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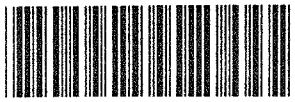

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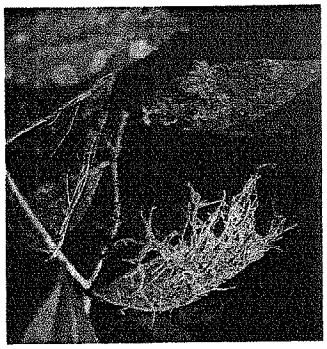

Cover Photo: Liverworts growing on leaves at 1200 m in Panama. Epiphylls, primarily liverworts and lichens, colonize leaves in tropical forests. At humid sites, liverworts dominate, and can completely cover leaves in 2 years. Liverworts may be detrimental to host leaves as they reduce the light reaching the host leaves by 55–85%. However, host leaves apparently can defend themselves against colonization. Data on six host species show that species with longer lived leaves have slower rates of epiphyll colonization as well as lower accumulated cover throughout the entire leaf life-span. This photograph was taken while Phyllis D. Coley and Thomas Kursar of the Department of Biology, University of Utah, and Jose-Luis Machado of the Smithsonian Tropical Research Institute, Balboa, Panama were carrying out research to be published in *Ecology* **74**(2), March 1993 as "Colonization of tropical rain forest leaves by epiphylls; effects of site and host plant characteristics."

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THOUGHTS ON A REVIEW OF A CRITIQUE FOR ECOLOGY

Some years ago I was fortunate to be able to teach a short intensive course on competition at the University of Uppsala (Keddy 1989). I wanted to include a series of discussions around key papers. One discussion was to centre around Rob Peters' 1980 paper, "From Natural History to Ecology" (Peters 1980). In it Peters draws a clear distinction between natural history and ecology, and explains how many of the problems and attitudes that bedevil our discipline today arise out of failure to understand this difference. But during our discussion, I was astonished to find that we never seemed to have the opportunity to discuss his thesis, because a majority (nay, a near unanimity) of the students only wanted to criticize the paper. I spent too much of the hour defending Peters, rather than leading a discussion on the pros and cons of his views. The most frustrating (and puzzling) aspect is that I am still unable to articulate what it was that the class objected to—it seemed that in some vague way they felt affronted by his paper, and that they wanted to express their annoyance at this. Let it be clear, by the way, that I am not criticizing those students—I appreciate their honesty—it is just that their reaction caught me totally off guard, and I am still surprised that I am unable to grasp what their difficulty was. And they are probably puzzled about what I saw in the paper.

I was reminded of this incident when I read a recent review of Peters' book (Lawton 1992). Yes, for all those who missed the 1980 paper, Peters has now presented us with a book (Peters 1991) that deals largely with this theme. In his insulting review, John Lawton calls the book "disappointing, uninspiring, negative" and compares it to "reading an essay written by a dreadfully earnest, but ill-informed, poorly read undergraduate, an essay needing copious red ink on every paragraph." I was frustrated by this insulting, disappointing, uninspiring, and negative review of what is a remarkably interesting book. But although the Uppsala students were far more polite than John Lawton, they too seemed to share this fundamental barrier to grasping the ideas Peters presents. Why should this be the case?

The book itself has 12 chapters dealing with important basic issues such as operationalization, causality and reductionism, explanation and understanding. In the first part of the book Peters covers a great deal of ground, and tries to lay out his criticisms of the practices of modern ecology. Surely there is no one who will not sympathize with some of the frustrations Peters brings to our attention, particularly the criticism that many of the questions posed by ecologists are inherently unanswerable. (At the same time, there are irritating parts of his analysis. While we may agree that there are problems in the way we conduct ecological research, I was not convinced that low citation rates, or calculations of journal impact, or journal rejection rates, are in any way meaningful indicators of the foregoing problem. Such examples, if anything, detracted from his thesis.)

