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PERSPECTIVE

Making ecological science policy-relevant: issues of scale and disciplinary integration

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Abstract In this paper, we ask why so much ecological scientific research does not have a greater policy impact in the UK. We argue that there are two potentially important and related reasons for this failing. First, much current ecological science is not being conducted at a scale that is readily meaningful to policy-makers. Second, to make much of this research policy-relevant requires collaborative interdisciplinary research between ecologists and social scientists. However, the challenge of undertaking useful interdisciplinary research only re-emphasises the problems of scale: ecologists and social scientists traditionally frame their research questions at different scales and consider different facets of natural resource management, setting different objectives and using different language. We argue that if applied ecological research is to have greater impact in informing environmental policy, much greater attention needs to be given to the scale of the research efforts as well as to the interaction with social scientists. Such an approach requires an adjustment in existing research and funding infrastructures.

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Limited scale, limited impact?

Environmental policy needs to be based on good-quality scientific data. However, there is currently concern within the ecological community about the lack of impact that ecological research has on policy. There is an increasing need to demonstrate knowledge transfer between science and policy (Sutherland et al. 2004; Balmford and Bond 2005; Mattison and Norris 2005). Within the UK, these concerns are reflected through initiatives such as the Centre for Evidence Based Conservation at Birmingham University, the Web site conservationevidence.com, the appointment of a Science Policy Manager by the British Ecological Society, the Research Councils and Department of Environment, Food and Rural Affairs (DEFRA) funded Rural Economy and Land Use (RELU) research program and the publication of the Natural Environment Research Council (NERC) publication '*Science into Policy—taking part in the process*' (NERC 2005). In light of the lack of policy impact we see in ecological research, this paper aims to draw attention to some of the barriers that stand in the way of ecologists and to make some suggestions for how they can be overcome. Landscape ecology represents an attempt by some ecologists to move beyond the confines of the discipline by embracing the need for interdisciplinarity (Wu and Hobbs 2002). This does not make landscape ecology an ideal model but it does make it a useful example of how to conduct this kind of research (e.g., Opdam et al. 2002; Santelmann et al. 2004; Wu 2006).

So why is there an apparent lack of policy impact in the UK? It could be because research has been unable to deliver appropriate insights to inform policy, or because policy has been developed without a sufficient evidence base. Whatever the ultimate reasons, we feel that one of the contributing proximate factors relates to problems of scale (i.e., the spatial, temporal, quantitative or analytical dimensions used to measure and study any phenomenon: Gibson et al. 2000). In particular, many land-use and environmental policies are designed, implemented and operated at regional, national or even international scales (i.e., Tomich et al. 2004), even though individual land-use decisions may be made at much smaller scales. Although there are some examples of genuinely large-scale experiments in ecology [e.g., the Hubbard Brook Experimental Forest (Likens 2004) in the USA and the Plynlimon catchment (Neal 2004) in mid-Wales], most ecological research is carried out on experimental plots that are at a field scale or smaller (sometimes very much smaller). Large-scale experiments can have an impact on environmental policy, e.g., the 'Science Links' project at Hubbard Brook Experimental Forest has conducted more than 20 briefings with policy-makers on acid deposition alone (Hubbard Brook Research Foundation 2007).

