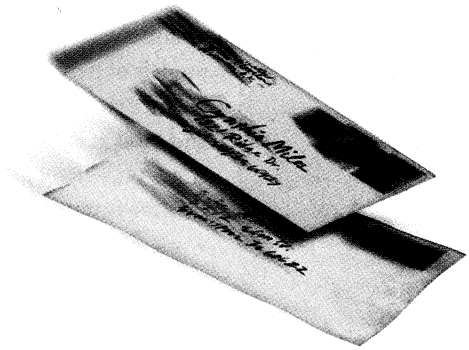
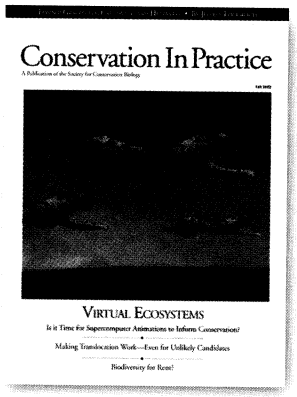
# From Readers



## Virtual Ecosystems

W. Wayt Gibbs (Fall, 2003) describes in glowing, optimistic terms a novel approach by Neo Martinez aimed at unraveling the complexity inherent in food webs. A supercomputer crunches the numbers; sophisticated programming produces glitzy graphics. It all suggests great promise for better informing conservation actions. The imagery is enhanced by portraying Martinez as an ecological iconoclast imbued with a twinge of self-righteousness.




At the risk of being charged a Luddite, I suggest a reality check. Martinez and his team fail to recognize that space and its added structure matters. The natural world is hardly homogeneous. Nature's complexity is admittedly daunting. Yet in their frontal assault to understand it, they appear to have dismissed a widely appreciated understanding that, while simple models necessarily exclude important aspects, those that are inordinately detailed may not be verifiable. One must balance complexity with transparency (1). Their models are opaque, even to themselves.

I was dismayed by the implication that ecologists haven't made much progress in understanding dynamic complexity. We have, the list is lengthy, and much of the understanding, while admittedly imperfect, does inform conservation decisions. Critical or keystone species, the pervasiveness and importance of indirect effects, per capita rates of interaction, the legion of small-scale experiments proving the dynamic consequences of altered abundance, mechanistically understood trophic cascades are but a few. All the above relate immediately to food webs. Virtual ecosystems remain a pipe dream, a fantasy world, especially when divorced from ecological reality.

Should efforts continue to develop them? Yes. Computer simulations will certainly prove essential, but a healthy and necessary skepticism should be demanded before conservation decisions are based on model predictions. The degree to which the virtual world reflects natural reality must be known. Ground truthing, in which predictions are based on known food web interactions and their consequences, should be a prior requirement.

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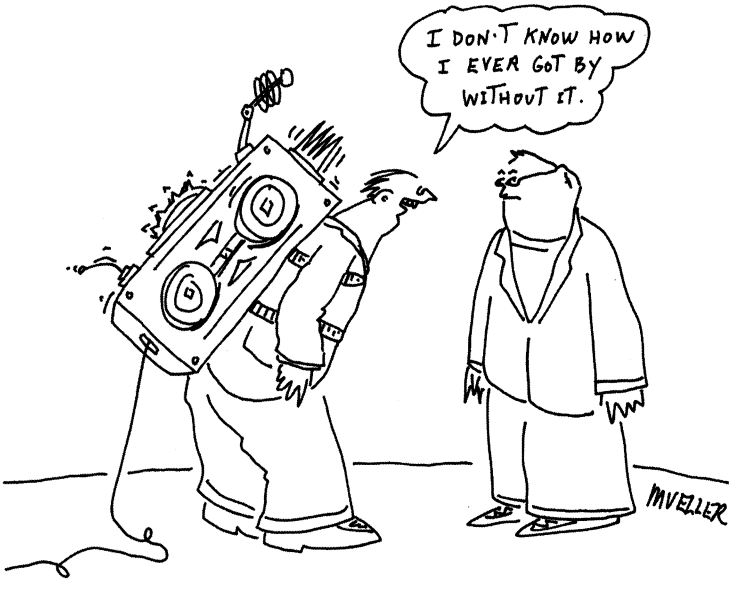
1. Carpenter, S.R. 2003. *Regime Shifts in Lake Ecosystems: Pattern and Variation.* International Ecology Institute. Oldendorf/Luhe, Germany.

*Neo Martinez and colleague respond:*

Bob Paine emphasizes many of our same concerns with *in silico* explorations of ecological complexity. There is indeed a critical trade-off between simple, general abstract models and opaquely complex, many-parameter models of particular systems. A delicate balance needs to be tuned to the task and tools at hand. We follow Yodzis and Innes (1) who cogently argue for a productive intermediate approach to network dynamics based on a relatively few easily measured parameters (e.g., metabolic rate, body size). We add network structure with a simple "niche model," which accurately predicts much of the topology of large natural food webs and is based on two readily estimated input parameters, species richness (2) and connectance (3). We also agree with, and study, the importance of considering spatial heterogeneity including prey refuges, metacommunities, and landscapes (2). While far from all-encompassing, our approach continues to build on these and other important insights including theories of body-size scaling, keystone species, trophic cascades, and nutrient competition.

In other words, we agree that modeling should be thoughtfully guided by, and consistent with, the best available theory and data, particularly as we explore how well-studied food-web modules connect into more complex ecological networks. While such modeling may have once been an unrealistic "pipe dream," it has become an increasingly powerful and transparent field-testable research program.

We only partly agree with Paine's suggestion and Gibbs' (p.18) assertion that "it can be dangerous to rely on [our models] until they have been thoroughly tested by field experiments." We also feel it can also be dangerous even after successful testing. For example, relatively simple well-tested models of keystone species can be misused to the detriment of an apparently "weak"



interactor. As Gibbs suggests, implications of model predictions should be studied before, rather than after, models inform conservation decisions. Such applications are not necessarily beneficial ecologically or ethically.

Another issue concerns whether ecology should follow other branches of biology and chemistry by vigorously pursuing rapidly advancing visualizations, informatics, and computation. Scientists have shown that these tools complement traditional experiments by enabling virtual experiments that formulate empirically-testable hypotheses while minimizing hazard and expense. We look forward to working closely with field ecologists to realize this synergy and bridge the gap between theoretical and empirical understanding. Finally, though it was beyond the scope of the Gibbs' article, we acknowledge and sincerely appreciate all the pioneers that made our work possible by illuminating key mechanisms and patterns through careful observations, ingenious experiments, and wonderfully creative theory.

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1. Yodzis, P. and S. Innes. 1992. Body-size and consumer-resource dynamics. *American Naturalist* 139:1151-1175.

2. Brose, U., N.D. Martinez, and R.J. Williams. 2003. Estimating species richness: Sensitivity to sample coverage and insensitivity to spatial patterns. *Ecology* 84:2364-2377.

3. Martinez, N. D. et al. 1999. Effects of sample effort on characterization of food-web structure. *Ecology* 80:1044-1055.

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Letters may be edited for clarity or brevity.

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