Ecosystems (2001) 4: 709–715
DOI: 10.1007/s10021-001-0039-7

ECOSYSTEMS

Bridging Disciplinary Divides to Address Environmental and Intellectual Challenges

Ann P. Kinzig

Department of Biology, Arizona State University, Tempe, Arizona, 85287, USA

Many of today's environmental problems are characterized by dynamics and interactions that do not allow a clean separation between social and biogeophysical phenomena. The species with whom we share the planet may be threatened by land-use conversion, but human decisions concerning land use cannot necessarily be separated from the biogeophysical attributes of the land being converted. These decisions are, after all, influenced by culture and social norms, which may themselves derive from the ecological attributes of a current or remembered home (see, for instance, Berry 1996; Diamond 1997; Thomas 2000). Humans make their decisions on conversion as creatures of the landscape they are converting. Climate oscillations such as El Niño or the North Atlantic Oscillation are natural phenomena, driven by ocean–atmosphere interactions, among other things. But anthropogenically induced climate change may alter the frequencies, magnitudes, and spatial extents of these natural oscillations (NRC 1998), leaving in doubt which portions of the signature can be attributed to "nature" and which to human agency. The successional patterns in the ecological dynamics of the Great Plains do depend on the biological attributes of native (and invasive) species, but they can only be fully understood in the context of history, including the use of fire by the earliest Americans (Pyne 1982) and the demise of the bison grazers under human assault (Robinson 1995). Thus, we cannot effectively parse the patterns and dynamics of today's environmental systems into "natural" and "anthropogenic" components.

Solving today's environmental problems requires an understanding of the complex ways in which nature and society interact to make a whole that is different from the sum of its parts. An integration of knowledge from many different traditional disciplines will be required to see this whole (Lubchenco 1998). We can, as researchers, be bound by our traditional disciplinary approaches and expect this integration to occur elsewhere—in the hands of policymakers, managers, or other knowledge users. Or we can ourselves engage in an integrated and interdisciplinary analysis. The advantage of having at least some integration occur "upstream"—in the hands of scientists—is that scientists can be expected to be more cognizant of the assumptions and limitations of the component pieces, can better capture the inherent uncertainties and identify the robust outcomes, and can effectively recognize and categorize emergent patterns. However, upstream integration will only be effective if there is an exchange of information and interpretations among scientists, policymakers, managers, and other stakeholders.

Interdisciplinary environmental research should, however, be compelled by more than policy and management considerations. Scholarship that can bridge the traditional divides among the social, natural, behavioral, and engineering sciences represents one of the great intellectual challenges of the 21st century. The behaviors of coupled socionatural

Received 11 July 2001; accepted 16 July 2001.
**Corresponding author; email:* kinzig@asu.edu

systems may differ fundamentally from those we might infer from a study of the isolated component parts. We know, for instance, that the rules governing behavior and dynamics at small scales "scale up" to influence emergent patterns at larger scales. At the same time, these smaller-scale behaviors themselves become constrained by the properties of larger-scale phenomena (Levin 1999; Hartvigsen and others 1998). We could examine these scaling patterns within a system—the lowland tropical forest, for instance—and understand how biological and ecological phenomena interact across scales, from gene to individual to ecosystem. We may get a different picture of the processes structuring the lowland forest system, and its likely evolution over time, if we also gain an understanding of the influence of social phenomena such as the history of human habitation, forestry traditions, or present-day village, state, or national land-use policies. In other words, we can examine emergent properties within a system when a common set of (scaleable) rules are operating—ecological principles within the forest, as one example, or social/institutional principles within the state forestry agency—or we can examine the emergent properties when two different systems, governed by different internal rules, collide. There is a potential for surprising new patterns, or nonlinear dynamics, in these coupled systems that would reveal much about the nature of complex adaptive systems (see, for instance, Carpenter and others 1999; Scheffer and others 2000) and about the nature of our real-world environmental systems.

