

US Ecologists Address Global Change

James H. Brown and Jonathan Roughgarden

ENVIRONMENTAL CHANGES caused by the expanding human population are altering the air, land, water and biota of the entire earth. The survival of millions of species, including *Homo sapiens*, is in jeopardy. If the earth is to avert an ecological holocaust, basic ecological research must be given highest priority. These were the conclusions of ecologists and scientists from other disciplines who met in Santa Fe, New Mexico in December 1988. The workshop on 'Ecology for a Changing Earth' was sponsored jointly by the US National Science Foundation (NSF) and Department of Energy (DOE), and it included 23 participating scientists and 15 invited observers from government agencies. The scientists reviewed recent accomplishments of ecological research, discussed the kinds of theory and data that are most needed to address the problems of global change, and made specific recommendations for new research initiatives and government agencies.

The participants agreed that the many interrelated problems of global anthropogenic change are the most serious that *Homo sapiens* has faced. In the last few centuries human beings have increased to become the dominant species on earth, commandeering 20–40% of terrestrial primary production and clearing more than 11% of the land area for intensive agriculture. In just the last few years it has become apparent that human activities have altered the composition of the atmosphere, increased pollution and toxification from local to regional and global scales, and caused drastic declines in biological diversity¹. Although these changes are being documented, major research efforts will be required to understand their causes and to predict their consequences.

In the last three decades ecological science has made significant advances, but most of the research has been at a level that is of limited use in understanding global environmental change. Empirical studies have emphasized experimental approaches. In order to achieve the scientific rigor that comes from manipulation and replication, most experiments have been conducted on small plots for brief periods. Theoretical studies have also focused on relatively small, simple systems. In order to facilitate mathematical tractability and experimental testing,

most ecological models are for closed systems containing only a few variables. Most ecologists have been concerned with investigating the structure and function of pristine natural systems. Because of the limited scale of their research, they have been able to choose study sites where the effects of human activities are minimal.

The result is that ecology has become an experimental, model-building, hypothesis-testing science, but one that has largely ignored the impact of humans on ecological systems and has not developed the intellectual and technological tools to tackle the size, complexity and diversity of the changing earth. Two major kinds of research are needed to redress the balance: (1) theoretical and empirical studies of the phenomena of scale, so that we can begin to understand the structure and dynamics of spatially and temporally heterogeneous ecological systems; and (2) studies that explicitly incorporate the diverse effects of humans both in ecological models and in the field. The workshop recommended that these two topics be the subject of major new research initiatives in NSF and DOE.

One initiative should fund research that will characterize the natural scales of ecological processes, and show how processes that operate at different scales are coupled. Theoretical research might explore relationships among different kinds of models of the same system that vary in mathematical technique, complexity and realism. There should be efforts to model large, heterogeneous ecological systems, and to compare the results with the temporal changes documented in the fossil record and other long-term data sets, with the spatial variation recorded by remote sensing, and with the projections of global climate and physical-oceanographic models. Empirical studies, both experiments and comparative observations, will be required to measure the direction and magnitude of environmental change, and to test the predictions of models. New advances in statistics and artificial intelligence will be needed to analyse and synthesize the results.

A second initiative should support experimental and theoretical research that incorporates humans as an integral component of ecological systems at all scales from local to global. There should be studies

of moderately to heavily human-impacted forest, pastoral, agricultural, suburban and urban systems. Basic research is needed on the effects of human-altered climate and landscapes on extinctions and invasions of species, and on the dynamics of population, community and biogeochemical processes. There should be support for collaborative studies among ecologists and social scientists on the growth and resource use of the human population, on the ecological basis of sustainable economic development, and on the reciprocal relationships between different social, economic and political systems and ecological landscapes, biological diversity and ecosystem processes. The support for long-term ecological research (LTER) should be increased to include suburban and other human-modified sites and to incorporate more population and community studies.

In addition to these immediate research priorities, the workshop stressed the need for new government agencies to acquire, analyse, synthesize and disseminate ecological information. For the United States, the most immediate need is for a US Ecological Survey (USES) to conduct a long-term, nationwide ecological inventory. This agency would interact closely with other government agencies to monitor the state of the environment, to detect and interpret change, and to respond rapidly to catastrophes, such as the Mt St Helens eruption or an accident at a nuclear power plant. It would maintain and make available large-scale and long-term data bases on climate, geology, oceanography, paleontology, and the abundance and distribution of living organisms. The agency would also coordinate with global change programs in other countries, and work towards an international data base.

Finally, the workshop recognized the need for a single new agency to assume responsibility for multidisciplinary environmental research in the United States. The model that was discussed was a National Institutes of the Environment (NIE), which might contain at least five institutes with specific missions: the US Ecological Survey (mentioned above), an Institute of Basic Ecology, an Institute of Applied Environmental Sciences, an Institute of Human Demography, and an Institute of Sustainable Economic Development.