Does scale matter? It would appear so from various debates in the literature. For example, there is a growing body of evidence that agri-environment schemes currently in place across Europe are not delivering the anticipated benefits for biodiversity. Under management agreements in the Netherlands, farmers are paid to make changes in the management of their land with the aim of conserving and enhancing biodiversity. Kleijn et al. (2004) reported that management agreements designed to enhance botanical diversity did not have any significant positive effect. Equally, meadow-bird agreements did not help any of the target bird groups and, at a field scale, waders were actually found to be avoiding fields under such agreements (Kleijn et al. 2001). This problem is not limited to the Netherlands. In a meta-analysis evaluating agri-environment schemes throughout Europe, Kleijn and Sutherland (2003) reported that prescriptions aimed at enhancing botanical diversity had a poor success rate. Impacts on arthropod diversity were more positive (14 out of 20 studies examined showed a significant increase in arthropod diversity) but the studies examining bird populations showed no significant pattern. In contrast, van Buskirk and Willi (2004) used a used meta-analysis and showed that 127 published studies on land retirement from agricultural production did produce improvements in biodiversity. As a result, they conclude that agri-environmental polices are achieving key biodiversity objectives. But, Kleijn and Baldi (2005) have cast doubt on this finding and the resulting policy implications because of, amongst several factors, the need to explicitly take into consideration and control for scale in statistical analysis. The original analysis (van Buskirk and Willi 2004) was conducted over North America and Europe; however, Kleijn and Baldi (2005) pointed out that this analysis should have been conducted at a country level for policy and land-use covariates to be assessed. In conducting the meta-analysis at such a large scale, small-scale dynamics are missed resulting in a failure of down-scaling. More generally, amongst several reasons proposed for the lack of success of agri-environment schemes [including, for the Netherlands, the highly intensive nature of agriculture and financial motivations of farmers (Kleijn et al. 2001)], one factor identified was that while management prescriptions prove effective under experimental conditions, when applied at a landscape scale they are not. This lack of success when up-scaling may be related to factors which cannot easily be accounted for at a smaller scale, such as landscape context, refuge effects and population dynamics. The problems with the analyses presented here highlight the importance of conducting investigations at a scale appropriate to the question being asked.

There are other examples of the importance of scaling up and the resulting impact on policy and land-use practice and planning. For example, in the development of biological alternatives to chemical pest control the UK government has made, and continues to make, considerable investment in research (Advisory Committee on Pesticides 2003). A few of these studies have achieved high levels of adoption (e.g., augmentative introductions of predators and parasitoids for biocontrol of pests in protected tomatoes) but most have not. In particular, the virtual failure of biologically based pest control in annual arable crop systems is striking. Again, while there are many factors that influence the adoption of new technologies, we would argue that scale is an important factor. The standard approach for evaluating a technology over maybe one or two fields for 3 years provides a weak foundation for effective translation to farm or landscape levels let alone the regional or national levels.

In light of this type of evidence, we argue that more thought must be given to the issue of scale as it relates to the generation of scientific evidence to inform policy design and landscape planning (see also Costanza et al. 2002; Urban 2005; Kremen 2005). There is a need for investigations to be conducted at a scale that is relevant for the policy question they are addressing. This may mean that experiments need to be combined with other approaches. For example, the Farm Scale Evaluation (FSE) of Genetically Modified Herbicide Tolerant (GMHT) crops was one of the largest ecological experiments ever funded in the UK. However, in spite of its name, this was, in reality, a highly replicated field-scale experiment, and not a farm-scale experiment. As such, the FSE did not and could not address the important landscape level question of what might happen if GM crops were grown over large acreages in the UK. Instead, by replicating single field treatments throughout the UK, it told us what might happen if GMHT crops were grown on several fields within a region, no matter what part of the UK these were grown in. Whether this is a realistic scenario or addresses the issue of greatest importance for policy is an interesting question. If there were high levels of adoption of the technology, the majority of arable farming within a region might be growing GM crops. Thus, while the FSE results show, for example, that bees and butterflies were negatively affected by GMHT crop management at the field level (Haughton et al. 2003), they give us no information on what would happen with increasing scale of adoption. At a landscape scale, we might see a linear decrease in the numbers of bees and butterflies with increasing GMHT crop cover. On the other hand, numbers could remain stable with increasing GMHT crop cover if the insects are able to survive the new management regime and the differences observed in the FSE are derived from a redistribution of populations because the insects find the crops less desirable than non-GMHT varieties. Alternatively, there could be a threshold of GMHT crop cover above which numbers fall dramatically due to a shortage of suitable habitat. Had the FSE been designed with the aim of addressing this landscape question of increasing scale of adoption, models and other tools could have been used to draw inferences and make meaningful policy-relevant predictions. As it is not directly possible to conduct an experiment at a sufficiently large scale to answer this question for legal and ethical reasons, such an investigation would inevitably need to draw upon more areas of expertise, i.e., the research project would become an interdisciplinary one by necessity. A scaling up of this study to several fields and even a true ‘‘farm-scale’’ would permit some scaling relationships to be determined but the really address this question larger-scale studies would need to be combined with an interdisciplinary approach. Field investigations could be related to study of the behavioural response of species of interest and individual-based models in order to determine the mechanisms of change. This could then be combined with social and economic studies on the likelihood of uptake and larger landscape-scale models in order to answer the question of policy importance.

