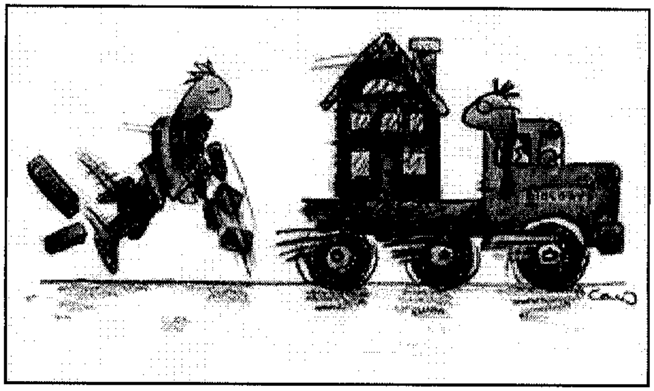
Why Ecology Lags Behind Biology

By Raymond J. O'Connor

The triumph of the mapping of the human genome is eloquent testimony to how fast the life sciences have come from the 1960s when the Nuffield Foundation found it necessary to launch a program of biological scholarships to leaven the "soft" biological sciences with expertise from the "hard" sciences of physics and chemistry. The program supported academically qualified physicists and chemists seeking careers in the life sciences, or life scientists wishing to obtain a degree in physics or chemistry to aid their career in the biological sciences.



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Yet the ecological and environmental sciences have lagged behind the surging advances of most of the life sciences. In no way can one seriously anticipate soon an ecological breakthrough of the magnitude of the human genome project. If the takeover of biological department after biological department by well-funded molecular biologists in the 1970s dismissed ecology as natural history, neither has the subsequent clothing of ecology in a mantle of hypothetico-deductive approaches yielded the expected harvest. As the National Science Foundation's biocomplexity initiative[1] is expected to pour in excess of $50 million of new money into ecological research over the next few years, it is worth asking whether money or mind-set underlies the faltering progress of ecological research.

I believe that ecologists have failed to distinguish form from substance in hypothetico-deductive research. We applaud our students when they learn to recite the mantra "My hypothesis was that ...," but we fail to fault the trivial content of the typical ecological hypothesis. In modern life sciences, as in physics, a hypothesis is typically based on a corpus of theory about how something works. It is operationally expressed as a statement of what will be observed experimentally–or observationally in the case of astronomy–if this theory is correct. Moreover, many of the critical breakthroughs in molecular biology have come from experiments that discriminate between alternate outcomes. Critically, ecology seems to have substituted the statement of what will be observed as the hypothesis. Our typical hypothesis is statistical in nature, establishing only that a pattern departs from some null model (often randomness). It is little wonder that the discussion sections of the resulting papers are so speculative.

Hypothesis statements in ecology range from the absolutely stupid to the truly insightful. The worst, statements such as "I hypothesized that breeding success would not decrease with decrease in forest patch size ...," reveal only the author's ignorance of Type II errors. Marginally informative hypotheses, such as " I hypothesized that breeding success would increase with forest patch size," can hint at process when published for the first time but are barely worth 200 lines in a journal, let alone the 20 pages typically used when reporting the 10th, 11th, 12th,... case study of the phenomenon. Additionally, and in contrast to biomedical research, such hypotheses rarely acknowledge that there may be more than one process that would yield the outcome expected, let alone provide for testing between the alternatives.

Scarce in ecology are hypotheses that quantify anything in advance ("I hypothesized that breeding success would increase linearly with the proportion of forest patch that lay more than 300 meters from an edge, 300 meters being the average distance of penetration by predators into forest"). Truly rare in ecology are hypotheses that start from theory of the particulars of processes and develop unequivocal quantitative predictions. An exceptional example is the model[2] of how the fractal geometry and hydrodynamics of branching resource distribution networks predicts universal allometric scaling in life span and fecundity, in energy and resource use and territory size, and in population dynamics.