Interdisciplinary research can also give us a chance to test existing theories developed under disciplinary paradigms. Such testing would increase our confidence when extant theories work under new conditions and suggest needed modifications when they fail. Much ecological theory, for instance, has been developed in systems that are absent humans, or in systems where humans are considered an exogenous, simple, and detrimental perturbing force. The intricate ways in which humans might interact with ecological systems—both visiting and responding to ecological change—are rarely considered. How do our theories of succession, disturbance-diversity gradients, community assembly, or nutrient retention stack up in these more complicated, socionatural settings? Similarly, much neoclassical economic theory is predicated on a reliable and uniform biosphere, one with flows of ecosystem services and natural resources that are expected to persist or expand so as to conform to stated political or economic goals, and to vary little from biome to biome. How do theories of development, investment, economic growth, and efficiency perform when we confront a heterogeneous and potentially "unreliable" or nonlinear biosphere?

There are two fundamental reasons, then, for promoting interdisciplinary environmental research. Solving our environmental problems demands it; pushing the frontiers of intellectual inquiry compels it. This is not to say that any one interdisciplinary research project or individual researcher need address both masters. However, the community of scholars as a whole should ensure that both the assessments that feed directly into the policy process and basic research divorced in its motivation from immediate environmental problems are valued and supported. There is much progress to be made, and there is no "one size fits all" approach to making it. In addition, continued advances in discipline-based science are absolutely essential to maintain and build the foundations upon which interdisciplinary research must be based.

At the same time, the limited government and private resources available for funding scientific inquiry—both in the United States and other nations—means that the assessment and prioritization of possible future research pathways is crucial. As a first step in setting those priorities, 45 researchers convened in Tempe, Arizona, for a 4-day workshop in June 2000. The workshop was funded by the US National Science Foundation (NSF), and the attending researchers came from the fields of anthropology, archeology, biology, climatology, ecology, economics, engineering, epidemiology, geography, political science, public policy, and sociology, among others. The participants came from 31 different research and academic institutions in 20 states and four nations. They were charged with assessing research priorities for interdisciplinary environmental research efforts that would span the natural, social, behavioral, and engineering sciences.

One striking feature of this meeting was that none of the participants called explicitly for more funding for his or her discipline. Instead, they all affirmed that a bold departure from the status quo—with the current preponderance of emphasis on single-investigator, disciplinary science—was needed to address pressing national and intellectual needs. They agreed that a broad array of new approaches is required, including the expansion of multiple-investigator long-term research, the promotion of multi- and interdisciplinary synthesis and assessment, and an increase in our capacity for scenario building and forecasting in coupled human and natural systems.

The meeting was organized by an eight-person Steering Committee that included Stephen Carpenter (aquatic and ecosystem ecology), Michael Dove (anthropology), Geoffrey Heal (economics and public policy), Simon Levin (theoretical ecology), Jane Lubchenco (marine biology and community ecology), Stephen Schneider (climatology), David Starrett (environmental economics and public finance), and myself (urban ecology). Eight white papers were solicited by the Steering Committee in advance of the meeting and distributed to meeting attendees. Five of these white papers are reproduced, in modified form, in this special feature; three others can be found at the meeting website (http://lsweb.la.asu.edu/akinzig/nsfmeet.htm).

The solicited white papers, which were used to initiate the 4 days of workshop discussion, helped to spawn several primary recommendations for both research and implementation. Participants ultimately agreed that five broad research areas were in particular need of increased attention. These were:

1. Evolution and resilience of coupled social and ecological systems
2. Ecosystem services
3. Coping with uncertainty, complexity, and change
4. Environmental dimensions of human welfare, health, and security
5. Communicating scientific information

Further details of these research areas can be found in Table 1; the full workshop report, the executive summary, a list of attendees, and three white papers can all be found at the meeting website.

White-paper authors were given relatively free rein in crafting their messages. In the end, however, the papers served three primary purposes. Some provided a review of the existing literature in the natural and social sciences. This allowed the meeting attendees, who came from diverse fields, to establish a common understanding and language. Others helped to outline the criteria that could be used to choose among competing priorities. Still others suggested what some of those priorities could or should be. These papers contributed to a cooperative and fruitful exchange at the meeting. We reproduce them here—and on the website—in the hope that the reviews and insights found therein will prompt similar exchanges in the larger community.

