ECOLOGY AND SOCIETY

Home | Archives | About | Login | Submissions | Notify | Contact | Search

ES Home > Vol. 1, No. 1 > Art. 9

Baskerville, G.L. 1997. Advocacy, science, policy, and life in the real world. Conservation Ecology [online] **1**(1): 9. Available from the Internet. URL: http://www.consecol.org/vol1/iss1/art9/

Perspective, part of Special Feature on Science, Policy, and Advocacy Forum

Advocacy, Science, Policy, and Life in the Real World

Is ecological advice unsafe practice for regional management of ecosystems? That is what Gordon Baskerville suggests in the following Perspective, but seven commentators with experience in both science and policy enrich and challenge that view.

Gordon L. Baskerville[1]

Faculty of Forestry, University of British Columbia
Vancouver, British Columbia

Disruption of the structure and function of ecological systems constitutes a set of major problems for contemporary society. It is straightforward to conclude that the actions of humankind that create these problems are improperly, or insufficiently, informed. By that, I mean the problems were not created with malice aforethought, but rather were/are largely the result of inadequate reflection before action. In areas with which I have some familiarity, this inadequacy appears to derive primarily from two sources: (1) weakness of models (of the mind or in a computer) used to create the forecasts that underlie the design of management of forests and of related environmental systems, and (2) insufficient attention to, or impossibility of following, those forecasts during implementation. People do not make dumb choices, so much as they choose from among dumb forecasts and/or make insufficient effort to ensure that the actions are invoked to "cause" the chosen forecast to happen.

Actions follow on decisions, and a decision is a choice among forecasts. It is rare that a manager making a decision chooses to implement a forecast for a bad future. Thus, in a failed decision, it is common that the decision maker chose a "best" forecast that was not functionally coherent; implementing the actions of that forecast, therefore, *could not* create the putative future (Raffia 1968).

As a society, we seem to converge on pathological gridlock with respect to linking economics and ecology in decision making. Ecological evaluation too frequently assumes (usually unconsciously) that there are no costs to be borne in an "ecological" action/constraint. On the other hand, the purely economic approach to decision making assumes ecology away by merely maximizing net present value as measured in dollars, subject, when necessary, to constraint "for environmental protection." When ecological science does not enter this computation, there is no basis for testing the forecast of the decision with respect to dynamics of the system. Thus, it is common that failure of a decision results in damage to the system, without benefit of learning by the manager, which, in the final analysis, is the place where learning counts. The opportunity to learn exists whenever expectation does match reality, but the quality of learning depends heavily on how clearly the functional basis for the expectation is stated a priori.

In this context, ecological science has not been, and is not now, of much help to the manager who *must* decide and who wants to learn from each decision. Ecological science seems to be tied up in describing what things look like, rather than in discovering how systems function. Problems in managing resource systems do not lie in what the systems look like, in general, but rather in how *the system* being managed functions. In a world where everyone who so chooses is an expert in ecology, the meaning of the word and, more importantly, the meaning of the science, seems tragically lost.

In forestry, more and more land is withdrawn from timber production and more and more constraints are placed on harvesting and silviculture to preserve "the ecology." Frequently, existing mill capacity is rendered nonsustainable by ecological zoning. In my experience, it is rare to encounter industrial resistance to these changes where the case is made *in science*. The credibility of ecological science is, however, stretched. Replacing "bad" practices with presumed "good" practices on "ecological" grounds (i.e., it is *known* that the replacement practice is"good" out of the context of natural system dynamics) too frequently results in even more damage to the system, no learning about the dynamics of that system, and a loss of the "ecologist's" credibility in the eyes of the manager.

Boothroyd (1978) defined management as articulate intervention, with the crucial element being "reflection before action." The reflection is on how the subject system is structured, how it functions, and, how it is likely to respond to intervention. Although written from different perspectives, Boothroyd's reflection before action is similar to the role of forecasts in informing a decision (Raffia 1968) and to the idea of adaptive management as advanced by Holling (1979) and by Walters (1986). For these authors, goodness or badness of tools lies *only* in context of the dynamics of the system upon which interventions are directed to solve a problem in system performance.