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The creation of NIE would be a bold step, requiring the leadership of natural and social scientists, and of the executive and legislative branches of the Federal Government. It would signal a widespread

recognition of the global environmental crisis caused by human activities. It would demonstrate a commitment to mobilize the money, technology and scientific expertise required to ensure the continued

health of the planet and the continued existence of our own species.

Reference

1 Dobson, A., Jolly, A. and Rubenstein, D. (1989) *Trends Ecol. Evol.* 4, 64–68

History and Evolution

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EVOLUTIONARY BIOLOGY and the study of human history are analogous. Both are concerned with directional change and chronology, the events that constitute change and the laws governing the processes that link those events. Can anything useful be done with this analogy? Is cultural change just an epiphenomenon of evolution? How does studying the past (in evolution and history) help us to understand the present? What can be gained by an historical analysis of evolutionary thought? What can evolutionary biologists and historians learn from each other's methods?

These questions, plus a host of embedded topics that were addressed at the Spring Systematics Symposium at the Field Museum in Chicago this May, are the sort that often generate more heat than light. By forcing us onto unfamiliar territory – for most modern evolutionary biologists are not well-versed in history – they engender a sense of insecurity with all the defensive posturing that that entails, together with a suspension of critical faculties and a consequent susceptibility to seduction by seemingly neat new ideas. Happily, however, these pitfalls were largely avoided at the symposium, and the 700-strong audience was offered, in the course of a dozen lectures*, an opportunity to view the study of evolution from a variety of historical and innovative perspectives.

The idea that cultural change in human societies can profitably be studied from a darwinian perspective is controversial. Robert Boyd (University of California at Los Angeles) and Peter Richerson (University of California at Davis), who amongst others¹ have written extensively² on this concept, argue

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that culture can be viewed as a system of inheritance, with variation between individuals and populations that is transmitted (through teaching, imitation, etc.) to other individuals and populations. They find evidence for analogs to processes akin to natural selection, mutation and random drift operating on cultural phenomena, and suggest that cultural change (i.e. human history) is – like microevolution – a sequence of unique and contingent events, which – like microevolution – can be investigated and modelled within the neodarwinian paradigm (for more details, see Ref. 2). An opposite view was taken by Will Provine (Cornell), who, while conceding the existence of a deep analogy between cultural and organic evolution, argued that the analogy is too general to be helpful. In Provine's opinion, and contrary to Dobzhansky's famous dictum, evolution is not essential to the successful pursuit of most biological disciplines; what, then, he asked, can it hold for the study of human history, given especially that most of documented history has taken place with no appreciable genetic change, and that cultural inheritance cannot be measured with the accuracy of genetic inheritance?

The theme of contingency in history and evolution was also explored by Stephen Gould (Harvard). The course of evolution can be interpreted very largely on the basis of particular unique events that happened to occur at particular unique places at particular moments: the emergence of *Homo sapiens* as an entity in Africa 0.25 million years ago was a matter of historical contingency. Of course, this notion is not exactly new: it was discussed in detail by Jacques Monod³ nearly two decades ago, and – as Gould pointed out – even Darwin argued that the domain of 'what we call chance'⁴ was of enormous significance in evolution. Gould's message was that the resulting unpredictability doesn't devalue the science: there is nothing that is not rigorous about attempting

to explain cause or process of a sequence of events in narrative terms. Sciences, said Gould, should certainly not be arranged in a pecking order according to their ability to predict, with physics at the top and the social sciences at the bottom, and ecology and evolutionary biology somewhere around the lower middle. Rigour lies in the choice of non-trivial problems to investigate, and in the level of objectivity that can be brought to bear. Similar arguments have recently been explored in *Origins*⁵, a collection of essays on sciences that require investigation of the past (reviewed in Refs 6 and 7).

Techniques for reconstructing the past, and methodological parallels between the studies of history and historical sciences (including evolution), were discussed by several speakers. Rachel Laudan (University of Hawaii) argued that studying the past involves few special methodological difficulties that would not be encountered in the study of anything else. Using geology as an example of a science that has been successful (albeit only since the revolution of plate tectonic theory⁶) in reconstructing the past, Laudan discussed methodology under three main headings – chronology, causal theory and narrative – and indicated how each of these components have strong analogs in the non-historical sciences.

Chronology, in the historical sciences, is the process of establishing past events and their sequence, and has its obvious equivalent in the non-historical sciences in the form of observation and description: the record of the past may be incomplete, but it is no more inaccessible, in Laudan's view, than the very small or the very distant entities and processes studied by, say, particle physicists. Laudan conceded that causal theory – the second mainstay of the scientific process – is less easy to establish in the historical sciences: students of non-historical sciences have the advantage of being able to investigate cause and effect by manipulation and experiment. Nevertheless, that experiment is not a prerequisite to establishing plausible cause in either historical or non-historical sciences is amply demon-