Environmental and Resource Economics **11(3–4):** 383–397, 1998.

Research Trends and Opportunities in Environmental and Natural Resource Economics

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Abstract. The research questions and topics most likely to emerge in the near term future are assessed. A common theme is that policy issues will be an important driving force, as has generally been true in the past. More specifically, future theoretical advances are expected to occur in the treatment of uncertainty, the incorporation of stock service flows into natural resource analysis, and the incorporation of institutional considerations into models of resource exploitation. Research on valuation is expected to remain vigorous, primarily in the testing of basic assumptions and reconciliation of existing inconsistencies. Opportunities in renewable resource economics center on the incorporation of richer behavioral and technological detail in the general frameworks that already exist. A better understanding of what drives technology, and how environmental agreements can be negotiated and enforced among sovereign nations, are two topics likely to shape future research on global externalities. Finally, questions related to spatial aspects of natural resource use, and matters of land use more generally, seem likely to emerge as important topics on the profession's future research agenda.

Key words: economics, environment, natural resources, research

JEL classification: Q2, Q3, Q4

1. Introduction

Assessing where the most promising opportunities are to be found in any field of research is an obviously speculative venture, though one that younger members of the profession might arguably find useful. The co-authors of the present paper, all U.S. environmental and resource economists, contributed topics independently. These topics naturally reflect the individual interests and specialties of the contributors as well as our particular institutional and economic circumstances. Some topics that lie in research specialties we do not cover, or that arise in different settings, may well have been overlooked.

A common theme is that research on natural resource and environmental economics will be driven by policy questions. This is not surprising, given the

growing importance of environmental and natural resource issues in government policy agendas, and the fact that economists often consider their role as advocates for efficiency in any setting. Evidence on past research trends, reported shortly, seems to show a correlation with policy questions, but this has not been examined rigorously. An emphasis on policy questions might seem to imply a bias toward empirical research and away from theoretical work, but this is not necessarily the case. The topics that follow include numerous examples of each genre.

We set the stage by examining how the research agenda of environmental and natural resource economics has evolved over the last 25 years, as indicated by topics published in one journal. Discussions of research opportunities in broad areas then follow.

2. The Past as Prologue: Research Trends Since the Early 1970s

Environmental and natural resource economics has emerged as a mature field of research over the last three decades. Examining its brief history to date can provide a context for the explorations of future research opportunities that follow. The proceeding discussion examines the research topics from one academic forum, the *Journal of Environmental Economics and Management* (*JEEM*).

Between its inauguration in 1974 and the end of 1996 *JEEM* published over 850 articles. Each paper in this collection was categorized according to research area and Table 1 reports the overall topic breakdown for three sub-periods.[1] The first line shows that the annual page count nearly doubled between the first and third periods. Considering also that the number of environmental and resource economics journals increased over the period, research in this field clearly has grown rapidly. Lines 2–7 show the percent of published pages by major category. When interpreting these data it is important to realize that a single article may address several topics. The environmental management category has consistently averaged more than 40% of total pages. The institutional/policy issues, renewable resources, and nonrenewable resources categories combined represented about 50%, with international/global and agricultural issues comprising the remainder. Overall, the page distribution across these categories was relatively constant.

The article count also reveals some interesting trends within the broad environmental management category. Lines 8–10 report page shares for three of the most important sub-categories: ‘pollution/waste control’, ‘valuation methods’, and ‘damages/benefits’. Forty percent of the 265 articles on pollution/waste control, which includes the economics of abatement and cleanup of air and water pollution and residual materials, had a policy instruments focus. Although this sub-category dominated the environmental management total, as time progressed this area diminished, and the ‘valuation methods’ and ‘damages/benefits’ sub-categories grew significantly. Together, the latter two account for ‘valuation’ research in general. Growth in the ‘damages/benefits’ share, from 18.6% in the early period to 26.2% in the final period, largely resulted from research on ‘recreation’. Research

Table I. Trends in environmental/natural resource topics published in *JEEM*.