The FSE also showed declines in the abundance of agricultural weeds with GMHT beet and spring oilseed rape management (Heard et al. 2003). Watkinson et al. (2000) modelled the effect of introducing GMHT beet to the UK on the population dynamics of the common agricultural weed *Chenopodium album* and the consequent impacts on bird populations. They observed that depending on the level of farmer uptake, the effects on weed seed numbers and hence bird populations could be negligible to severe. The uncertainty in this data could potentially have been tackled with a large-scale interdisciplinary project.

Opdam et al. (2002) present a research strategy for assisting in the decision-making process undertaken by landscape planners and policy-makers which could have been applied here. They describe a four-layered 'knowledge pyramid' consisting of empirical studies on different scales, organisms and processes followed by modelling to extrapolate results over space and time. These studies are further developed with modelling to produce guidelines and general rules. Finally, tools can be developed for integration at a landscape level. Opdam et al. (2002) highlight a lack of research in the final two areas. However, as demonstrated here, this can also be partially attributed to a lack of appropriate data in the first stage. An alternative approach is outlined by Carpenter (1998) who emphasises the importance of the four legs upon which ecology stands: experiments, long-term monitoring, modelling and comparative analysis. All the approaches have strengths and weaknesses and alone do not provide a sufficient basis on which to form policy decisions but when used in an iterative manner, forming hypothesis from theory and comparative studies, conducting experiments to test the hypotheses and using the results to build models and then returning to comparative studies to test the models and begin the cycle again, knowledge is built up over time in an adaptive process (Holling 1978).

Not just ecology alone

Beyond the ecological, science-based challenges (of which we argue scale to be one of the most important), policy relevance demands an interdisciplinary, or indeed transdisciplinary, approach to research that combines the working of natural scientists and social scientists and policy and landscape professionals. Without interdisciplinarity, there is little knowledge transfer between ecological scientists and policy-makers; understanding the needs of land owners, those who use the land and the general public is critical to creating relevant policy (Gutrich et al. 2005; Thornton and Laurin 2005). The need for integration between disciplines has been identified by the ecological, policy and social science research communities (e.g., Brewer 1999; Daily and Ehrlich 1999; Fox et al. 2006; Karlqvist 1999; Naiman 1999; Pickett et al. 1999; Wear 1999; Wijkman 1999; Armsworth and Roughgarden 2001; Campbell 2005; Harvey 2006). However, what is apparent from many of the advocates of this collaborative research agenda is that "talking the talk" is far easier than "walking the walk."

There are clearly barriers to effective working between scientists and social scientists and although there has been considerable progress in bringing different disciplines together through thematic research programmes such as RELU (Lowe and Phillipson 2006), progress appears to be quite slow. It is still the case that many scientists do not think to bring social scientist into their projects, and certainly not at the important early planning stages. The reciprocal is probably equally true. One reason for this is, we feel, a basic lack of "trust" and understanding between researchers from different disciplines.

The importance of trust in interdisciplinary research cannot be overstated. It is well understood in the social sciences that trust complements all forms of relationships by reducing uncertainty, helping with the management of unforeseen contingencies, and creating valuable time resources (i.e., you stop having to waste time) (Wilson 2000). Therefore, trust is a productive and valuable asset (albeit non-tangible) and it needs to be invested in. It is also the case that trust can be lost or it can diminish, but equally, it is transferable to other relationships as a result of reputation. One of the contributing factors to the lack of trust is the ''language barrier'' (Norton and Toman 1997). Terminology is frequently very subject-specific and without considerable effort from all parties involved, the lack of a common language easily becomes a barrier. Multiple meanings of words, different use of common words and the use of ''jargon'' lead to a lack of understanding.

A further constraint to effective communication and collaboration is the issue of scale itself. The word ''scale'' has different meanings within different disciplines. This can be as a geographical scale where the size or spatial extent is referred to, or a measurement scale such as the spatial or temporal extent of a database. Social scientists and natural scientists tend to work on different spatial scales (Chave and Levin 2003). Economists will generally work at a larger scale than ecologists. Veemaat et al. (2005) carried out a literature search of spatial studies in landscape ecology and those in economics or combined ecology and economics. They found that studies in landscape ecology had a very broad range of extents and grain size, whereas those involving economics tended to be larger in both extent and grain size. They suggested that this was because economics is more geared towards ''real-world spatial entities'' and economists are frequently addressing policy issues and questions at a regional, national or supra-national scale.

