and what? Motivations for evaluating impact and tools for framing the right question](#)) can help target the most appropriate unit of analysis and related indicators.

See [Simon Batchelor's slides](#).

## Outputs (and expectations)

The motivation for evaluating the research programme (see [here](#)) should shape the type of evaluation outputs that are most suitable for the intended audience. Are the outputs from the evaluation likely to be suited to the intended audience and users and be meaningful to them? Are the outputs descriptive or explanatory? Can an appropriate balance be struck between brevity and depth of insights?

## Uptake and impact (of the evaluation)

There is little point investing in evaluating research programmes if this information is not taken up and used to shape future policy/practice/funding. Funders and commissioners of evaluation should consider the uptake and impact strategy of the evaluation itself, not just the uptake and impact strategy of the research.

## Serendipity, and windows of opportunity

**We know that the impact of research is heavily non-linear and, to a considerable extent, unpredictable. Sometimes this involves a serendipitous convergence of factors that provide a pathway to impact, or taking advantage of particular moments when decision makers want new ideas (after shocks, scandals, changes of leadership etc.) which can come from research. While recognising the value of planning to have influence (through an uptake strategy), the framing of pathways to impact, and associated evaluations, also need to allow for previously unforeseen opportunities. The opening and closing of these ‘windows of opportunity’ should be taken carefully into account, with associated revisions to the [Theory of Change](#), [LogFrame](#) and/or [stakeholder map](#).**