Introduction

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Environmental and natural resource economics is a young sub-area of economics compared to others, such as international trade, industrial organization and public finance. Young though it may be, however, one could argue that the diagnosis of the problems at its heart has been made and most of the public policy prescriptions have been written. It may be, as has also been argued, that the patient too often chose to ignore the doctor's advice, but that does not contradict the claim that this area of economics has come a long way in a short time to establish a secure theoretical paradigm and a set of policy-relevant research results.

To elaborate, we have long understood the principles of market externalities and market failure that underlie the misallocation of environmental resources at a point in time. Recently, scholars have shown increasing interest in intertemporal resource misallocation, prompted by the appearance of the term 'sustainability' in the mid 1980s. Although the term itself continues to defy clear definition, if it is given some clear definition as a starting point, analysis can typically be pushed through to yield interesting insights. To the extent that the growing environmental 'problem' traces its root cause less in market failure and more in the twin pressures of population growth and economic development, again the diagnosis is clear even if implementation of policy prescriptions might prove difficult or impossible.

All this is to argue that environmental economics is a 'normal science' with the advantages and liabilities that this entails for researchers. Its accepted paradigm is the product of the research of preceding generations of academics, which guides the research of the next. Yet research in a normal science is surely less original, less exciting and perhaps less enervating, than pre-paradigmatic research. One imagines that for a normal science, all the really important academic work has been done, the Nobel Prize winners named (even if history still keeps their names secret) and that the field is on the declining side of the marginal product curve.

This may indeed be true, but we the editors believe that environmental and natural resource economics has yet to tackle some very important research

topics and that it will be a growth market for years in the future. Our belief motivates this book.

The objective of this book is to focus attention on areas of environmental and natural resource economics and management that we believe have received insufficient research attention and thus should constitute fertile areas for future exploration. We have tried to bring together in this volume a mix of eminent European and American scholars to provide a research agenda and to stimulate cross-fertilization within and across various disciplines like economics, geography, biology and ecology. The language and the methodology common to each chapter are economics, yet all chapters are accessible to a wider audience than academic economists. The papers are original research contributions, but they are less developed in a mathematical or empirical sense than would be expected for major journal publication. We hope they may reach researchers and policy makers in the public sector, professional staff in research institutes and think tanks and consultants doing environmental work with either public or private sector clients.

Although we feel that environmental and resource economics is mature, our belief that it will be a future growth market provokes some questions. Why might some areas have been under-researched? What were the barriers to progress? Where are the specific growth areas of the future? In the collection of chapters that follows, we can see some answers to these questions.

First, one might attack the proposition that the field is mature; that it has an established and adequate paradigm. Most economists, however, would probably accept that this is so, but, as the reader will see in several of the following chapters, this view is not unanimous. Obviously, an attack on the basic paradigm would open up a vast research agenda. Some of the chapter authors are sceptical of the existence of a paradigm (see Ayres's chapter on industrial ecology); others of its adequacy in light of unsolved problems (see Kaufmann's chapter on ecological economics). The main attack on the paradigm comes from ecology, but Kaufmann reminds the reader that in science, as in poker, something always beats nothing. That is, if ecology or ecological economics are to take the research agenda forward, it must be by demonstrating empirically that they perform better than their neoclassical economic rival.

Many of the following chapters see future research opportunities in interdisciplinary work; for example, joint work between environmental economists and ecologists (see Kaufmann, Ayres, Perrings), organizational theorists (see Gabel and Sinclair-Desgagné), engineers (see Ayres), behaviouralists (see Kunreuther), biologists and geographers (see Crocker and Shogren).

Just as interdisciplinary opportunities for new research lie unexploited, so too do opportunities for more intra-disciplinary research. These opportunities can take at least two different forms. One is between different sub-areas of

economics. Examples include research that links regional and urban economics, the ‘new economic geography’, and environmental economics (see Gerking and List); the integration of organizational economics and environmental economics (see Gabel and Sinclair-Desgagné); and technological change, R&D and endogenous growth (see Carraro).

The second form of intra-disciplinary research brings to environmental economics methodologies previously used only elsewhere in economics. Experimental economics is an example. Cummings, McKee and Taylor see exceptional opportunities to use that methodology to contribute to areas of environmental economics where old problems still lie unsolved.

