The vast majority of scientific endeavor is, of course, quite ordinary. Thus the research projects in which we engage ourselves are relatively pedestrian, constituting little more than building blocks for major advances. Of course, such "normal" science is absolutely essential in that it provides the raw empirical material for progress in understanding. Periodically an extraordinary event occurs that enables a novel breakthrough. Occasionally, this may be just a serendipitous discovery by a more-or-less "ordinary" scientist (provided, of course, that someone has the wisdom to appreciate the true significance of the discovery and the creativity to develop it). But, more often than not, major new directions are charted by rare individuals with incredible intellectual prowess. Population biology has attracted a few of these extraordinary people in the past, and ecology today stands poised, awaiting another such genius. But time's a wasting—the very systems we study are rapidly being destroyed by the press of humanity. In the words of Holmes-Rolston (*BioScience* 1985), "Destroying species is like tearing pages out of an unread book, written in a language that humans hardly know how to read." Just as ecologists are finally beginning to learn to read the "unread" (and rapidly disappearing) book, they are encountering governmental and public hostility and having difficulties attracting support. This backlash in response to rabid environmentalism is most unwise and must be changed.

In the meantime, we simply cannot afford to wait patiently for our next genius to pop up. The rest of us could benefit immeasurably from attempting to simulate inductive genius by means of think tanks. At a recent NSF workshop in Santiago, Chile, I was impressed with the very considerable insights that can emerge from interactions between even a relatively small number of ecologists with similar interests. Such "brainstorming" efforts effectively enhance our intellectual prowess. But we are too scattered around the country, too isolated from one another, to take fullest advantage of the possibilities. The duration of most meetings is far too short for interchanges to solidify into really lasting contributions. What is needed are more substantial blocks of time, say repeated meetings or meetings lasting from several months to a year, to continue bouncing ideas back and forth until the best ones pop out. This process would not only prove exhilarating for the participants concerned, but it would also greatly benefit the rest of the scientific community and ultimately everyone in the world.

For me, the most challenging and tantalizing subdiscipline in ecology is the study of communities. It is not only very abstract, but remains in its infancy. Community ecology is also very promising: major new insights lie just around the corner. But community ecology is not for the faint of heart: it is very probably the most difficult of all sciences. As the human population continues to burgeon, we are increasingly finding that we need all the ecological understanding we can possibly marshal, particularly concerning the organization and function of ecosystems. However, as explained above, there is a great urgency to basic ecological research, particularly at the community level. We desperately need to improve our understanding of how ecological systems behave. We might even find that we are not collecting the right kinds of data.

In particular, the properties of complex networks must be evaluated. This will be a most challenging task, and one that will require considerable expertise in both the empirical and the theoretical dimensions, as well as a solid coupling between them. No one person is likely to be capable of doing it alone. Topology and graph theory, while intriguing, require the simplifying assumptions that all interactions are plus–minus and can be represented as either "on" or "off." Loop analysis allows minus–minus and plus–plus interactions, but still bypasses interaction intensity. Analogous, but more complex, approaches that incorporate mutualisms as well as variable intensity in interactions need to

be developed. Horizontal patterns of connectance within trophic levels also need to be included and distinguished from the vertical ones that operate between trophic levels.

We need to convince governments and the public that they cannot afford *not* to support ecological research. In particular, a program should be organized to support think tanks in ecology, and we should begin using our capabilities to the fullest extent possible.

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