Buz Brock (article posted at http://lsweb.la.asu.edu/akinzig/nsfmeet.htm) provides an extensive review of the economics literature. He concentrates on the areas of economics that would be most useful in promoting research at the interface of ecology and economics, requiring expertise in both disciplines. Some of the topics he covers include: (a) measurements of human well-being, (b) measurements and conceptualizations of sustainability, (c) reform of management of the commons, (d) revisiting benefit–cost analysis, (e) controlling the "offloading" of environmental costs, and (f) scaling laws (including speculation on why they are so pervasive). His entire review is informed by the need to achieve a level of global environmental prosperity comparable to the level of material prosperity that has been achieved in the developed world under free-market and private-property institutions. Achieving global—or even regional—environmental prosperity will mean devising similarly effective environmental institutions. I would encourage any reader interested in economic issues, and the interface between ecology and economics, to peruse this paper. It is not always easy going—the information density is extraordinarily high—but the committed reader will be rewarded by a number of gems throughout the paper, as well as a valuable guide to and summary of literature relevant to development and natural resource management.

Terry Root (article posted at http://lsweb.la.asu.edu/akinzig/nsfmeet.htm) reviews the literature on natural capital, uncertainty, and scaling, with a particular focus on animals and their ecosystems. She integrates the ecological and economic literature by summarizing the various ways in which animals and animal activities are valued, both in and external to the market. Root also provides an extensive review of what is known and—just as importantly—what is not known about the ways in which animals will respond to and influence global climatic change, with an emphasis in the latter context on the methane cycle. Root then discusses an approach for understanding complex dynamics in a rapidly changing climate known as Strategic Cyclical Scaling (SCS). SCS employs a detection, attribution, and testing analysis with explicit shifts among scales (regional to local) to determine emergent patterns and underlying mechanisms. She ends with a discussion of the synergistic effects of habitat fragmentation and global climate change on animal populations and communities, and the demands on knowledge and research that will be required to address these synergistic effects.

Michael Oppenheimer (article posted at http://lsweb.la.asu.edu/akinzig/nsfmeet.htm) examines issues associated with large-scale, nonlinear, and rapid changes, with particular emphasis on the

Table 1. Summary of Research Recommendations

Increased interdisciplinary environmental research is needed in the following five areas:

1. The Evolution and Resilience of Coupled Social and Ecological Systems

   The ways in which human social and economic systems evolve will depend on the ecological endowments of a region. The changes in these ecological systems over time will in turn depend on the extent, intensity, and types of human activities. This "coevolution" will determine the trajectories and resilience of social and ecological systems. Thus, integrated analysis of these systems is required if we are to improve our ability to forecast and respond to environmental change. Possible research areas in this category include:

   - The evolution of social norms regarding the environment
   - Understanding past and predicting future land-use change
   - Feedback loops in social and ecological systems
   - Disturbance and resilience in social and ecological systems
   - Developing coupled models of social and ecological systems

2. Ecosystem Services

   Healthy ecosystems provide numerous economically important and societally beneficial services. Substantially more interdisciplinary research is required to advance our understanding of the key ecosystems and ecological structures required to sustain these services, the ways in which human activities alter these systems, the approaches for their proper valuation, and the institutions required for realizing this value. Possible research areas in this category include:

   - Human impacts on ecological structures and the delivery of ecosystem services
   - Valuation of ecosystem services
   - Variations in the delivery of ecosystem services and their valuation from local to global scales
   - Assessing manufactured or managed substitutes for ecosystem services

3. Coping with Uncertainty, Complexity, and Change

   Social and ecological systems are sufficiently complex that our knowledge of them, and our ability to predict their future dynamics, will never be complete. We must work to reduce uncertainties when possible, improve assessments of the likelihood of various important future events, and learn—scientifically, socially, and politically—to cope with environmental change that may elude precise prediction. Possible research areas in this category include:

   - Indicators of human welfare and environmental change
   - Risk assessment and risk reduction for technology deployment
   - Governance and management of common-pool resources
   - Adaptive institutions and social learning

4. Environmental Dimensions of Human Welfare, Health, and Security

   There is increasing recognition that local and regional environmental quality can significantly influence human welfare, including health and security. Human social arrangements—including the degree of political democracy or socioeconomic equity—can in turn profoundly influence welfare-environment interactions. Significantly more interdisciplinary analysis is required to assess the dynamics of these interactions and to identify the approaches that can simultaneously improve human welfare and environmental quality. Possible research areas in this category include:

   - Environmental change and human health
   - Environmental justice, poverty, and inequity
   - The environmental dimensions of human conflict