Experimental economics is a logical step following the growing application of game theory in environmental economics. However, within the area of game theory there also exist ample opportunities for new research. Evolutionary game theory is a rapidly developing area. However, applications of this field in environmental economics are still scarce. Similar observations apply to interconnected game theory. This field has proven to be instrumental in the explanation of seemingly irrational behaviour in the practice of environmental policy and the development of instruments to promote cooperation with respect to international environmental problems.

Of course, new observations (real or imagined) that contradict the predictions of conventional models confront the researcher with new challenges. We see one in the claim that strict environmental policy might help the competitiveness of firms subject to it (Gabel and Sinclair-Desgagné). Tietenberg and Wheeler’s chapter on information strategies shows another instance in which research has been stimulated by observation, in this case of an induced response to environmental concerns.

Like new observations of practice, new policy issues often appear on the horizon and attract academic attention. Concern for pollution havens that trade liberalization has provoked is an example that comes up in a number of chapters (for example, Gerking and List, Ayres, Carraro). Another is the concern for carbon leakage in light of the Kyoto agreement (see Hoel). We see in the chapter by Gerking a curious case in which a topic – spatial issues – may shift meaning as new problems replace old ones in the literature. When Horst Siebert wrote about ‘Spatial aspects of environmental economics’ for the *Handbook of Natural Resource and Energy Economics* in 1985, he wrote mostly about the geographical dispersion of pollutants. Fifteen years later, most research attention focuses on industrial and labour location decisions.

Another emerging area of research interest – interest generated by events – is that of catastrophic risk. The chapter by Boyer and Sinclair-Desgagné is interesting in terms of this ‘Introduction’ because it takes up an emerging area – major technological risk – via an interdisciplinary approach (see the Gabel and Sinclair-Desgagné chapter). Corporate governance and the firm’s

internal organizational structures and systems have not traditionally interested economists. This chapter shows another novelty that may prompt research in the future. That is its shift of audience from the public policy maker to the corporate policy maker. The former has been virtually the exclusive target of advocacy writing in the past, but the latter may be the growth market as we look forward.

Catastrophic risk is the topic of the Kunreuther chapter, too, but here the focus is more on natural hazards: acts of God rather than acts of corporate principals and agents. As do others, the author sees the need to consider alternatives to the conventional expected utility-maximizing model of choice, which is a cornerstone of the neoclassical economics paradigm.

Finally, as stimuli for new research, there are simply problems that have long been around but have not been solved. Several appear in the following chapters. An example is the valuation problem that global externalities create for green accounting (see Aronsson and Löfgren). More generally, a number of authors (Kaufmann, Markandya, Carraro) point to the need for more applied empirical work to inform theory and to guide policy.

SUMMARY OF CONTRIBUTIONS

The first chapter, by Thomas Aronsson and Karl-Gustaf Löfgren, addresses the question of how to measure green net national product welfare indices that explicitly account for changes in environmental capital. The authors examine situations involving global external effects, which can cause fundamental valuation problems, yet have been frequently ignored in related studies. Aronsson and Löfgren explore the relationship between appropriate national and global welfare measures, on the one hand, and national and global tax policies used to improve resource allocation on the other. If countries act cooperatively, it turns out that the global welfare level is appropriately measured by a static index related to the sum of the countries' green net national products. When countries do not act cooperatively, however, this simple result no longer holds.

Chapter 2, by Robert Kaufmann, turns attention to issues in ecological economics. It begins by describing the field, outlining inherent conflicts between economic development and ecological preservation and reasons why ecologists have not been satisfied with the way in which economic models frequently characterize man's relationship with the environment. The chapter also speculates on the future development of ecological economics, concluding that ecological economists must begin to (1) represent key components of the ecological economic system better than more traditional economic models and (2) improve public policy regarding resource

and environmental management. This perspective is then more fully developed by arguing that the field of ecological economics might usefully become more interdisciplinary and emphasize empirical studies to a greater extent in the future.

The chapter by Shelby Gerking and John List begins by noting the growing importance of the spatial dimension and focuses on the interaction between environmental and natural resource economics and regional and urban economics. This intersection is characterized by the fact that the environment both affects and is affected by geographic movements of labour, capital and other mobile factors of production. The chapter treats traditional topics in this area, such as amenity valuation, human migration, the construction of quality of life indices, and industrial location as well as the more recently emerging issue of the possible impact of the spatial concentration of pollution on economically disadvantaged groups. Future research directions suggested highlight the role of 'new economic geography' models, endogeneity of environmental policy and regional growth.