Time period:	74–80	81–88	89–96
Total pages published per year	363	404	681
1. % Environmental management	46.6	42.0	43.5
2. % Institutional/policy issues	21.6	19.8	20.0
3. % Renewable resources	13.1	17.8	17.8
4. % Nonrenewable resources	14.6	13.5	11.5
5. % International/global issues	2.8	2.1	4.5
6. % Agricultural issues	1.4	4.8	2.8
Environmental management category			
7. % Pollution/waste control	59.5	39.9	27.7
8. % Valuation methods	10.8	21.4	27.5
9. % Damages/benefits	18.7	19.7	26.2
10. % Env. management, other	11.0	19.0	18.6

Note: The research sub-categories within each major category are:
Environmental management: pollution and waste control options, risk and uncertainty, valuation methods, damages and benefits, enforcement issues.
Renewable resources: forests, marine and coastal resources, fisheries, water resources, land use, biodiversity and wildlife, sustainable development.
Nonrenewable resources: energy, minerals, issues in growth, intergenerational equity, and discounting.
Agriculture: non-point source pollution, land use, sustainable agriculture.
International/global issues: international trade, climate, population, development, transboundary externalities.
Institutional/policy issues: environmental policy instruments, externality theory, environmental and resource law, political economy, property rights, environmental accounting, benefit-cost analysis, federalism.

on ‘valuation methods’, which emphasizes methodological developments, grew even more rapidly beginning in the mid-1980s. Sub-topics within the institutional/policy issues category were also examined. The dominant topic in this category is ‘environmental policy instruments’, accounting for 43% of the total. The remaining topics comprise the balance and exhibit no clear trends.

To the extent that research published in *JEEM* reflects trends in research interests of the environmental and natural resource economics profession, certain conclusions follow. First, the allocation of research effort among broad categories has been generally stable. Second, the mild trend away from nonrenewable resource issues, and toward renewables and international/global issues, seems broadly consistent with policy developments, in the U.S. at least. Third, research interest within the environmental management category has been dynamic, with growth in valuation related research the most notable feature.

3. Emerging Theoretical Issues

The evolution of research in any discipline is often shaped by the introduction of new theoretical tools or shifts in the way key processes or relationships are perceived. Although forecasting such events is clearly hazardous, a few possibilities seem particularly likely.

First, economics largely regards human behavior as 'black box' maximization within constraints. While this view has yielded important insights, it may be inadequate or even deceptive in the long run and in periods of dynamic change. It seems especially ill-suited for analyzing interactions among economic development, population growth, and the use of resources and the environment, particularly in very long run contexts. While there is growing agreement on the importance of not sacrificing safe and attractive environments in the process of pursuing economic development, there is little in the way of concrete knowledge of how to accomplish this. It does seem clear, however, that persuading a government to adopt a particular green tax, or limit access to a specific resource, is not the key. Rather, the answers seem likely to depend on factors such as education, governance structures, and the evolution of formal and informal social institutions. The forces that determine these factors, the reasons for their marked variation across nations, the instruments available to alter them, and the way they combine to influence economic growth and the way the environment is used are poorly understood at present. A better understanding may require the profession to focus more on historical and institutional considerations than it normally does.

At a lower level of abstraction, the discipline now seems poised to make valuable advances in the way human interactions with the environment are incorporated in the existing paradigm. One concerns the treatment of uncertainty, and the increasingly evident need for environmental and natural resource economics to go beyond 'certainty equivalence'. In part, this is because researchers are coming to realize the importance of looking at measures of variability, or at the tails of distributions, and how these may change over time, rather than just at central tendencies. A recent example of this shift in focus is Yohe's (1993) extension of the Nordhaus model of the optimal control of greenhouse gas emissions to study the implications of correlated 'low likelihood tails' of distributions, say of temperature change and impacts. It is also germane to recognize that these distributions can and do change over time, so the decision made at a point in time should take into account the prospect that later information may dictate a different decision. Here, one seeks to know how this *prospect* of new information will affect the initial decision. Dixit and Pindyck's (1994) recent work on investment under uncertainty and financial options has provided a major impetus for this change in approach to uncertainty. Some of the essential insights were, to be sure, already in place, developed independently in environmental economics many years ago. What is new is the increasing use by environmental and resource economists of the concepts and methods of stochastic calculus, made more accessible by Dixit and Pindyck.