Furthermore, before data can be integrated, a common scale at which to aggregate data must be established. For example, Pascual et al. (2003) used data on species diversity from the countryside survey Haines-Young et al. (2000) and the Farm Business Survey (FBS) conducted annually by DEFRA, provide financial and technical change information together with conservation input for farm businesses. Data from the countryside survey are collected at a plot level; this varies from 2×2 m^2 for habitat plots to 14×14 m^2 for broader field habitats. Data provided by the FBS are at a farm level. In order to combine these data, an appropriate level for meaningful aggregation had to be determined. In this study, the level chosen was the one which led to a biodiversity index per farm being calculated. This approach was used because farms frequently cover more than one habitat type and different landscape features. This practical solution to a complex problem allowed the research to continue, but it does cast doubt on the meaning of a farm level index of biodiversity that is based on observational survey data collected at a much smaller scale. Although this study attempts to overcome difficulties associated with data collection at different scales, it highlights a problem commonly encountered by social scientists; ecological data are frequently collected at a scale which is not socially or politically relevant. Consequently, assumptions must be made to scale data up, thus introducing further error. With the data limitations understood, the authors argued that there was no negative impact of biodiversity conservation measures on production of those farms using environmental best practice methods. The results of this study have potentially important implications for agri-environmental policy indicating as they do that the introduction of policies to protect biodiversity in semi-natural habitats has the potential to enhance biodiversity without impairing agricultural productivity.

If results have been scaled-up using complex models, not only does this increase uncertainty but it also results in complex arguments and extrapolations which reduces the transparency of the results and makes them difficult to explain to non-specialists (Carpenter 1998). Policy-makers also want clear answers based upon a transparent research process which can be acted upon with confidence. The issues of scaling-up have been debated extensively in ecology but they are also relevant to many other sciences. Medicine provides a close analogy; drug trials are conducted on small numbers of people and the results must be scaled up to the whole population; however, the main difference between the medical sciences and ecological sciences is the extensive collaboration with specialist statisticians. Statisticians are involved in all stages of drug trials including the experimental design, indeed medical statistics has become a discipline in its own right with several highly cited journals (e.g., biostatistics). Collaboration with statisticians at the initiation of an experiment is something that should be incorporated into all ecological investigations; there are moves towards this in ecology but it is not a solution to all problems, indeed the FSE is a good example of a study that has done this explicitly and from the outset of the research.

The issue of scale and the demand for interdisciplinary research requires social scientists to understand the subtle but important differences in scientific research findings that stem from the use of experimental or observational methods. Experimental studies provide the most detailed form of investigation and are essential to determine process. But, large-scale experiments are prohibitively costly and face a great number of constraints. For example, at very large scales it becomes difficult to implement a fully controlled and replicated experiment (Mattison and Norris 2005). In addition, in many landscapes, human influences are impossible to control for and there may be ethical issues if the study is effectively ''using'' human subjects. Observational studies provide a demonstration of actual change in the environment rather than potential for change (Stevens et al. 2004). But, observational studies do not demonstrate causation, and interpretation is potentially subject to confounding variables. However, there are plenty of examples of policy change which lend themselves to observational study, such as the introduction of set aside (e.g., Stevens and Bradbury 2006) and the adoption of organic farming methods (Hole et al. 2005). These changes often embody technological change. The impact of technology and technological adoption on the environment is clear across many scales (Benton et al. 2003). Social scientists almost always employ observational data and it may be that collaboration between disciplines will be easier and more productive (i.e., yield evidence-based research) when there is an observational aspect to the research to provide a common entry point.

Transdisciplinary research presents further challenges as non-academic stakeholders such as government agencies become involved in the investigative process. Not only are issues of scale, communication and trust significant here, but different goals, bureaucracies and working methods as well. If ecological research is really to have policy impact, then transdisciplinary research should be our goal. However, bearing in mind that many disciplines of ecology have still to get to grips with the barriers to interdisciplinary research, this is a considerable further challenge.