5. Communicating Scientific Information

   Interdisciplinary environmental research will not serve society unless the knowledge gained can be communicated effectively to policymakers and stakeholders at all levels of the social and political spectrums. At the same time, scientists must be responsive to society's articulations of goals and perceived national challenges. Yet there are significant differences in the ways in which different social and political groups access, interpret, and use scientific information, and influence the scientific process. Research is needed into the ways in which scientific information is constructed and communicated, and ways to improve the process of information dissemination, from scientist to citizen and vice versa. Possible research areas in this category include:

   - The effects of disparate access to science and scientists
   - The impact of information technology and nongovernmental organizations on the flow of scientific information
   - Stakeholder participation in natural resource management and policy formulation
   - The effectiveness of interdisciplinary training

Earth's climate system. Noting the potential for small-scale perturbations to initiate rapid change at regional or global scales, he uses as examples of this phenomenon the Antarctic ozone hole, climatic changes associated with cessation of thermohaline circulation, and the potential instability of the West Antarctic Ice Sheet (WAIS). A review of these three cases leads Oppenheimer to conclude that anticipating large-scale (and potentially catastrophic) change might require a specialized observational and modeling program that focuses on small-scale events. Oppenheimer also notes that our definition of "rapid" may need to be contextual. Climate flips that occurred over only a few years would clearly be considered rapid because adaptation for either ecological or social systems on those time scales is severely constrained. But changes that take decades or even centuries to evolve may also appear rapid if systems with high inertia are affected, or if change is so severe that adaptation is difficult or impossible. Similarly, our definition of "large-scale" events may need to be defined less in terms of geographic extent and more in terms of the potential to affect critical biological systems or human institutions. In both cases—"rapid" and "large-scale" changes—a dialogue among natural, social, behavioral, and engineering scientists is required to elucidate those nonlinear phenomena that are most likely to have severe or catastrophic consequences.

In the first white paper reproduced in this special feature, Katherine C. Ewel reviews the need for interdisciplinary education, research, and management collaboration in a world where all ecosystems are—either intentionally or inadvertently—managed. She suggests that the concept of ecosystem services may be particularly useful for communication across disciplines and among scientists, managers, and stakeholders. In determining together the suite of services provided by a particular ecosystem, laypeople can increase their understanding of the hierarchy of processes governing ecosystem patterns and dynamics, and scientists and managers can increase their appreciation for those processes or attributes society deems most in need of protection. This portrayal of the benefits of broad communication leads naturally to a discussion of adaptive management. Ewel nicely lays out both the advantages and limitations of this management approach. Finally, she suggests that small islands may be one of the best places to begin our studies of the relationships among people, management, and natural processes, since these islands, with their well-bounded biogeographic regions, make it relatively easy to gather the relevant economic and demographic data and to gain ready access to a relatively small group of key decision makers. In such settings, researchers can begin to disentangle the evolving character of the human–nature interaction in a rapidly changing world and elucidate the relative influences of "internal" and "external" processes.

John M. Antle and others follow with a specific focus on lessons learned in agroecosystems. Their review of the economics and ecological literature suggests that much of the integrated assessment conducted in highly managed systems relies on building overall systems models from component parts designed originally for a discipline-specific purpose. These component models operate at spatial and temporal scales most appropriate for the discipline in question; potential scale mismatches limit the degree to which these discipline-based models can be linked and thus limit our understanding of emergent patterns when systems interact. Antle and others promote a conceptual model of managed ecosystems as interacting and—to some extent—inseparable physical, biological, and social systems; and they argue persuasively that understanding and predicting the behavior of real-world agroecosystems demands communication across disciplines and a more highly coordinated set of models designed specifically for integration at appropriate scales. They contend that "coordinated disciplinary research," as much as interdisciplinary research, is needed to advance our fundamental understanding of highly managed systems and improve our predictive capacity relative to them.

