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Review

Policy-oriented environmental research: What is it worth?

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ABSTRACT

Environmental and conservation scientists are increasingly being asked to justify their work in terms of benefits to society. This article describes economic theory for conceptualizing the benefits from environmental research, and provides a framework for estimating those benefits. In particular we discuss the evaluation of environmental science that is intended to benefit society through informing policy decisions. The chain between environmental research and its benefits through policy change includes at least four links: the research itself, policy change, behavior change and environmental change. Each of these four stages presents challenges and entails time lags. If any link fails, the chain breaks. The standard economic model of supply and demand, which is used to quantify benefits from research into market goods, can be adapted for application to environmental goods. Improved conceptualization and measurement of benefits from environmental research would assist environmental scientists to: (1) select research topics that are likely to deliver large environmental benefits; (2) design their research in a way that will increase its relevance, usefulness and potential impact on policy and, ultimately, the environment; and (3) make the case for funding particular research proposals. It could assist research agencies or research funders to: prioritize proposed research; make a case for increased funding for environmental research; and identify obstacles to the translation of research into environmental impacts, allowing attention to be focused on addressing these bottlenecks.

1. Introduction

Little is known about the returns to investments in environmental research. Governments are requiring greater accountability, and some research funders are requiring information about research benefits to be provided in research proposals, but a coherent framework for evaluating the benefits resulting from environmental research is lacking.

Literature on the economics of research provides detailed guidance on how to estimate the benefits generated by research, and shows that typical rates of return on research investments in some fields are high (e.g., Alston et al., 2000; Productivity Commission, 2007; Hurley et al., 2014). However, existing methods are best developed for research that leads to reduced cost and thereby increased profit for commercial firms, or benefits to buyers from improved products or lower purchase prices (e.g., Alston et al., 2010; Hall and Rosenberg, 2010), or both. Research that is intended to provide "non-market" benefits—through the provision of environmental goods that are not fully priced in market transactions—is more difficult to evaluate, particularly if the target audience is policymakers (e.g., see Pardey and Smith, 2004). Few publications on the evaluation or prioritization of environmental research (e.g., Guidotti 1995; Wu and Hobbs, 2002; Spilki and Tundisi, 2010; Bell et al., 2011; Sutherland et al., 2011) make use of the extensive literature on the economics of research (Ferraro and Pattanayak, 2006).

The main objective in this paper is to identify and discuss principles, concepts, and methodological approaches for estimating benefits from environmental research, with a particular focus on policy-oriented research. In doing so, we use theory and evidence from existing literature, particularly in agricultural economics; we describe conceptual models of the mechanisms through which benefits arise from policy-oriented environmental research; and we discuss the range of information

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required to estimate the benefits, and how that information can be combined to provide meaningful estimates of the benefits.

2. Challenges in valuing benefits from environmental research

That environmental research can generate benefits through improving policy seems obvious. Major environmental agencies around the world employ researchers, fund external researchers, and emphasize the importance of considering research results in policy. Examples of environmental issues where research appears to have played a role in prompting new policy or shaping changes in existing policy include global climate change (Manabe and Wetherald 1967), ozone pollution (Farman et al., 1985), management of renewable natural resource stocks such as fisheries (Smith et al., 2008), water pollution (Doole and Romera, 2015), soil conservation (Mitchell et al., 2016) and conservation of marine biodiversity (Possingham et al., 2009). However, quantitatively estimating the benefits that have resulted from the research that influenced these policies remains difficult, even when evaluating completed research.

To estimate the benefits from any type of research, a key challenge is the "attribution" problem: the difficulty of determining the contribution of particular research investments to a real-world outcome. Part of the difficulty arises because we have to estimate benefits from research as the difference between two scenarios: outcomes that occur with the research versus outcomes without the research (the "counterfactual" – Ferraro, 2009; McConnachie et al., 2016). Even if the research has already been undertaken and the results are known and in use, at least one of these scenarios—the second—is not observable. We have to estimate what would have been different if the research had not been undertaken. If the evaluation is of research that is yet to be conducted (e.g., to assist with setting research priorities), then even the results of the with-research scenario have to be predicted, adding further to the attribution problem. The challenge includes predicting what the research will yield, in terms of knowledge about the world, as well as how that new knowledge might be used in policy, and what the consequences of that use might be, allowing for the behavioral responses of diverse individuals.