One cannot read far into articles in the biomedical literature without encountering a specific of a process–the SP1 transcription factor, the U12 spliceosome–whilst in ecology the terms are generalities often imported from everyday life–habitat corridors, sink populations, competition. The Oxford ecologist Charles Elton would be as home with today's ecological terminology as he was in the 1930s.

The fundamental difference between the successful life sciences and the environmental sciences is, I submit, that life science and biomedical research has developed ideas about how things work whilst ecology has retained a focus on what is. The practitioners of ecology still have a building-bricks-world view of science–the idea that any piece of work in science is like a brick added to a pallet from which a few outstanding architects and builders will construct the walls, and then buildings, edifices of scientific understanding. What is missing with this mind-set is the synergy that has innervated biomedical research. Modern biomedical research is a turmoil of competing ideas, each with a logic that is rapidly followed through to yield predictions inconsistent with those derived from the competitors. Behind this sea of ideas is also a community of expertise providing solutions to the technical problems thrown up in experimental testing between such predictions.

Some solutions, such as the development of PCR, are breakthroughs of first order, akin to the discovery of a new phenomenon in physics; others, such as the development of a particular knockout mouse, are specific applications of known principles, akin to engineering achievements. Often straddling multiple streams of such activity are the innovators, scientists with the breadth of knowledge to be able to unite conceptual advances in diverse fields at critical junctions. If science has building bricks, progress lies in identifying the keystones. In contrast, too much of field ecology is what I label as "Me too!" research: Too often, despite there being dozens of previous studies of deer diets in other states and in multiple seasons, a lengthy ecology paper begins, "The fall feeding habits of white-tailed deer in Maine have never been studied, so I ...." Confirmation of previous studies is desirable, and some idea of the breadth of a phenomenon across taxa is informative, so such work perhaps deserves reporting as a short note. The norm in ecology, though, is yet another long paper, typically padded with what is little more than paper on topic X for species Y in region Z to join the dozens already published on the topic and species.

It may be argued that the reproducibility of physiological and molecular processes has allowed the biomedical research community to develop "hard" laws equivalent to the laws of physics. The implication is that ecology at large has to cope with excessive variability in those observations possible, aggravated by restricted ability to conduct experiments. Yet astronomy cannot conduct experiments but still manages to advance the testing of rigorously formulated ideas about the nature of stars and the origins of the universe–ultimate explanations rather than trivial statements about patterns to be observed.

Additionally, in biomedical research, one finds that a clear distinction between measurement error (instrumental error) and variability among the cases in a sample is maintained. Even more telling is the scientific practice universally evident in the rare case histories of outstanding success in ecology. Our understanding of the population dynamics of parasitoids, for example, owes much to the exemplary approach of Michael Hassell in Imperial College in England: His long history of alternating his focus between modeling (to determine which component of then-current ideas about population dynamics generated most uncertainty about outcome) and laboratory experiments (to parameterize the effects of that source) yielded a comprehensive understanding of parasitoid dynamics.[3]

Contrast that with our abysmal understanding of fishery population dynamics for which contemporary management practices rooted in current ecological understanding has seen every major fishery globally drift down the food chain.[4] Lest laboratory experiments seem too close to biomedical work to be a fair representation of ecological research, consider the success of the long-running field experiments of David Tilman at Cedar Creek in bringing order to understanding of plant population dynamics.[5] And if even field experiments are too controlled for pure ecology, consider the long-term observational studies of Richard Holmes and Thomas Sherry at Hubbard Brook on the population ecology of black-throated green warblers and America redstarts.[6] What each of these studies (and their counterparts in other outposts of quality ecological work) shares with biomedical research is rigorous analysis and logical thinking: Each piece of each of these investigations provided *immediate* critical feedback to understanding the ecological processes involved.

Each of these researchers has forsaken the building-bricks-of-science approach to substitute "How?" and "Why?" questions for the "What?' inquiry that dominates contemporary field ecology. It is in this consideration and dissection of how things might work, and of why they should work in that particular way, that effective ecology must converge on the successful practices of biomedical research.

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