Having laid out his complaints, Peters proceeds to offer advice on how to do things differently. He emphasizes the pitfalls of infinite regress in the search causality, the importance of prediction as a goal of ecology, and regression techniques as a predictive tool. He illustrates both his criticisms and proposals for remedial action with a wide array of examples.

At this point, it may be useful to distinguish between the medical terms diagnosis and treatment. Peters offers us both. When one is seriously ill, if one fails to notice the symptoms, or if one is laboring without even a diagnosis, treatment is impossible. Recognition of the illness and diagnosis are essential first steps. But accurate diagnosis does not necessarily guarantee that treatment is possible. And there is still plenty of room for argument about how best to treat the illness. In this context, Peters has laid out a series of troubling symptoms (which, if we exclude his questionable analysis of the publication process), boil down to (1) research programs being driven by unanswerable questions, and (2) research techniques being inherently unable to answer the questions they are allegedly addressing. He offers a tentative diagnosis, and a possible treatment. The latter of course, for those who have practised ecology without reading Peters' earlier paper (do such people exist in ESA?), is an increased emphasis upon prediction as a goal in ecology. Now before we discuss this prescription, I suggest that his diagnosis is a service in its own right and justifies reading the book. Anyone familiar with medicine will know that we cannot begin to treat an illness until we recognize it.

Now to his prescription: an increased emphasis upon prediction. There is obviously room for debating this matter further, but as a first approxi-

mation, it has a great deal of merit. Engineers can build bridges and ICBM's. Physicists can build bombs and solar panels. Chemists can make napalm and plastic. In each case their knowledge leads to obvious outcomes—and if they are wrong, the bridges fall down, bombs fail to explode, and so on. But what of ecologists? What, if anything, are outcomes of our work, and how would anyone know if we were wrong? This is where prediction surely has its merits—if nothing is predicted, we have no way of separating things that work from things that do not. Perhaps one of the reasons that the public still often find us irrelevant is that they sense that our discipline does not yet make predictions strong enough to matter, that is, strong enough to be proven wrong. And if we cannot be proven wrong, there is no filter of failure to remove the bad work from the good—just accumulated papers sorted by political intrigue. While prediction has its merits, I am not as confident as Peters appears to be that regression techniques are the only or best tool for doing so. But this is quibbling. The doctor has described the symptoms and suggested a diagnosis, so now the debate about treatment can begin.

This book is therefore must reading for all graduate students and professors who practice ecology. This does not mean we have to agree, or even like what he says or the way he says it. But it does mean we have to do ourselves the service of at least thinking about his ideas. Once you have done so, you may wish to assign yourself to one of the following three categories.

One of three categories, you say? Yes I have still been reflecting upon the polite students of Uppsala, and the impolite John Lawton. How do we explain their reactions, or, to put it in terms which Peters might approve of, what attributes would allow us to predict the responses of readers to his book? The answer may lie in personality types and political styles. Political styles have a considerable impact on our discipline, and are a complicating factor in interpreting the scientific literature in ecology. Some of the peculiarities in the current literature on competition illustrate this (Keddy 1989). But let us consider an historical example. Peters' approach reminds me of Christ chasing the money lenders out of the temple. He is outraged, he wades in, he knocks over their tables, he flogs them, money falls everywhere, people are shouting—the confusion and shock as everyone spilled out of the temple must have been something to see. Righteous indignation at its very best! Now, what would your reaction have been?

Group 1: Enthusiastic support. If you had a sense of what was appropriate in a temple, you probably thought that those money lenders had probably got away for years with committing sacrilege and that they therefore got what they deserved. Besides, even if it was outrageous, it was fun to watch.

Group 2: Anger. If you were one of the money changers, you were undoubtedly angry, and complained to the authorities and demanded at very least an execution—which, by the way, was the final result. It may have been that those in power were more frightened by Christ's attack on financial power than by his preaching. To this group, maintaining power was essential, even at the expense of public crucifixion.