Susan Hanna focuses on marine fisheries, which are often used as the textbook example of the failure to correctly manage the interface between human and ecological systems. In a review of the modern history of marine fishery management (post–World War II), she notes that marine fisheries, like other managed natural resource systems, are being challenged today by a changing set of values and interests, including the need to preserve ecological and social complexity, mediate among multiple and potentially competing uses, and effectively integrate ecological and social dynamics to achieve some measure of sustainability. The present-day problems of reduced productivity and diversity in marine fisheries can ultimately be traced to outdated institutions, fashioned in a different era, that cannot accommodate these changing values. Hanna also points to several key areas where significantly more knowledge is needed to design effective management institutions, including the role of incentives in shaping behavior, the appropriate scale for management, and the role of history in influencing present-day dynamics. She goes on to discusses what she perceives as some of the highest barriers to pursuing the interdisciplinary research needed to acquire this knowledge, such as the view that marine ecosystems are fundamentally biological (and not social or economic) systems and the myth that management is more about finding the proper (and "scientific") end points and less about following an adaptive process to implement social goals.

William Ascher has contributed a valuable review of the ways in which resource management institutions can become suboptimal due to two phenomena—the need to reduce complexity, and the desire to advance organizational or institutional interests. Any decision making involves some degree of reduction in complexity and uncertainty. To try, for instance, to understand complicated ecological systems in their entirety before making decisions would create management paralysis. Instead, scientists and managers identify what they believe are the key structuring mechanisms in the system and the key drivers of crucial dynamics; this sort of reductive analysis, while understandable, in the end simplifies the true workings of the system. Certainly, the reduction of complexity is necessary if we are to act at all. At the same time, organizations, and the people within them, are motivated not only by the mission of husbanding the natural resources they are charged with overseeing, but also by the desire to advance their own organizational or personal interests. These two phenomena—the reduction of complexity and the maximization of self-interest—can interact in interesting and often detrimental ways. Ascher provides a compelling review of these institutional challenges, shows how they can lead to perverse outcomes and perverse learning in natural resource management, and lays out a comprehensive research agenda that can help us to understand and address them.

Finally, Donald Ludwig attacks the notion that systems can be "managed", in the traditional sense of that word, at all. According to Ludwig, the dominant ideologies of our time support the notion that scientists and managers, deploying the scientific method and objectively weighing evidence, can solve the world's problems. But most, if not all, of our environmental problems are actually "wicked" problems—problems with "no definitive formulation, no stopping rule, and no test for a solution." That is, they defy final resolution. In addressing wicked problems, scientists must accept that science is only one of many considerations, that deciding on a course of action must involve issues of values, power, equity, risk, and justice as much as science. They must also acknowledge that their science is not value-free. Ludwig proposes some potential replacements for the old system, including consensus-based methods and "civic science." Such a revising of the management endeavor will require analysis and communication that not only crosses traditional scientific boundaries but also transforms the traditional means of communication between scientist and nonscientist. This article should challenge many scientists who believe they can and should play unique roles as "experts" in natural resource management.

The calls for interdisciplinary research contained in the white papers and workshop report are not new; efforts at integration have been under way for some time. There are some positive signs that support is increasing—witness, for instance, NSF's new Biocomplexity focus on the "Dynamics of Coupled Natural and Human Systems." The vast majority of research, however, continues to be largely disciplinary or integrated across closely related fields within the natural, social, behavioral, or engineering sciences. Again, a significant proportion of discipline-based research is required to advance knowledge and to provide the component pieces for integration. We should also not underestimate the challenges of achieving a broader integration across disparate fields. We will have to overcome or dismantle several barriers to such research, including those of scarce funding, disciplinary institutional traditions and organizational structures, inadequate interdisciplinary training, and insufficient rewards for integrative research. We will have to communicate across divides of vocabulary, reconcile different investigative approaches, and fuse competing paradigms to achieve a shared understanding. Such achievements can only occur in an atmosphere of mutual respect, which can itself be distressingly difficult to create.

To advance an interdisciplinary environmental agenda, we will have to make other strides as well. Scholars will need to forge stronger international relationships. The nations of the world have diverse academic traditions—alternative ways of slicing the disciplinary pie and different cultural and historical perceptions of human–environment interactions—and a more vigorous exchange among those schooled in different traditions and paradigms would advance our understanding more rapidly. We will have to gaze over longer time scales to consider not just the decade-long time scales that characterize much "long-term" research, but historic and prehistoric time scales that tell us something more about the myriad ways in which the human–environment interaction plays out (see, for instance, Tainter 1990; Redman 1999). The distant past may not be a perfect guide to the future, but we ignore its lessons at our own peril. And we must be confident enough to become beginners again, to occasionally put down our hammers and pick up saws, pliers, screwdrivers, however awkwardly we might handle these implements at first. Such experiences will help us to value those more expert in handling different tools and to learn to work in concert with them.