Tom Tietenberg and David Wheeler look at public and private attempts to increase the availability of information about pollution. These disclosure strategies form the basis for what some have called the 'third wave' in pollution control policy, coming after legal regulation and market-based instruments. While these strategies have become commonplace in natural resource settings, they are less familiar in a pollution control context. However, research on and experience with this approach are now growing both in the OECD and developing countries. Following a review of the conceptual foundations of disclosure strategies, the chapter considers how the policy setting influences the type of information strategy employed. Examples of innovative disclosure strategies in the US, Latin America and Asia, and the channels through which they operate are followed by a review of empirical research on their effectiveness and suggestions for where future research might be helpful.

In the next chapter, Ronald Cummings, Michael McKee and Laura Taylor outline the applicability of experimental methods to issues in environmental economics and policy and then offer a view of future research opportunities in this area. Experimental methods are particularly relevant to further development of environmental economics because of their capacity to frame hypotheses, generate data (particularly when field observations are unavailable) and demonstrate the consequences of policy alternatives to policy makers. These themes are drawn out in detail by considering applications of experimental methods to valuation of environmental amenities or damages, policy design issues related to compliance with environmental rules and regulations and issues involving market-based policies and privatization. Examples chosen from these areas support the view that the interface of experimental

economics with environmental economics represents a challenging yet highly promising area for further research.

H. Landis Gabel and Bernard Sinclair-Desgagné examine the effects of environmental regulations from the perspective of a firm. Their starting point is to recognize that neoclassical economists assume that perfectly rational and efficient firms represent a crucial link between environmental regulatory policy and the allocation of environmental resources. Yet, in reality, firms are quite complex; they may have titular principals, but they are actually run by agents to whom a great deal of autonomy is delegated. Thus firms' activities are imperfectly controlled and coordinated by a set of interrelated management systems and procedures for operationalizing the principals' objectives. This chapter applies this view to show how it is possible for strict environmental regulations to be win-win (the 'Porter hypothesis') by simultaneously reducing the firms' private costs as well as external costs imposed on the environment.

In Chapter 7, Michael Hoel studies the links between international trade and transboundary pollution problems when capital is mobile. The general point is that actions taken by a country (or group of countries) will affect equilibrium prices of internationally traded goods, which in turn will affect production and consumption decisions as well as emissions. An important example in this regard is 'carbon leakage', which is a potential problem under the Kyoto agreement because developing countries have no quantitative commitment to scale back their greenhouse emissions. This chapter explores the carbon leakage problem from a theoretical viewpoint, concluding that industrialized countries should have the same carbon tax in all sectors and then supplement this instrument with appropriate import and export tariffs. The possibility of inducing developing countries to cut back on emissions by offering transfer payments is also explored.

The next chapter, by Anil Markandya, considers links between poverty, the environment and development. The chapter is developed around six research questions: (1) Does an increase in poverty within a community lead to increased environmental degradation? (2) Will an environment inhabited by the poor become more degraded than one inhabited by the rich? (3) How have important social changes resulted in concurrent increases in poverty and environmental degradation? (4) Does a deterioration of the ambient environment hurt the poor more than the rich? (5) Do policies to change the environment hurt the poor more than the rich? (6) Do increases in economic development over the long run ameliorate poverty and improve the environment? A survey of current literature suggests that existing research in these areas is strong on theory but that greater emphasis should be placed on high-quality empirical work in the future.

Robert Ayres contributed Chapter 9 on the topic of industrial ecology, which is the study of materials, energy flows and transformations in an

economic system and across its boundaries. Cross-boundary impacts on the biosphere have economic implications because they affect environmental services to humans. The services in question range from climatic stabilization and nutrient cycling to food supply and waste assimilation. Thus, industrial ecology overlaps resource and environmental economics. From this perspective, Ayres discusses trade and tax policy, economic growth, welfare and dematerialization, which benefits the environment by conserving value and by reducing dissipative uses of materials. He concludes that environmental assets are a crucial part of national wealth and, among other things, should play a more important role in economic growth analyses in the future. A specific research area to be targeted would be to determine whether technologies are selected by a 'survival of the fittest' mechanism or whether accidents, non-economic factors or path dependence appear to be equally or more important. Unless a survival of the fittest mechanism dominates, there may be a significant potential for double dividends and support for the Porter hypothesis.