Contemporary ecology seldom advocates or facilitates reflection before action. There can be no doubt that ecologically naive policies of the past (and present) exacerbate problems in achieving sustainability of the natural systems humankind chooses to develop for economic purposes. However, intelligent use of ecological reasoning in contemporary problem solving and future building, with respect to natural resources management, is hard to find. Perhaps it is smothered in the so-called optics of communication. There is plenty of theory, and an abundance of blunt instruments (stop signs), but not much thoughtful exercise of ecological science thinking, and not much reflection before action. This is true even if one allows that theory gets published, while practice just gets done. One is moved to paraphrase an old saying "ecology is used in resource and environmental management much as a drunk uses a lamp post: more for support than for illumination."

Although there is no shortage of research on the hot ecological topics in resource management, most is descriptive. From the point of view of a manager, not much addresses system dynamics, is integrative, or is capable of being integrated across the range of temporal and spatial scales required in designing forest management. Not much contemporary ecological research seems to be targeted at understanding natural *system dynamics* at the scales at which they function and are encountered by a manager, or at building usable scientific constructs of dynamics at those scales to inform management of resource and environmental systems. Holling (1995) has noted this problem of temporal and spatial scales, both in studying and in regulating ecological systems.

Relative to the design of forest-level management, far too few contemporary science papers are usable building blocks in evolving a science of forest system dynamics and management. Scientific papers on forest resource issues all contain a statement averring that "this information is essential to protecting the ecology of... ," or "is necessary for management of... ." However encouraged the manager might be by such introductions, he/she will quickly find yet another anecdotal description, outside any framework of scientific theory useful in designing management of the unique system the manager faces.

Commonly, papers are comprised of observations out of temporal/spatial context. Few address *processes* at the time and spatial scales of forest system dynamics, or are written in contexts understandable to a manager who must think about change in a forest of 200,000 ha over a time horizon of about one century into the future. Most research is not within a coherent body of scientific knowledge that is identifiable *and accessible to* a manager. Perhaps most importantly, the so-called building block papers are out of context of the dynamics in whole systems to which they might be applied (if that were possible) in a management context. Presumably, the manager is supposed to assemble the "blocks" himself/herself.

Management of natural systems involves interventions in those systems, sometimes to alter system structure, and *always* to alter system evolution. When management is undertaken, it is, by definition, with the intent of causing some part of the system to evolve differently than it would without management. Because of the costs involved, management is undertaken only when it appears that value in some form can be gained/retained, commensurate with the costs of managing.

As one who has had occasion to employ ecological reasoning, in the design of treatments of local stands in a forest and to design strategies for whole forests, I worry that "ecological science" has become an unsafe practice. Local observations at one moment are used continually as a basis for stating where whole classes of systems are going. The public, and resource managers, have difficulty distinguishing ecological science from pop ecology. That is, system state, system structure, and system function have become thoroughly confused with what an element of a local system *looks* like now.

Although science has not been of much help to the manager of natural systems, scientists are at the forefront in prescribing rules for wrong actions to avoid, and right actions to take. In most instances, this advice has been (is) generic. That is, the advice is not state dependent, in the sense of first analyzing the emerging problem in the context of dynamics in that particular stand or particular forest system, and then choosing an intervention to invoke a dynamic system response that will correct the problem. Managers wonder how scientists, who limit their study to narrow temporal and spatial scopes that are unhelpful to the manager, feel so competent to pass sweeping judgment on whole classes of management issues.

Our society is accumulating resource/environmental problems at an alarming rate, and it is usually easy to identify a "manager" upon whom to lay the blame. What is not at all clear is where such a manager might have found *useable* knowledge (in the sense of Ravetz 1986) to avoid the problem, or *useable* knowledge to design corrective actions once the problem had emerged.