This can be seen in recent work on climate change. Most economic analysis to date has focused on the relatively modest change in average conditions, e.g., mean global temperature or average precipitation in a region, expected to result from the benchmark doubling of greenhouse gas concentrations. Not surprisingly, perhaps, rather modest impacts have been estimated. But at least a few economists have argued that changes in variability, e.g., in the frequency and intensity of storms, droughts, and heat waves, are likely to matter more for human health, agriculture, fisheries, and unexploited ecosystems than changes in average temperature or precipitation. The stochastic calculus of diffusion processes and jump processes is proving to be an attractive way to represent climate events, and changes in the underlying processes that drive them (Fisher and Rubio 1997). The same techniques are being used to study a range of other environmental and resource problems, such as the decision of whether to cut old-growth forest (Conrad 1997), and it seems likely that the field will continue to move in this direction.

A second area where a general shift in emphasis seems increasingly important is integration of the *in situ* environmental benefits of natural resource stocks into optimizing models of natural resource use. To a large extent, research on efficient natural resource extraction has focused on maximizing extractive values net of direct extraction costs over time. Extraction often impairs environmental, ecosystem, and other non-market services, however, and these effects can be critically important to the optimal extraction decision. Recognition of these effects certainly is not new to the literature or to policy-makers. The associated analysis seldom focuses on questions of intertemporal resource allocation, however.[2] Since both the extractive and environmental components of many resource decisions have important dynamic features, both should be considered simultaneously. Hartman (1976) made this point by integrating the use value of a forest into the harvest decision, but few others have followed this integrated path.[3] The fundamental insight is that the flow of ecosystem and environmental services is determined simultaneously with the flow and stock of the resource. As a consequence, the impact of environmental considerations can be far more complex than making a dichotomous choice between conservation and extraction. Moreover, accounting for the complex dynamics of ecosystem services is likely to amplify the importance of flow considerations. Ragozin and Brown (1985), for example, show that predator-prey dynamics can have significant impacts on optimal harvest policy in commercial fisheries. More generally, any environmental or ecosystem service provided by a natural resource stock can have important dynamic dimensions. For example, a given forest ecosystem and its associated environmental services might be less disturbed over the long term by occasional, intensive timber harvests than by the constant disruption of steady extraction. Here, a cyclical extraction pattern may well be optimal.

The temporal dimension of resource use is not the only one that matters. As discussed in section 7, the spatial pattern of natural resource use may affect the value of environmental service flows in ways that existing analysis often ignores.

Clearly, important research opportunities lie at the nexus of natural resources and the environment.

4. Valuation

It is doubtful that anyone could have anticipated the explosion of research on non-market valuation that has taken place in the last 25 years. A quick summary of where we stand might suggest that the 'heat' has not necessarily generated 'light'. Basically, the profession is not yet entirely comfortable with the numbers it is producing.[4] Discussing some sources of discomfort for each method can help identify research opportunities.

The contingent valuation (CV) method is one of the most controversial topics in non-market valuation. The mainstream of the economics profession seems unconvinced that CV can provide an accurate basis for valuing environmental amenities. In 1993 a distinguished panel of social scientists chaired by Kenneth Arrow and Robert Solow reviewed the CV literature available at the time. They proposed a set of requirements for CV results to be considered a credible starting point in the determination of the monetary damages for injuries to environmental resources (see the *Federal Register*, January 15, 1993). Their conclusions have not reduced skepticism. It is difficult to pinpoint the reasons. Two elements seem to be especially important. CV questions, however they are phrased, are *hypothetical*. As a result it is argued by some that they can be reliable indicators of preferences, but may not be consistent with people's financial constraints. Equally important, several economists have questioned consumers' ability to make consistent and reliable choices with any new good (Lucas 1986). As these concerns apply to both real and hypothetical transactions, CV applications combine both sources of discomfort. Clearly, any method used to determine amounts *actually paid* in very large, complex claims will be closely scrutinized. Setting this aside, the effects that consumer experience and hypothetical questioning have on CV responses deserve more careful research attention than they have received to date.

A number of generic temporal and cross-commodity substitution issues arise when using observed recreational choices to value environmental resources. The value a consumer places on a change in the quality of a resource depends both on the alternative sites available and on the time and income available to use them. The interactions involved in consumer decisions in this realm can be complex. Free time and income interact through labor supply decisions, spatial dimensions of resource availability determine implicit prices of access, and time can serve as a constraint, a measure of use, and an index of amenity conditions. Efforts to deal with the temporal dimensions of choice inevitably confront all of these complexities.

