Illuminating, however, is the content that is absent from this volume. While providing a strong argument for an integrated and comprehensive framework to guide ecosystem management, papers tend to be niche-oriented and sectoral in focus. In large part, this may be a product of necessity. However, it does not account for a lack of analysis and discussion on the socio-economic, institutional, and policy issues identified in introductory papers as a fundamental aspect of ecosystem management. While good science is fundamental to ecosystem management, greater emphasis on its role as a framework for planning and decision-making is required.

Despite this omission, *Ecosystem Management* provides an insightful, if not comprehensive, assessment of the current status and understanding of the concept and its application in complex contexts. While the focus and examples are strictly oriented to the United States, many of the articles will inform the Canadian scientist and manager interested in the topic; especially those involved in the management of our forests and their wildlife resources.

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Null Models in Ecology

By N. J. Gotelli and G. R. Graves. 1996. Smithsonian Institution Press, Washington. 368 pp., illus. U.S.$30.

Field naturalists are a pragmatic bunch of people. We learn our natural history one step at a time as we familiarise ourselves with nature's rules. Bogs are where we find Pitcher Plants. Lakes have White Water Lilies. Deciduous forests have Black-throated Blue Warblers. Ephemeral ponds in deciduous forests have Blue-spotted Salamanders. Often we acquire this knowledge from an older more adept field naturalist who shares with us the rules acquired over many years of practical experience in the field.

Good naturalists may know where to find things; professional ecologists, in contrast, are sceptics. They will observe that not all bogs have pitcher plants, not all lakes have White Water Lilies, and not all forests have Black-throated Blue Warblers. Are naturalists therefore discovering rules of nature, or simply inventing convenient stories to enrich their lives? A good naturalist may be equally sceptical of this scepticism: perhaps the inability of professional ecologists to find patterns arises out of their ignorance about nature. Too much time in the library and lecture theatre, and too much specialisation, means, perhaps, that the professional's scepticism is just a convenient way to avoid admitting ignorance.

Yet, the sceptic's view may have its merits: how many of us have gone to a habitat that "looks perfect" and yet failed to find the species that we sought? Our rule of pattern has been challenged. Do we then have to explain it away with an added fact? Past grazing? A fire? No dispersal? No pollinators? Over-collection? Acid rain? Climate change? Because it is so easy to always come up with an explanation for pattern, or the lack of it, professional ecologists have erected a rather intimidating series of rules for the evidence that must be provided to demonstrate that a pattern is real.

The general procedure is that one must first have a *null hypothesis*, that is, a description of how the world would appear if a certain pattern did *not* occur. Then one collects real data from the field. Only if these real field data depart "enough" from the null model, do we accept the pattern exists. This of course, is just a rephrasing of the scientific method. It seems straight-forward. Yet, over the past decades, there has been a surprising amount of acrimonious and divisive debate about how one constructs null models.

Before we move on to the book in question, one more piece of background is necessary. The construction of null models is not as simple as we might first suppose. Consider the question: is there pattern in the ecosystems of Algonquin Park? Depending upon the null model, we might answer either yes or no. If the null model assumes that all species can occur anywhere, then almost certainly we will reject the null model, it being unlikely to find fish nesting in the trees or birds living in the bottom of lakes. But if the null model, instead, proposes that all lakes will have the same kinds of fish, or all stands of deciduous forest the same spectrum of birds, we may well find it much more difficult to reject the null model. Most lakes and deciduous forests in Algonquin Park, do, after all, have similar species. In this case, we have used our existing knowledge of nature to create a biologically real null model. But the more biological realism we put into the null model, the more likely that nature will fit it! Thus, professional ecologists have become stuck upon the issue of how much real biology ought to be put in a null model. If we construct a very realistic null model, should we be surprised when we find that nature conforms to it? And if nature does conform to a realistic null model, are we therefore justified in concluding that there are no patterns in nature?

