Other People's Data

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Several years ago, at an Ecological Society of America (ESA) meeting, I overheard a colleague explain his work to a stranger as "help[ing] other people find more in their data than they knew was there." Over the course of the meeting, I talked to several friends and colleagues who were in the same position, working on other people's data, helping other people answer other people's questions. Like me, these quantitative ecologists come out of good labs, have good jobs, and are not lacking for resources. Are we quantitative ecologists really spending our time answering other people's questions, and not answering—or even asking—our own? If so, why?

Quantitative ecologists are only loosely anchored by the natural history of particular systems. Even the word "systems" is a giveaway; we see organisms as realizations of ideas, not as furry, feathery, or green individuals. Many of us came to ecology from physics, or mathematics, or statistics, because we loved its ideas. If we didn't care about the organisms, we would have been content as mathematicians or physicists, but our true love was for the way that real ecological communities could embody general mathematical concepts of dynamics and variation. Our attachment to ideas gives us great flexibility, even more than other ecologists. Some of us are drawn to model systems, such as microcosms of flour beetles or plankton, where we can put ideas to searching experimental tests; others are drawn to the opposite extreme, that is, to long-term observational data from systems such as lynx populations or measles epidemics that challenge our ability to infer ecological processes from patterns. In either case, we are primarily interested in how we can use organisms to understand general principles rather than in the particular organisms themselves. This flexibility lets us pursue interesting questions wherever they lead.

These days, quantitative ecologists typically work with existing theories, either extending them or exploring how to test and apply them in the real world. Most ecologists have similarly focused on existing theories, but such conservatism is especially important for theoreticians, because we are the ones who are supposed to be asking new questions. Back before the "heroic age" of theoretical ecology (the 1960s and 1970s), ecology was theory poor; ecologists were in dire need of a framework to structure the masses of data that they had collected. The broad-scale patterns of productivity, diversity, and stability were just being established, and explanations were in short supply. Ecologists are not short of data now, either, but we have many of the ideas we need in hand; our challenge is to rigorously quantify and test them. Current hot topics in ecology—how spatial structure changes the stability of communities, why indirect interactions in food webs matter, how parasites can affect ecosystems—are important extensions of existing theory, rather than new ideas.

When the big ideas have been laid down, what is left? There are two ways to work on pure theory: elaborating existing theory or trying to create new paradigms. The former often produces elegant ideas that have little to do with real ecological communities. As for the latter, I confess that grand schemes like hierarchy theory, macroecology, and biocomplexity leave me cold, even if they turn out to be the next big thing. My guess is that some idea that is more bound to the realities of ecosystems, something that is simple and big and answers a question none of us had thought to ask, will be it. But we won't know until afterward; that's the way paradigm shifts are.

The only remaining alternative is to work toward testing theories. Ecologists have realized that testing ecological ideas often comes down to sophisticated statistics. The clean, qualitative tests that aim to reject a simple null hypothesis now seem limited to a very narrow corner of ecology. It seems almost all of the explanations that were suggested in the heroic age are true—somewhere, to some extent. Rather than devising a single experiment that determines which explanation is right, we are being forced to provide quantitative answers that describe what fraction of a particular pattern is explained by different mechanisms: "how much" rather than "which." Even more challenging, data from natural systems we care about come in incomplete, correlated, and otherwise tangled forms that require fancy statistical techniques to interpret. There are many lifetimes' worth of fascinating statistical questions. The quantitative ecologists of my generation, who would have been building simple theoretical models in the 1970s, are now practicing maximum likelihood estimation and computationally intensive statistics.

These statistical interests also push us toward other people's questions; statistics (unlike purely mathematical theory) is so useful to practicing empirical ecologists that we can easily find ourselves preoccupied with "other people's data." Although theory should go hand in hand with data, and many of the interesting questions in ecology are inspired by specific systems, focusing too much on statistics can be a trap for people like me who are in the business of interpreting rather than collecting data. As is traditional in jeremiads of this kind, we can look toward a more "mature" science (yes, physics; Loehle 1987) for answers. Another colleague once stood up at another ESA meeting and pointed out that ecologists envy physicists for their clean