Economists commonly represent preferences with a weighted sum of instantaneous utility functions, with each period's utility depending only on consumption in that period, and this raises a more specific substitution issue. Such separability implies that a consumer's marginal rate of substitution between, say, trips to the

Grand Canyon and generalized income is independent of whether or not the Grand Canyon was visited last year. Introspection suggests that this is incorrect. Over relevant price and income ranges, many people choose to visit the Grand Canyon exactly once during a lifetime, but seem largely indifferent about exactly when the trip is made. This type of intertemporal substitution is potentially important for valuing temporary amenity losses, e.g., beaches temporarily oiled, campsites temporarily closed by fires, or recreational fisheries temporarily closed by regulations. In each case the consumer can substitute more intense use at another time for foregone use during the closure, and this has implications for estimating welfare effects that standard methods do not capture. To date, such questions are largely unaddressed.[5]

Recent advances in the travel cost paradigm for valuing recreation sites rely on random utility models (RUM) to examine how the quality characteristics of a group of distinct sites affect the demand for each site (see McFadden 1974; Hanemann 1978). Even the most general RUM framework fails to offer a convincing description for the number of visits an individual takes over a given time interval. This is an important gap because recreation benefits clearly depend on the frequency of individual visits.

To date no hedonic model with site specific environmental amenities has successfully estimated the second stage marginal willingness to pay function. While the identification requirements for simple versions of the model are arguably well understood (McConnell and Phipps 1991), a new set of questions has emerged. Hedonic models normally take environmental attributes as given and rely on market equilibrium to identify tradeoffs. Selecting a place to live determines both private housing attributes and local public goods, however, so general equilibrium effects presumably extend to such endogenous local amenities (Epple and Seig 1997). Over some time scale, the operation of housing markets and local collective choice processes interact to determine to equilibrium transactions summarized in the hedonic price function, a complication just now being recognized.

General equilibrium questions appropriately receive emphasis when the scale of environmental policy change is large relative to the economy. There is, however, a curious inconsistency between the assumptions required for revealed preference valuation methods and the assumptions incorporated in general equilibrium models for policy analysis. The former clearly require links between market goods and environmental resources in order to recover consumer values. In research on the social costs of environmental regulation, however, environmental services are assumed to be separable from market goods.[6] The same assumption is maintained in the evaluation of eco-taxes and the double dividend hypothesis (Bovenberg and de Mooij 1994). In simple terms, the separability assumed in policy models undercuts revealed preference valuation methods.

Finally, it seems appropriate to acknowledge that valuation research has to date focused mainly on a ‘developed country’ context. In this context valuation issues tend to be preference-based, involving the study of income and substitution effects

in behavior related to non-market goods in order to learn about preferences. In developing economies, by contrast, environmental values are tied more closely to the use of resources to generate income that sustains life. In this setting resource conversion decisions emphasize the input values of resources (Dasgupta 1996) rather than amenity values. Both the methods appropriate for measuring contributions to well-being, and the institutions required to assure that economic values are reflected in private and public choices, differ in important ways in these two contexts. On balance, it seems that the valuation questions most salient to the developing world are relatively under-studied.

Valuation research over the last 25 years has been dominated by applications, rather than testing and refining of the basic theory. The most valuable future contributions are likely to emerge from research programs that identify specific gaps or inconsistencies in the current state of the art, and develop empirical or theoretical strategies that will close them.

5. Renewable Resources

Reviewing the contributions made to renewable resource economics over the past 25 years, it is hard to ignore the long intellectual shadows cast by H. Scott Gordon's (1954) and Anthony D. Scott's (1955) papers, both published over 40 years ago. Gordon's paper was mostly predictive, offering a simple model of the rent dissipation process under open access. Scott's was primarily normative, asking: how should society manage renewable resources? Both papers specifically dealt with fisheries, but their central lessons applied to renewable resources more generally. These two papers formed the foundation for a vast literature on renewable resource economics that flourished in the 1970s and 1980s. They also contributed important insights to environmental economics more generally.