Institutional constraints

In addition to the technical difficulties that scale and interdisciplinary/transdisciplinary research have to overcome, there are also some very important institutional issues that need to be understood. Without a broader recognition of these issues and strategies to deal with them, the ability of ecologist to yield policy-relevant research will be limited.

First, many scientists and social scientists working on interdisciplinary research feel that they are not well served by funding bodies, the need to publish in high impact journals and the UK Research Assessment Exercise. Put simply, it is very difficult to get recognition and funding for interdisciplinary work in what is currently a very discipline-based culture. By their very nature, large-scale interdisciplinary projects are conducted by teams (sometimes very large). This can lead to both high costs and lack of recognition at the individual level, which is a problem when many of the current research career structures emphasise individual performance, rather than collective team effort. There is also a trade-off that comes with measuring slow large-scale processes, but these may take longer to yield results and will inevitably be more costly.

Second, large-scale and/or long-term research is not generally considered a top priority and in the past it has not been the kind of research that has received much in the way of support from the research councils. This applies particularly to observational studies. Few would question the value of the research findings that have come out of large-scale studies such as the UK Countryside Survey (Haines-Young et al. 2003), the plant, bird and butterfly atlases (Thomas et al. 2004) or long-term studies such as the Park Grass and Broadbalk experiments at Rothamsted Research Station (e.g., Moss et al. 2004; Blake et al. 1999; Silvertown et al. 2006) and yet obtaining and maintaining funding is difficult.

Moreover, major effects may only come into focus at a large scale or after a long time period. For example, in a catchment liming experiment monitored over a 10-year period, Bradley and Ormerod (2002) were able to demonstrate the effects of liming on freshwater macroinvertebrates and water chemistry. The study was conducted in Mid-Wales using 11 streams with catchments of variable size. Following artificial liming, the chemistry of the streams changed significantly: pH increased, calcium content increased and aluminium content decreased. Despite changes in the water chemistry, the effects on macroinvertebrates were only modest. These changes were only detected for the first 2–3 years of the experiment. It became apparent that acid episodes were still occurring despite the liming. Although these were not as severe as pre-liming, they were sufficient to reduce the minimum pH below that suitable for acid-sensitive taxa. If the results had been reported after 3 years, they would have been very different to those reported after 10 years.

Towards integrated research for policy

The research model of many applied ecological investigations is currently an extremely linear process from the original conception of the idea, through the [scientific] research and generation of results and discussion. Only at the end of the process is there a possible (''bolt-on'') consultation with social scientists or policy-makers or some other end-users regarding the social and economic implications of the findings. We believe that this traditional research model is a very ineffective way to deliver policy-relevant science and that it can be easily improved with more interaction and adaptive feedback between scientists and social scientists. This interaction needs to begin with increased involvement of social scientists in the problem definition and information-gathering stage of the research process (Sutherland et al. 2006). Conversely, there also needs to be more involvement of science in policy design. A more integrated and interactive research process, where scientists and social scientist work through the investigative process together, will enhance efficiency and minimise risks from, for example, missing information. This means that in contrast to the current linear research model, an approach is needed where scientists and social scientists work in tandem, consulting with each other regularly and with purpose. Although consultation between scientists and social scientists can be a difficult process, especially at the outset of a research project due to the different disciplinary approaches to formulating questions and hypotheses, it is clear that early consultation could save considerable time and wasted effort later. Making interdisciplinary research fit for the purpose of tackling complexity and uncertainty requires natural and social scientists to extend the tool kit of methods including greater exploration of techniques such as expert opinion, output-driven modelling and scenarios (Sutherland 2006).

All disciplines need to be aware of these difficulties and be prepared to cooperate for the collaboration to be successful. As we have already noted, there is a need for careful choice of language and a willingness to discuss meanings and terminology well beyond simple semantics. With a little patience and open-mindedness and by recognising all other research partners and their relative expertise, communication barriers and disciplinary ignorance can be overcome. Through this, mutual trust can emerge. Internationally, there have been some very successful examples of ecologists and social scientists working together. The Iowa landscape project is an example of such a success (Santelmann et al. 2004); the project investigated alternative landscape scenarios with the goal of informing decision-makers. The different scenarios placed varying levels of emphasis on drivers such as water quality, biodiversity and economic profitability. The project brought together experts from agronomy, plant and animal ecology, wetlands ecology, water quality, hydrology, agricultural policy and geographical information systems in order to evaluate scenarios for their impact on different environmental and socioeconomic drivers. Changes in driver response were reported as a percentage change from the current situation in order to compare multiple endpoints. The project produced results indicating that a scenario designed to enhance biodiversity could cause very little reduction in profitability, be acceptable to farmers and improve water quality as well as enhancing biodiversity.