The analysis of natural or environmental capital continues in the next chapter, written by Thomas Crocker and Jason Shogren. They model the nexus of private land uses, collective environmental protocols (constraints) and ecosystem services by depicting ecosystems as lotteries. They argue that for an ecosystem to provide protection against natural or man-made hazards, landowners must often exercise self-restraint over land use. A fundamental non-convexity exists in an owner's preferences if he chooses a discrete self-restraint alternative. Moreover, policy makers who disregard non-convex owner preferences for ecosystem configurations are likely to invest excessively in ecosystem care. Overprotection creates an environmental threshold that truncates both public and private opportunities to fill the void with lotteries to smooth preferences across states of nature.

In Chapter 11, Marcel Boyer and Bernard Sinclair-Desgagné focus on the issue of environmental risks associated with technology development. Large corporations are major developers of new technologies that are both an engine of growth in an economy as well as a source of new hazards, as illustrated by recent and highly publicized industrial accidents. Their chapter begins by reviewing these hazards and then models the way in which corporate boards assess prospective liability, including new environmental liability rules that apply to financial creditors. It then analyses internal mechanisms for implementing policies toward risk, such as stakeholders' involvement and selective audits, in a multi-task principal-agent framework. Further research is suggested to refine the definition of major technological risks and to study the evolution of regulation concerning corporate liability, the development of new insurance markets and institutions for sharing major risks.

In Chapter 12, Howard Kunreuther also focuses on aspects of environmental risk by asking what alternative strategies are appropriate for reducing

losses and providing protection against natural and environmental hazards that create potentially catastrophic losses to individuals, firms and society. The chapter emphasizes the importance of combining market mechanisms, such as incentives and insurance, with regulations and standards, and then illustrates this view based on a study of Hurricane Andrew in Florida in 1992. This discussion leads to a proposed programme for hazard management that is developed around improving estimates of risk, property inspections, building codes, mitigation incentives and the use of benefit–cost analysis. Specific suggestions show how each element in this programme can be strengthened by further research.

Charles Perrings's chapter advances understanding of the concept of sustainability of economic growth and development. It focuses on advances that stem from the application of the ecological concept of system resilience to environmental economics. The chapter begins with a critical review of empirical evidence provided by estimates of the environmental Kuznets curve and then turns to approaches for modelling sustainability using the concept of resilience applied in a Markov framework. However, much future research is needed to identify the transition probabilities associated with specific institutional conditions, and to weight those probabilities in terms of social objectives. This effort is quite important because real economy–environment systems generally involve states that are only partially observable and controllable and decision processes that are adaptive, that involve learning and that include conflicting objectives of agents who often behave strategically.

Carlo Carraro, in Chapter 14, analyses the role of technological innovation and diffusion to control and reduce polluting emissions. First, the main effects of environmental innovation on growth and emissions are highlighted. Then the incentives for firms to undertake R&D and carry out innovation are reviewed. Market imperfections and externalities imply that public innovation policies are necessary to supplement firms' investments in environmental innovation. Hence, the chapter discusses how R&D and innovation are linked to environmental policies, analyses how optimal policy mixes can be designed and focuses on the impact of related externalities on technological diffusion, crowding-out and endogenous growth. Given the international dimension of many environmental problems, the chapter also discusses the impact of environmental innovation on the geographic distribution of polluting industries and of environmental policies on the international dissemination of innovation. The first part of the chapter is devoted to theoretical models, whereas the second part provides a survey of empirical attempts to model environmental technological innovation and diffusion.

In the final chapter, Rüdiger Pethig looks to the future of environmental economics. He organizes his contribution around methods of analysis rather than specific environmental problems, beginning with theoretical concepts

and their ramifications and then turning to the role of interdisciplinary research, empirical work and the potential impact that environmental economics can have on policy formulation. In consequence, this chapter has a broader focus than the others. Yet, Pethig resists any temptation to advance a specific research agenda of his own, preferring to conclude that substantive research opportunities can arise from honestly addressing a few basic questions. For example, how much empirically sound theoretical knowledge has really been accumulated and how does empirical research and hypothesis-testing compare with other areas of economics? How much economy–environment interaction is accounted for in current environmental economics research and what is the still unused potential to draw on other disciplines' ecology-related knowledge? Answering these types of questions on the basis of reliable information certainly would promote successful corrective policies.