Perhaps the most prominent development in renewable resource economics over the last three decades is the use of sophisticated dynamic models capable of addressing the optimal management question raised by Scott. For the most part this literature reiterates and reinforces Scott's earlier conclusions – that resources are optimally conserved when they are utilized to maximize the present value of rents, and that rents are optimized when current profit opportunities are balanced at the margin against the marginal user cost of those gains. In hindsight, elaborating the basic conditions for optimal dynamic resource use absorbed an enormous amount of intellectual effort for a payoff whose practical importance has been relatively small. In fisheries, managers are virtually never concerned with getting biomass stocks close to dynamically optimal long run levels. Instead, fisheries managers raise questions like: how will the industry be affected by trip limits, mesh size changes, or limit entry? How will bycatch and discards be affected and is the biomass safe from stock collapse? Significantly, many of these 'management' questions are predictive rather than normative and closer in spirit to Gordon's focus. Ironically, they remain largely unanswered because economists chose to emphasize the optimization problem Scott posed instead.

The problems that seem most vexing to renewable resource managers today, and which remain largely unmined by the profession, have common characteristics. First, many are 'what if' questions that address the likely configuration of resource using industries before and after implementation of some policy. Second, most require detailed attention to the resource, the exploitation technology, and the formal and informal institutions governing use. Finally, much of the information managers need is empirical – measurement of key relationships and judgment about the sizes of various impacts. All of these practical questions offer significant research opportunities for economists, opportunities to make fundamental conceptual advances, to investigate interesting empirical issues, and to add value to the actual management process. The more detailed discussion that follows is aimed toward fisheries, though many of the points apply more generally. It is organized around the three 'agents' in such problems, the exploiting firm, the regulator, and the exploited resource.

First, the nature of the resource exploiting firm has not been investigated adequately. Our characterization of the exploiting firm under ideal conditions – the single owner managed fishery, for example – is relatively complete. By contrast, we know very little about how firms make decisions in the bulk of real world settings, namely those involving open access, restricted access, or a regulated variant of these conditions. Gordon made a start in the open access setting with a simple, single input model, but how are rents dissipated across multiple potential margins in a multi-input technology? Most existing literature assumes firms are 'input efficient' under open access, and that the dissipation process results in too many firms. At the same time, however, other researchers posit excessive capital, or 'capital stuffing', with other inputs used in correct proportions. These inconsistencies point to the fact that we do not have a well accepted theory about decision making by technologically flexible firms under open access.

Second, it must be recognized that resource exploitation under pure open access conditions is now rather rare. Since the extension of national jurisdictions over coastal waters in 1976, most of the world's fisheries have come under the legal domains of coastal nations and most of these nations regulate use in some fashion. This raises questions about the motivations and behavior of the regulators: What governs how regulators select and apply instruments? How do firms react, and offset regulations such as reduced season lengths, trip limits, input controls, mesh and other gear regulations? Should we model regulated resource use as a process in which the behavior of all parties is endogenous? Despite the dominance of regulated systems in the real world, these questions are only now receiving attention (Homans and Wilen 1997).

Third, the profession would gain both credibility in the policy process and better insights about how these resources behave by adding more realism to our biological models. Economists have largely stuck with simple paradigms that most biologists regard as useful pedagogical metaphors, but of little practical value. Fisheries biology has progressed well beyond the simple lumped parameter representations

of Schaefer, Ricker, and Beverton and Holt. Models developed for actual management often contain age and/or size structure, gear selectivity, sex differentiation, and spatial dimensions of population abundance and processes. Economists have paid little attention to these complexities, partly because they muddle the rather simple paradigms we have successfully used to gain fundamental insights. If we are interested in being useful beyond offering simple generalities, however, then it is time to begin incorporating more of the realism that already exists in the biological literature into our models.

Expanding our useful understanding of how renewable resource industries operate will require learning more about the behavior of harvesters and regulators and the resource itself, specifically about the details we are prone to suppress when seeking analytical insights from simple constructs. Stylized, optimization-based models probably have already yielded their most important insights. Significant progress in the future will require a more complete understanding of real world circumstances.

6. Global Externalities

It is increasingly evident that the damages from several forms of environmental pollution are international in scope, and this complicates the control problem substantially. Considerations of sovereignty imply that voluntary international agreements will be important policy instruments. The convention on global warming is a good example but there are many others, including treaties on trade in endangered species, CFCs, and biodiversity. Here, the existing literature points to a rather unsatisfying result – that voluntary agreements will not work unless the stakes are small, i.e., the treaty has little effect on the equilibrium outcome (Barrett 1994). Some authors argue that countervailing tariffs can be used to enforce such agreements. Given the importance of the topic and the limited state of our current knowledge, further work on this area seems both likely and desirable. It may be possible to apply cartel theory to this question. Cartels share many of the same problems, principally the necessity of relying on self-enforcing rules because third party enforcement by 'the state' is unavailable. Natural questions are: What credible enforcement mechanisms can be developed? Is it effective to partition a large number of players into a set of separate coalitions, each consisting of similar players? What conditions are necessary for an international environmental agreement to be effective? The control of one specific type of transboundary pollutant, greenhouse gases, is made more complex, and possibly more important in the long run, by the fact that it accumulates as a stock. Here, the problem of obtaining agreement among different nations is compounded by the necessity to balance the interests of present and future generations.