In Europe, the EU Framework research programme has to a degree embraced the need for interdisciplinary and policy-relevant research (Bruce et al. 2004). The Fifth Framework EU-funded project, BioScene (Mitchley et al. 2006), took an explicit interdisciplinary approach to the relationship between agriculture and biodiversity in six European mountain study areas to provide recommendations for reconciling biodiversity conservation with social and economic activities through an integrated rural development strategy. BioScene employed scenario analysis and stakeholder participation as tools for structuring the analysis of alternative mountain futures. BioScene brought together ecologists, economists, sociologists and rural geographers to carry out interdisciplinary analysis of the scenarios: identifying key drivers of change, assessing the biodiversity consequences and evaluating cost-effectiveness. BioScene used a sustainability assessment to integrate the research outputs across natural and social science disciplines to assess the broader sustainability of the scenarios in terms of biodiversity, natural resources, rural development, social development, economic development and institutional capacity. The sustainability assessment showed that in addition to delivering key biodiversity goods and services, an explicitly conservation-orientated scenario was potentially the most sustainable of the three BioScene scenarios. Through the reconciliation of potentially conflicting objectives, such as conservation, economic development and human livelihoods, and with a strong participatory planning approach, a biodiversity lead scenario could represent an alternative strategy to current ‘‘business as usual’’ economic-lead scenarios for sustainable rural development in Europe’s mountains.

This process is beginning in some fields of research in the UK, for example, the phosphorus co-ordination project funded by DEFRA draws together scientists and policy-makers looking at the sources, transfer processes and impacts of phosphorus pollution in streams, lakes and rivers (Haygarth 2005). This initiative is fostering close links between scientists and policy-makers as well as drawing in other disciplines in order to address the questions policy-makers need answering. This direct communication between policy-makers and researchers provides a strong basis for evidence-based policy to become a reality. This type of forum (together with systematic review papers) also helps address the problem that scientific research is frequently too dispersed amongst scientific literature to deliver a clear message to policy-makers (Sutherland et al. 2004).

Regarding scale, we acknowledge there is nothing fundamentally new in identifying the importance of scale as an issue for ecological research but it is clear that scale is a critical factor if research findings are to yield policy-relevant results. Identifying the appropriate temporal or spatial scale will depend on the nature of the problem. Given finite resources (and certain other ‘‘institutional constraints’’ referred to above), there will tend to be trade-offs in experimental design, information generation and domain of relevance. However, if policy is to be usefully informed then there needs to be confidence in the results. It is fairly obvious that an experiment that does not give statistically valid results due to insufficient replication is not good value for money. Equally then, is it a good use of resources to fund an applied research project that fails to address a policy issue because it asks the wrong question or is implemented at too small a scale? We would argue that it is not and that after so many years of following a standard funding model that emphasises short-term returns on typically three years support for a single researcher within a single discipline, it is time to review some of our institutional structures. There may be lessons to be learnt from other disciplines such as physics and astronomy where the large cost of experimentation is accepted and research priorities are decided with the backing of the whole community. A primary example of the cooperation that has been achieved in these disciplines is CERN, the European Organisation for Nuclear research which permits some of the world's most advanced physics experiments with the costs being distributed among many funding bodies in 20 member states (CERN 2005). A call to formulate biodiversity research along these lines has recently been made by Kremen (2005). Internationally, there are a number of examples of schemes that address the need for large-scale or long-term research and integration between policy-makers and scientists. The US National Science Foundation has funded several programs that directly address some of these concerns including the Long Term Ecological Research Network which aims to maintain long-running ecological experiments, the Water and Watersheds program which supports interdisciplinary science and engineering research with the aim of supporting decision-making and management, and the Methods and Models in Integrated Assessment which takes an interdisciplinary approach to tackling global environmental change. Of course, supporting more large-scale, long-term interdisciplinary activities may mean fewer small-scale, short-term investigations but we need to evaluate return on investment and consider net benefit and not just cost.

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