CONCLUDING THOUGHTS

Although the coverage of this volume is broad, we do not claim to have encompassed all of the areas of important future research nor all of the main topics within the areas covered. The following omissions are especially noteworthy. Extensive empirical work on environmental justice has been done over the past several years, primarily empirical studies of the negative environmental effects on ethnic or racial minorities and low-income groups. However, much of this work is *ad hoc* and thus disconnected from welfare economics or any formal conceptual base. The issue is whether this empirical research can stand on its own or whether it will require a theoretical foundation, including revisions or extensions of traditional welfare economics.

Another important area only partially addressed in this volume is the economics of natural hazards. This topic may fit into environmental economics if one were to think of positive attributes of the environment as resources and negative ones as hazards. The chapter by Kunreuther offers many other links. One basic consideration to emphasize is that a natural hazard is not solely a natural phenomenon; it is an interaction between the human use system and the environment. Thus the acceleration of natural disasters in recent years has not been caused so much by any increase in the frequency or severity of the natural stimulus as by our increased vulnerability to human actions such as the build-up of structures in flood plains. More empirical work is needed on the extent of this vulnerability now and in the future and on accurately measuring losses from natural disasters so that appropriate cost–benefit studies of mitigation can be undertaken. This will require more collaboration with geologists, meteorologists and engineers and could benefit from some cross-fertilization from benefits valuation in other areas of environmental economics.

Still another area is environmental valuation, which has been a dominant force in environmental economics for the past twenty years. In this volume this work has been extended to analysing ecosystems as lotteries and to formal rules for applying experimental techniques. Still further extensions are necessary, however.

Future research opportunities in interdisciplinary work are not restricted to further interaction between environmental economists and physical scientists but also include other social sciences, notably sociology and psychology. For instance, environmental economists have largely ignored the role of social networks in the development and diffusion of innovation. Such aspects have been left to sociologists who in their turn have shown little or no interest in typical economic concepts, such as profit and utility maximization and their determinants. As standard econometrics shows, omission of systematic explanatory variables, such as social networks, leads to biased estimators of the coefficients of variables included in the analysis as well as of their variances. Similar observations apply to the interaction between economics and psychology, in particular with respect to valuation of environmental quality. What have often appeared as anomalies to economists, such as the discrepancy between the willingness to pay and the willingness to accept, have long been well-understood phenomena to psychologists.

'Observation' is another area where environmental economics is still in its infancy. Most research has been theoretical. In spite of the elegance of the results and the many important insights that have been obtained in theoretical environmental economics, there undoubtedly exists a growing need for empirical research. Without empirical support the impact of environmental economics in policy making will remain limited. Because environmental issues hardly played a role in public policy until the late 1970s, there exist only short-run (multivariate) time series of such variables as emissions, environmental policy, employment, firm birth, exports, etc. Moreover, there exist substantial differences among countries. For instance, compared to the United States, most European countries are lagging behind. Lack of data seriously hampers analysis of such important issues as the impact of environmental policy on foreign trade and location behaviour of firms. In particular, cross-national comparisons are seriously hampered. For the further development of environmental economics, systematic collection of data is a prerequisite. Only then will it become possible for environmental economics to reach the stage of elegance and practical importance it should.

Despite notable advances in environmental economics and clear progress in reducing environmental pollution, extensive pollution problems persist. Several reasons can be offered, including the fact that environmental economics is still a relatively new field and the fact that economic advice goes only as far as policy makers take it. Still another explanation is that our

current paradigm does not require us to eliminate all pollution; we only reduce it to the point where the marginal benefits equal the marginal costs. One of the most valuable insights offered by environmental economists in the current paradigm, although it can sometimes be embarrassing when taken out of context, is that the optimal level of pollution is not zero. At the same time, most of us would prefer a world where pollution is reduced substantially below the current level. Marginal benefits increase with population and affluence, but the major mechanism for reducing pollution in the future will reduce the costs of attaining it. Some would suggest that competitive pressures and good foresight should cause this to happen. Others would point to the public goods nature of R&D, myopia in decision making and uncertainty about the future, implying that either market strengthening or market intervention is needed. Therefore, a very pressing area of future research would be developing incentives and institutions for reducing transaction and mitigation costs over time. The second major area of research would be to close the gap between good economics and good policy making by improving risk communication, institutional design and the decision-making process with respect to the environment.