On the general question of climate change, there are several glaring gaps in our knowledge. Little is presently known about the potential damages from climate change. There has been much speculation, and rough quantitative assessments

have been obtained from simulations. There has been very little econometric work, however, to shed light on the costs or benefits associated with different climates or on the costs of adjusting to different climate regimes. The analysis of land rents by Mendelsohn et al. (1994) was an important step forward, but it has not been followed by much in the way of theoretical or empirical refinement. Consequently, the damage function for climate change remains largely unknown, or at best highly uncertain.

The role of technological change in the climate change problem is another area where our knowledge is primitive. Policy makers worldwide and in the U.S. are putting a great deal of faith in being able to invent our way out of the climate change problem. Just how does innovation respond to incentives, such as a tax on carbon? This suggests a number of research questions. It is of course impossible to know just what innovations may be coming our way. However, posing the policy question "will technology bail us out?" illustrates how little we know about the way relative prices induce technical change, or even the adoption of existing technologies. Addressing even preliminary research questions in this arena would be useful. For example, what is the best way to model the innovation and technical change that would be induced by policies that make carbon intensive fuels more expensive? How much technical change has been generated by past policies that have made it more expensive to pollute, such as the U.S. sulfur allowance program? Clearly, a number of fundamental economic questions remain unanswered in this realm, and a lack of answers is stymieing practical policy solutions.

7. Spatial Relationships and Land Use

Spatial considerations, such as spatial interdependence and spatial non-convexities, are important features of many ecosystems and hence are salient for the design of policy. The habitat value of a forest area generally depends on whether it is fragmented or continuous. For a continuous patch, habitat value may depend on general aspects of its shape, such as the ratio of boundary length to patch area. Such factors are potentially important for policy regarding timber harvesting in tropical and temperate forests, with the preferred harvest policy depending on the particular ecosystem dynamics of the region. Similar considerations apply to policy regarding agricultural land use, particularly the potential for conflicts along borders of different uses.

The spatial dimension of resource use may turn out to be as important as the exhaustively studied temporal dimension in many contexts. Curiously, the profession is only now beginning to move in this direction. Numerous basic questions remain to be addressed regarding both urban and natural land use.[7] Urban and transportation economists have, of course, studied the spatial allocation of resources in urban settings. They have not focused on how the spatial configuration of economic activity and land use determines the overall value of a city's environmental amenities, however. At present the policy debate on such questions

is dominated by the work of landscape and systems ecologists. Their land use models, developed to guide empirical analysis and simulations, often lack rigorous economic reasoning and frequently ignore the operation of property markets when addressing policy questions (Bockstael 1996). Infusing more economic content into this effort could have a high payoff. In natural settings, there are numerous spatial research questions worth addressing. What economic factors determine continuity versus fragmentation in land use, at both theoretical and empirical levels? How do habitat values depend on the spatial configuration of land in different uses, e.g., whether forests and urban areas are contiguous or separated by agricultural uses? How does the spatial configuration of land use respond to economic forces, and what policy instruments are available to influence it?

It is also worth emphasizing that land use in the U.S. has historically been heavily affected by public policy, a fact that points to additional research opportunities. Government incentives for rapid and large-scale exploitation of natural resources have favored use of materials in general, and more specifically favored the use of virgin over recycled materials. The resource extraction that results often affects land use and the value of habitat services in important ways. The same is true of the numerous agricultural subsidies embedded in our farm policies, including government subsidized water in the arid west. To date, economic analysis of the efficiency effects of such policies has largely focused only on the 'private' costs and benefits captured in market prices. The possibly vast environmental costs or benefits associated with the land use consequences of these policies have received far less detailed study.[8]

8. Conclusions

Little would be gained by summarizing the preceding discussion at this juncture. It seems more useful instead to comment on the general scope and approach to research in the field. When the foundations for environmental economics were being laid in the 1960s, Ayres, d'Arge, Kneese, and others borrowed concepts from the physical sciences to represent the essential physical realities concerned with resource use and pollution generation. This view yielded certain direct insights, e.g., that considering problems of air, water, and solid waste pollution in isolation is insupportable. It also served as an initial framework within which research could proceed.