Levins (1966) argued that there are three characteristics of a hypothesis, and natural resource management plans are, above all, hypotheses. The plans are hypotheses at grand temporal and spatial scales, but they *are* hypotheses. Interpreted for present purposes, the three characteristics are: (1) the precision with which the management system can be implemented and measured in time and geographic space; (2) the functional realism of actions invoked in the plan relative to function in the target system; and (3) the level of generality expected in applying the plan (interpreted from Levins). In management of natural systems such as forests, these three properties of models are mutually exclusive (Baskerville 1994). That is, anything gained in the way of precision in a model used to forecast the management design for a 200,000-ha forest tends to result in reduced generality of application in the real forest and in reduced functional realism of the plan relative to the real forest. Similarly, gains in making a model more generally applicable to an array of conditions tend to result in losses of functional realism with respect to forest dynamics, and of precision relative to any one particular forest estate to which the model might be applied.

This principle is helpful in understanding the stress between renewable resource scientists and renewable resource managers. Great precision in a forecast from a scientist's model that lacks functional realism in key areas of dynamics, *at the scale of management*, is a liability when it comes to designing management in the real forest. However, scientists are not rewarded at all for generality, and rather little for functional realism; the reward system in contemporary science seems to be locked on precision. Hence, science models rarely are usable/ informative in real resource management decision making.

I no longer expect a "scientist" to think/work/write in the temporal and spatial domains in which the manager of a forest must work. Scientists do not collect data in that context, they do not publish in that context, and they do not read much that is in that context because, *at that scale*, it cannot be *scientific* with the classic contemporary focus on precision. Equally, I no longer expect a forest manager to be troubled with the abstractly small temporal and spatial scales of the scientific literature, because these have no context in management problems at scales the manager must face.

So what to do? It is unlikely that the world of science, in which numbers and precision of publications count heavily in advancement of the scientist, will change. Assessment of managerial accomplishment is driven by the state of the forest, as assessed primarily in an economic context, and that also is unlikely to change. It would be refreshing to see more scientific literature that attempted to help the manager improve the form, function, and outcome of management at the temporal and spatial scales at which the manager manages, rather than continually reasserting an alternative paradigm that is unusable by real managers in real natural systems.

RESPONSES TO THIS ARTICLE

Responses to this article are invited. If accepted for publication, your response will be hyperlinked to the article. To submit a comment, follow this link. To read comments already accepted, follow this link.

Literature Cited

Baskerville, G.L. 1995. The forestry problem: adaptive lurches of renewal. Pages 39-97 *in Barriers and bridges: renewal of ecosystems and institutions*. Columbia Press, New York, New York, USA.

______ . 1994. Gaelic poetry for deaf seagulls; encore. *Forestry Chronicles* **70**:562-564.

Boothroyd, H. 1978. *Articulate intervention*. Taylor and Francis, London, UK.

Holling, C.S. 1978. *Adaptive environmental assessment and management*. John Wiley, Chichester, UK.

______ . 1995. What barriers? What bridges? Pages xxx-xxx *in* L.H. Gunderson, C.S.Holling, and S. Light, editors. *Barriers and bridges to the renewal of ecosystems and institutions.*

Lee, K. 1993. *Compass and gyroscope*. Island Press, Washington, D.C., USA.

Levins, R. 1966. Strategy of model building in population biology. *American Scientist* **54**:421-431.

Raiffa, H. 1968. *Decision analysis: introductory lectures on choices in the face of uncertainty*. Addison-Wesley, Reading, Massachusetts, USA.

Ravetz, J.R. 1986. Usable knowledge, usable ignorance: incomplete science with policy implications. Pages 415-434 *in* W.C. Clark and T.E. Munn, editors. *Sustainable development of the biosphere*. Cambridge University Press, Cambridge, UK.

Walters, C.J. 1986. *Adaptive management of renewable resources*. MacMillan, New York, New York, USA.

Address of correspondent:
Gordon L. Baskerville
University of British Columbia
Faculty of Forestry
270 - 2357 Main Mall
Vancouver, British Columbia, Canada V6T 1Z4
Phone: (604) 822-3362
Fax: (604) 822-9106
basker@unixg.ubc.ca