

bereft of much of its ecological interest. At least there is one picture of acacia woodland that contains giraffes.

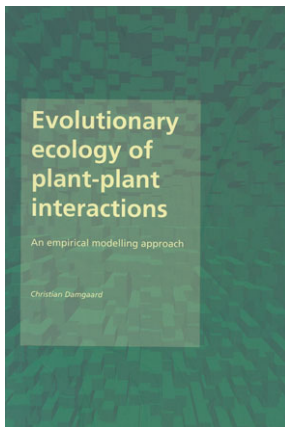
The book concludes by looking at ecological questions on a global scale, such as the causes of latitudinal gradients of diversity and the likely consequences of climate change for plant communities of the world. The problems and questions facing ecologists are considerable, but their solution, I fear, is not made easier by ignoring the vast majority of the living species occupying the planet and

by concentrating only on the plants. This volume is extremely well produced, rich in attractive illustrations, and supplied with an interesting and informative text. My one very major reservation is my lack of conviction that plant ecology can be studied in isolation from the greater whole.

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**Evolutionary ecology of plant-plant interactions: an empirical modelling approach.**

Damgaard C. 2004.  
Aarhus, Denmark: Aarhus University Press.  
€26.95 (softback).  
151 pp.

*Why did looters leave the books in Wal-Mart?*

Let me begin this review with three short digressions. All address the value we place on

books and literacy, and also, perhaps, the success with which we can transmit knowledge to future generations.

In autumn 2005, after Hurricane Katrina smashed southeastern Louisiana and flooded the area around our home, we became house guests in a small Louisiana town. While we sweated through the heat and humidity, with no power and falling water pressure, we heard rumours of widespread looting in New Orleans. Allegedly, mobs were not only stripping stores of food and water (which was perhaps forgivable under the circumstances), they were also taking everything else, wading out the doors through the flood waters with new running shoes, televisions, guns and dresses. There was an element of black humour in all of this, however, because, according to our sources, one item was invariably left untouched – books.

Last autumn, a course in ‘Mass Communications’ followed my ‘General Ecology’ class at Southeastern Louisiana University. When the arriving mass communications students saw that I was requiring every student in my ecology class to read a book, they asked for my name. I was touched – until one of them explained, ‘I want your name so I can make damn sure I don’t sign up for any of your courses’.

According to a recent survey of the National Endowment for the Arts (2004), *Reading at risk: a survey of literary reading in America*, the proportion of American citizens who have read a book has fallen precipitously. In their words (p.vii), ‘literary reading in America is not only declining rapidly among all groups, but the rate of decline has accelerated, especially among the young’. Their data show that between 1992 and 2002 the number

of Americans who have read any book has declined by 7%, while the number who have read any literature has declined by 14%. The highest rate of decline is found in the youngest age classes.

I thought of attitudes toward reading and writing of books (and scholarly papers) as I wrestled with this review.

First, the bare facts. This slim volume offers seven chapters and four appendices on plant-plant interactions. Since the first chapter is an introduction, there are actually six chapters, titled: Individual Plant Growth; Demography; Interspecific Competition; Genetic Ecology; Natural Selection; and Evolution of Plant Life History.

Since there have been so many books, monographs and papers written about plant competition and plant-plant interactions in general, particularly over the past 50 years, I was curious about the author’s intentions. How was he going to build on previous work, and what new insights had stimulated him to write a book? (Previous monographs, to name only a few, have included *Practical plant ecology*, Tansley, 1923; *Plant competition*, Clements *et al.*, 1929; *Quantitative and dynamic plant ecology*, Kershaw, 1973; *Mathematical ecology*, Pielou, 1977; *Population biology of plants*, Harper, 1977; *Plant strategies and vegetation processes*, Grime, 1979; *Resource competition and community structure*, Tilman, 1982; and *Competition*, Keddy, 2001.) In the Preface, the author says ‘... there has been a strong tradition in plant ecology to describe different plant communities and succession processes in a qualitative way’ and refers to the ‘lack of communication between field ecologists and mathematical modelers’. The irony is that the first quote repeats almost word for word from A.G. Tansley, 1923 and J.L. Harper, 1982 – neither of which is cited – and the author appears unaware of either their critiques or their suggested remedies. The proposal to promote communication actually struck me as insulting, since the remainder of the book is dismissive of plants and plant ecologists.

The introduction ignores most of the advances in the past thirty years. I recalled my days of graduate school in the 1970s in the library at Dalhousie University in Halifax, reading John Harper’s essays such as ‘*A Darwinian approach to plant ecology*’ (Harper, 1967) and, later, ‘*After description*’ (Harper, 1982), as well as trying to work my way through E.C. Pielou’s efforts to bring models into plant ecology (e.g. Pielou, 1975, 1977). But

Damgaard's book is dated 2004. I flipped to the literature cited. As is typical of the book, there is only one reference to Harper, his encyclopedic *Population biology of plants* of 1977. Harper's fine book may be canonical (Keddy, 2005a), but the fact remains that, rather like the Bible, it is also conveniently long enough that it can be cited whether or not you have read it. Perhaps we should insist that authors cite the page and line references to indicate that they did more than scan the table of contents. The author might instead have read some original papers on plant–plant interactions (Harper, 1965) or demography (Harper and White, 1974), but did not. In spite of the homily about communication with mathematical modellers, there was no reference to any of E.C. Pielou's papers, let alone her books, all of which, I understood, had the express goal of bringing quantification and models into plant ecology.