Early large scale, multi-disciplinary, research efforts, principally at Resources for the Future, sought to quantify the key physical relationships involved in environmental degradation across an entire set of economic activities on a regional basis. The extensive study of the heavily industrialized and populated lower Delaware River valley is an example. It was soon apparent, however, that key pieces of information needed to translate physical data into useful policy advice were missing. Estimates of the marginal costs and marginal benefits of pollution control were largely unavailable and methods for obtaining them did not exist. Knowledge

of the incentive properties of alternative environmental policy instruments, at either conceptual or empirical levels, was rudimentary. The special problems associated with controlling cross boundary pollutants had yet to be addressed.

Research over the last 25 years or so has largely been devoted to these more specific questions of benefits, costs, and incentives, and has addressed them primarily at the level of broad conceptualization. While no one is content with the current state of our understanding of the relevant concepts, significant progress has been made. This suggests that it may be appropriate, either now or in the near future, for academic researchers to become more directly involved in the policy implementation process. Without direct involvement it seems unlikely, at least in the U.S., that state-of-the-art knowledge on benefits, costs, and regulatory instruments will be reflected in actual policy choices. Such work would likely differ from most policy-directed research presently ongoing in matters of scale, integration with other disciplines, and orientation toward providing actual policy proposals rather than generalized advice.

The profession's current orientation toward developing new methods and testing hypotheses, with economists largely working in isolation on research questions of their own devising, will and should continue. The questions that more directly concern actual policy makers are usually more practical and more integrative, however. Addressing them directly, in all their untidy complexity, would arguably provide more of the advice actually needed. Such work would also, no doubt, spin off interesting conceptual questions that presently are overlooked.

If the research we do is largely motivated policy issues, as claimed in the introduction, then it seems clear that the profession should get more heavily involved in practical policy implementation than it is at present. Such involvement can have an important impact on policy outcomes. Indeed, some observers attribute the success achieved by the SO_2 trading program and the RECLAIM program in Los Angeles to the direct involvement of academic economists in the policy implementation process. Also, the broad trend toward transferable quota systems for managing fisheries has received important impetus from the policy work of academics. If this general assessment is correct, then a challenge for environmental and resource economics is to begin thinking more thoroughly about the practical aspects of environmental and natural resource questions, and to raise the place of rigorous applied research in the academic preference order.

Acknowledgement

Thanks are due to Mike Toman, Paul Portney, and an anonymous referee for helpful comments.

Notes

1. The major classifications are: environmental management, renewable resources, nonrenewable resources, agricultural issues, international/global issues, and institutional/policy issues. Each classification contains sub-classifications, and each article was assigned up to three of these more detailed categories. Data for individual articles were multiplied by the count of published pages for each article to arrive at data on pages published on individual research topics. In cases where an article covers more than one detailed topic, each topic is assigned an equal share of the article's total count. Annual totals and totals for major classification categories were then computed by aggregating appropriately.
2. At least since Krutilla's (1967) 'Conservation Reconsidered', environmental economics has been concerned with the environmental and ecosystem costs of natural resource use.
3. Two recent examples are van Kooten et al. (1995) and Olsen and Shortle (1996).
4. A notable recent example can be found in the U.S. E.P.A.'s retrospective assessment of the net benefits from U.S. air pollution regulations during 1970–1990. Their analysis relies largely on revealed preference methods and finds the present value of benefits to be about $22 trillion (1990 dollars.) If one believes this number and considers the policy to be an asset, then it provided U.S. households with a return equal to about 25% of their personal income over this period. Most observers believe this appraisal does not pass the 'laugh test'.
5. Browning's (1991) 'simple non-additive preference' structure represents one possible formulation.
6. Examples are Hazilla and Kopp (1990), Jorgenson and Wilcoxen (1990), and Stokey's (1998) recent evaluation of the theoretical consistency of environmental Kuznets curves.
7. Bockstael (1996) urges more emphasis on spatial questions. See Albers (1996) for an application.
8. Stavins and Jaffe (1990) is a notable example.

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