When engaging in policy-driven science to address environmental problems, scientists also need to realize that even an integrated scientific assessment is only part of the decision-making process. Issues of power, equity, justice, and efficiency, among others, also come into play, and scientists are no more expert than other citizens when navigating among these considerations. This also means that there is never a single "right" or "rational" decision to be made; the solutions and tradeoffs society is willing and able to tolerate will depend on values, and history, and power-sharing arrangements. Scientific research can help to define the suite of possible solutions and can articulate some tradeoffs, but it cannot determine the best choice among them. To be effective in this decision-making process, scientists need to take their place at the table. That means an exchange of information and perspectives, not just a one-way provision of scientific data to guide or determine policy. Interdisciplinary environmental research is a necessary but not entirely sufficient endeavor in solving environmental problems. The process of achieving consensus on the futures we choose and the different perspectives governing those choices are equally important.

The final hurdle each of use will have to clear to engage in truly integrated and interdisciplinary environmental research—into studying the human–environment interaction—is to understand and question where we think "nature" leaves off and society begins. What does it mean when we separate humans from nature? Can we think of nature today as having an existence independent of human thought and action? How and why do we separate these two things? What does that mean for the boundaries we draw circumscribing the scientific questions we are permitted to ask, the analyses we are willing to conduct, or the experimental designs we are trained to deploy? Our biggest challenge will lie in pushing our ideological boundaries a bit beyond where they are now, and our reward will be in seeing what we discover in the process.

ACKNOWLEDGMENTS

I thank all of the workshop participants for their impressive ability to bridge disciplinary divides and engage in constructive and respectful disagreement on the way to consensus. Each of them served to advance my own thinking on these issues. The members of the Steering Committee and the white-paper authors deserve particular thanks. Greg Guibert was invaluable in meeting preparation and provided both intellectual and logistical support. I am grateful to Scott Collins for his thoughtful comments on an earlier version of this manuscript. Many others have influenced my understanding of the needs for and challenges of interdisciplinary research; while acknowledgment of any will overlook some, I give particular thanks to John Holdren, Sander van der Leeuw, and members of the Resilience Alliance. The workshop for which these white papers were prepared was funded by the National Science Foundation (DEB-0073653).

REFERENCES

Berry W. 1996. The unsettling of America: culture and agriculture. 3rd ed. Sierra Club Books, San Francisco. 234 p.

Carpenter SR, Ludwig D, Brock WA. 1999. Management of eutrophication for lakes subject to potentially irreversible change. Eco Appl 9:751–71.

Diamond J. 1997. Guns, Germs, and Steel: the fates of human societies. New York: WW Norton. 480 p.

Hartvigsen G, Kinzig AP, Peterson G. 1998. Use and analysis of complex adaptive systems in ecosystem science. Ecosystems 1(5):427–430.

Levin SA. 1999. Fragile dominion: complexity and the commons. Reading (MA): Perseus Books, 250 p.

Lubchenco J. 1998. Entering the century of the environment: a new social contract for science. Science 279:491–497

[NRC] National Research Council, Panel on Climate Variability on Decade-to-Century Time Scales. 1998. Decade-to-century scale climate variability and change: a science strategy. Washington (DC): National Academy Press.

Pyne SJ. 1982. Fire in America: a cultural history of wildland and rural fire. Princeton University Press, Princeton, NJ. 654 p.

Redman C. 1999. Human impact on ancient environments. Tucson (AZ): University of Arizona Press. 239 p.

Robinson CM III. 1995. The buffalo hunters. Austin, (TX): State House Press. 162 p.

Scheffer M, Brock W, Westley F. 2000. Socioeconomic mechanisms preventing optimum use of ecosystem services: an interdisciplinary theoretical analysis. Ecosystems 3(5):451–71.

Tainter J. 1990. The collapse of complex societies Cambridge (England): Cambridge University Press. 250 p.

Thomas JL. 2000. A country in the mind: Bernard DeVoto, Wallace Stegner, history, and the American land. Routledge, New York. 256 p.