Everywhere I tried to read, I found more of the same. I do not want to go through each chapter in the same way, and will further examine just the introduction and two other chapters where some striking issues arise. Consider the general treatment of models in the introduction. We are told there are two types: mechanistic and empirical. For the former, there was only a single paragraph in the introduction, and of the five cited references in this paragraph, Tilman, Pacala and Levin appear in four. I have written elsewhere (Keddy, 2004, 2005a, b) about how this small cadre has cultivated the illusion that no substantial models existed before their efforts. This book repeats the tale.

More surprising is the treatment of empirical models (page 15) – a treatment lacking any citations *whatsoever!* I really do not understand why anyone would write about empirical models while ignoring the entire field. Where do I begin? I have written repeatedly about the value of empirical models (a few examples: Keddy, 1987, 2001, 2005b), drawing attention to earlier work by Rigler and Peters (Peters, 1992; Rigler, 1982; Rigler and Peters, 1995). Indeed, the word *empiricism* is even in the title of Rigler's paper. One can trace the origins of this approach back to philosophical roots in pragmatism (James, 1907), while reviews like Aune (1970) explore empiricism and pragmatism as tools, and newer books open the door to new realms of quantitative empirical inquiry (Shipley, 2000).

Chapter 2 addresses individual plant growth. Although the back cover warns that 'the biological information underlying the discussed models is only briefly discussed', I was still unprepared for the superficiality. The first section, strangely called 'competitive growth', introduces the idea that plants need resources and that they can negatively interact. The sole introduction to the literature in this field is a little-known book chapter by Begon. Apparently students need not read (Larcher, 2003), or (Levitt, 1980), or (Morowitz, 1968), nor be aware that all three used models. The figure that presumes to summarize competitive interactions refers to no other literature, and is marked 'Figure provided by Jacob Weiner'. Several of the other examples in the book come from Professor Weiner. While it may have been kind of Jake to donate work to the project, might he not have suggested reading say, my own monograph (Keddy, 2001), which has a 71-page chapter (pp. 333–404) on models for competition, or work by

other authors who have built and published models for plant–plant interactions [Pielou, Shugart, Givnish, . . .]? Given the importance of forests, the absence of the vast literature on models for plant–plant interactions in forests (e.g. Urban and Shugart, 1992; Shugart and Smith, 1996) is particularly noteworthy. Instead of trees, many examples use *Arabidopsis*.

Finally, consider the section on environmental gradients (two pages in Chapter 4, Interspecific competition) – a theme in my own work for some twenty years. Although the author cites a single, barely relevant paper on which I am first author, the rest of my gradient work is ignored. The book offers a model for plant competition along a gradient – but neglects to mention that E.C. Pielou built such a model thirty years ago (1975, pp. 90–99, 'Modeling Competition on a Gradient'), even though that same model reappears in *Competition* (pp. 351–354). I considered re-reading Pielou's model to try to sort out how it compared to Damgaard's model, either with respect to objectives or mathematical structure, but decided that such comparisons were his job, not mine.

The last paragraph of the Preface says that this book was first written as part of a doctoral thesis, and the author tells us that it is 'a reflection of my own views rather than a balanced account of the field and the cited references are somewhat biased towards my own production'. Far from placating me, this just raised further questions about the book, not to mention the attitudes and standards of scholarship accepted by a committee, external examiner, university, and publisher. Properly placing a contribution in the historical context of a discipline is absolutely essential for evaluating its significance and advancing the discipline.

Hence, this book, combined with the incidents mentioned at the beginning of this review, have made me reflect on current attitudes toward books and scholarship in general. We have all, of course, missed occasional important references in our published work, and as the number of journals increases, such errors will most likely continue. The challenge for younger scholars must be daunting. But surely that is no excuse for simply ignoring the field and making it up as you go along. Perhaps it is post-hurricane stress, from which it is said we are all still suffering – but what, I have been asking myself, is the difference between looters who ignore the books in a flooded Wal-Mart and writers who ignore the books in an air-conditioned university library? Do these both reflect the same trend uncovered by the National Endowment for the Arts? Such thoughts led me to a broader question, the difference, if any, between a person who is *unable* to read and a person who is simply *unwilling* to read.

Whether or not you agree with my ruminations on literacy, let me return to why this book, and others like it, merit concern. The knowledge we currently have about plants and plant communities has been painstakingly gained by the hard work of generations of committed scholars. Many have already died; more are now nearing retirement. They have left us their herbarium specimens, data, papers, and books. This accumulated knowledge is already at risk from forces outside our field, such as increasing emphasis upon molecular biology, limited

employment for plant systematists and field botanists, and lack of financial support or professional credit for collecting plants and maintaining herbaria (e.g. Held, 2004; Prather *et al.*, 2004). My canonical list of readings in plant ecology (Keddy, 2005a) was motivated by the intention to counter some of these forces. Simultaneously, plants and their habitats are disappearing.

At best, new and superficial publications mislead students and erase history, while diluting the quality of our libraries (and increasing their costs). By weakening our discipline from within, they increase susceptibility to the external forces mentioned above. The threats to the disciplines of botany and plant ecology (e.g. Held, 2004; Prather *et al.*, 2004) may be more significant than we realize. Two recent books (Russo, 2004; Wright, 2004) have used very different sources and arguments to come to a similar conclusion. Both authors suggest that we, as members of a culture that has seen enormous material progress, tend to assume that progress is inevitable and automatic – when in fact there are also historical precedents for rapid decline. We may not realize just how quickly the work of generations can be lost. Hence, the successful transmission of knowledge in botany and plant ecology to future generations (as in any other professional field) absolutely demands an attitude of respect for existing knowledge and for the people who produced it.

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