Defining these scenarios (with research and without research) is made more difficult because research time lags are long and uncertain, and many changes occur in tandem. Typically, applied research takes years, perhaps decades, before it yields useful results that can be adopted. The adoption process itself then takes time as managers learn about the research results and how to apply them best in their specific contexts. The effects of that adoption may persist for many years. For example, Alston et al. (2010) found that aggregate U.S. public agricultural research and extension had little effect on farm productivity within the first 10 years, and reached its maximum impact with a lag of 24 years, with residual impacts detectable beyond 40 years. Notably, these long time lags are predominantly for research that generates private benefits for the adopters. For environmental research, the lags may be even longer because the intended research users may lack the incentive to adopt, or face political barriers to adoption.

Whether evaluating research after its effects have fully unfolded, or anticipating future effects from research that may not have been done yet, it is necessary to make estimates of various parameters (see, e.g., Alston et al., 1995): (1) the costs attributable to the particular research investment (and to the associated adoption process if additional resources are required for that); (2) the time path and extent of adoption and use of the research results; (3) the magnitudes of the impacts on outcomes of interest (e.g., environmental conditions, costs) with adoption of the research results compared with a well-defined counterfactual without-research scenario; (4) the values associated with the changed environmental conditions attributable to the research, and (5) the benefits or costs of any side-effects of the changes resulting from the research investment.

In the case of policy-oriented environmental research, these challenges are exacerbated for at least two reasons. First, the benefits typically cannot be observed in market transactions. Second, the "adopter" of the research results is a policymaker or policy administrator rather than a commercial firm, so we usually cannot observe "adoption" as such. We may observe a policy change but we typically do not know whether we can ascribe it to a particular cause. In short, in addition to the general challenges in research evaluation, evaluating policy-oriented environmental research is especially problematic because of difficulties in ascribing a particular policy change to a particular research investment, estimating the consequential changes in environmental outcomes, and assigning a monetary value to them.

If the aim is to estimate the benefits that will arise from research that has not yet been conducted (*ex ante* research evaluations—as opposed to *ex post* evaluations conducted after the research has been completed)— the difficulties are further increased. Importantly, research is an inherently risky business whose results cannot be known in advance. Many research projects will not yield information that is pertinent for policy decisions. We may know little about the time it takes to generate research results or the time it takes for those results to influence policy decisions and to see those decisions implemented. Looking forward 20 years or more, we have considerable uncertainty about the economic, environmental, social and political context in which policy changes will take effect. This uncertainty extends to the potential consequences of a policy change (presuming it does happen), the size of any adverse or favorable side-effects, and the counterfactual against which it should be compared.

3. The role of research-based knowledge in the policy process

We envision a policy process in which research plays a role by generating information that can change public or policymaker perceptions about alternative policies and consequently can influence the policy choice. Our focus is on research that changes perceptions leading to altered policy decisions.

Suppose that various policy options are available for managing an environmental issue. The options may differ in their budgetary implications, policy mechanisms, spatial targeting, or approaches to policy implementation, as well as their effectiveness in managing the issue. Information obtained from research about their attributes influences which of the policy options is perceived to be superior. The comparison of the performance of the policies is based on perceptions and involves uncertainty, although probably less uncertainty than there would be without the research.

In the absence of the research, a particular policy option would be chosen by policy makers. If the research project is conducted and yields policy-relevant information, it may lead policy makers to choose a different policy option. If the policy choice does not change, the research generates no benefits via this route, although it may do so in other ways. For example the new, research-based information may influence how people act in the presence of a given policy (e.g., the extent to which they comply with the policy), which could change the environmental outcomes even without changing the policy.

There are various possible mechanisms through which research could lead to a policy change. Most obviously, it could directly influence the perceptions of policymakers about the need for a policy, or the relative merits of the policy alternatives. It could generate technologies that make a policy more feasible or less costly, causing policy makers to switch from opposition to support for that policy. Alternatively, it could motivate people to advocate for policy change, thereby influencing policy decisions.

If the policy choice does change as a result of the research, the environmental benefits equal the community's value of the difference between environmental outcomes under the without-research policy, and the outcomes under the with-research policy. This framework is essentially a Bayesian-style value-of-information analysis, long-established in economics and recently growing popular in applied ecology