Group 3: Puzzled irritation. These people (probably the largest group) would not necessarily agree with the money lenders, but they would be so unnerved by the impropriety of Christ's actions that they could not stop to think whether he was right or wrong—they could only express shock that the norms of society were being violated. To this group, it is more important to maintain social norms than try to correct injustice.

Or to put a more contemporary American spin on it, how did you react to the riot at the Democratic convention in 1968? If you thought it was a despicable use of police provocation and violence, you will probably like Peters. If you thought the police should have beaten more demonstrators, you probably will not like Peters. And if you were part of the masses who watched on television and wondered what the fuss was about, or blamed the students for stirring up trouble, well, then you may be irritated by Peters' effrontery.

Since humans are the complex organisms we are, most readers will likely find themselves cycling among all three views, depending upon which part of the book they are reading and how close it is to their own work! I would use this book in graduate courses precisely because it demands that we think about what we do. It is a subversive book because it demands that we rethink commonly held assumptions (such as blind worship of the deity of reductionism), and challenges the view that the ability to tell entertaining explanatory stories is the ultimate goal of ecology (and Ecology). I do not begin to agree with everything Peters says, particularly his views on evolution. But if our discipline is going to thrive as a science, surely we can cherish subversives such as Peters rather than condemning them.

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LIFE HISTORY STRATEGIES AND POPULATION BIOLOGY IN SCIENCE FICTION FILMS[1]

Since Georges Melies first amazed audiences with his "Trip to the Moon" in 1902, science fiction filmmakers have liberally borrowed from (and built upon) the research findings of legitimate scientists to add an air of verisimilitude to their films. Practical applications of technological advances are often depicted in films sooner than they are developed in the real world. Genetic engineering is commonplace in films (e.g., cockroach genetic engineering in the 1987 feature film, "The Nest") despite the fact that, to date, no successful genetic transformation of any insect other than *Drosophila melanogaster* has been conducted (Rubin and Spradling 1982). Groundbreaking discoveries in scientific disciplines as disparate as physics and molecular biology have been faithfully rendered in a fictional manner in films, and in fact often appear with greater rapidity than they do in the refereed scientific literature.

Despite the fact that factual elements are routinely incorporated into science fiction films, these elements are frequently distorted or exaggerated, generally for dramatic effect—science fiction is perforce fiction. That such liberties have been taken by filmmakers has been gleefully pointed out by scientists and educators for years, often for reasons of personal aggrandisement (e.g., *The Star*, 3 March 1989).

It is somewhat puzzling that the scientific subdiscipline perhaps least often incorporated thematically into science fiction is population biology. Science fiction filmmaking and population biology (particularly life history theory) have developed in parallel throughout this century. Melies basically created the science fiction genre in 1902 (Baxter 1970); Lotka introduced the concept of the stable age distribution and his equations for population growth in 1907 and 1913, respectively (Hutchinson 1978). Life history theory was certainly dramatic in the manner in which it was presented by its developers. (Take, for example, Gause's 1934 classic on competition, flamboyantly entitled, "The Struggle for Existence" and Pearl's 1922 "The Biology of Death."

According to Stearns (1976), "By 1954, the main ideas of life-history theory were well-defined." Thus, a broad range of population processes was available to science fiction filmmakers at midcentury. Population biology should have been of particular interest to filmmakers at this time. By 1950, a new theme came to dominate science fiction films—the idea of invasion by hostile forces. This theme is attributed by film historians at least in part to the aggressive imperialism of Nazi forces during World War II and Cold War paranoia about the Soviet sphere of influence immediately after the war (Brosnan 1978). Many films are thinly disguised cautionary tales about the evils of communism. It is therefore all the more puzzling that the considerable body of information accumulated by at least two generations of population biologists on the genetics and life histories of colonizing species does not appear to have been consulted in the scripting and production of these films.

As early as 1925, the idea that species-specific birth rates are adaptive characters influenced by natural selection (Stearns 1976) was well established. MacArthur (1960) described a set of syndromes reflecting the influence of opposing selection on life history parameters. So-called

[1]Based on a plenary talk given at the 1991 Midwest Population Biology Conference.