Given this background, I was most interested to see how these topics might be covered in *Null Models in Ecology*. Gotelli and Graves take us through many of the main topic areas in which null

models have been discussed: species diversity, relative abundance, niche overlap, size ratios, co-occurrence, species-area relationships, biogeography, and food webs. In doing so, they cover familiar ground for many of us. Perhaps part of the reason for the familiarity can be found in the introduction: ". . . we contacted a number of colleagues and asked them to send us reprints and give us their perspectives on null models in ecology." Many of these are the same American zoologists who have dominated the literature on this topic for the past twenty years. The book does an admirable job in covering the perspectives of this group of scientists.

The task of an author, however, is not just to do the easy work and summarise the work of a few colleagues, but to provide a fuller perspective on the discipline, particularly drawing attention to broader issues of historical context, overlooked work, and de-emphasising familiar and perhaps over-worked examples. Here Gotelli and Graves have let us down.

Canadians will have a particular perspective on this failure. Although Gotelli and Graves say on page 1 that the term null models was coined by two Americans, Colwell and Winkler at a conference Florida in 1981, in fact the word goes back at least a decade earlier to the pioneering work of a Canadian ecologist, Chris Pielou. Pielou's work, summarised in two books in 1975 and 1977, is full of descriptions of "models" that provide "null hypotheses" for the structure of ecological communities. She follows in a long lineage of prominent ecologists who have studied plant communities along gradients (e.g., Tansley, Clements, Ellenberg, Gleason, Whittaker . . .) yet her work has been routinely and consistently overlooked by the American null models school, not even being cited in many recent so-called reviews (see Jackson 1981 for an independent critique). Gotelli and Graves at least cite some of Pielou's work in passing, and call her a pioneer, but after this bit of foreplay, we are left disappointed. Indeed, they do not even seem willing to take personal responsibility as authors for the obvious conclusion that her work has been overlooked; when this opinion is expressed, they invariably credit Simberloff and Connor for the insight. You will note that the list of topics in the preceding paragraph does not include gradient models of plant communities, yet this is the area in which an entire discipline of null models and tests has evolved, largely with work by Canadian scientists. Any responsible history would begin with a chapter on this topic. The first application of Pielou's null models was by Pielou and Routledge (1976), examining patterns in salt marshes from Nova Scotia to Manitoba. Gotelli and Graves briefly describe the work (on page 246) in the sort of detail you would copy from an abstract, but they fundamentally fail to explain, or perhaps even understand, the significance of this work in the analysis of communities. Further, they irritated me personally by ignoring the only two other studies of vascular plants which have used Pielou's methods — my work on lake shores (Keddy 1983) and Shipley and Keddy's (1987) work in marshes. All three of these studies found important similarities in the way in which plant communities depart from null models, with significant implications for debates about community organisation. Further, Shipley and Keddy spent some time discussing the problem of erecting and testing null models in plant communities. None of this is discussed by Gotelli and Graves. Indeed, Pielou's entire body of work on gradients and null models is tucked way in the second last chapter on biogeography. One gains the strong impression that Gotelli and Graves already had written their book from their friends' reprints, and then discovered Pielou's work by secondary sources. Rather than expend the necessary effort to read her books and revise their manuscript, which would have meant rewriting at least the entire first chapter, they apparently decided to take the easy way out and stick with their biased history (null models as a magnificent discovery of American zoologists) with some short sections on Pielou as an afterthought. Readers deserve better.

In the final chapter of my book *Competition* (Keddy 1989), I discuss the degree to which a small group of ornithologists have hijacked the field of community ecology, in part by ignoring the developments in ecology that took place earlier in the century. (Needless to say, this book also is not cited by Gotelli and Graves, even though they talk a good deal about competition and pattern!) Their perspectives on null models adds to the accumulating evidence that the biggest trouble with community ecology since MacArthur has been ornithophilia: the inordinate emphasis upon a small colourful group of organisms that may be a fine hobby but are relatively insignificant when measured by criteria such as biomass or number of species (This is, after all, a planet of insects and plants). Perhaps it is instructive that Graves is a curator in the Division of Birds at the National Museum of Natural History, Smithsonian Institution.

In conclusion, naturalists will probably not care all that much about null models, although the book would provide a salutary reminder that not all patterns we claim to see may really exist. Practising ecologists may want to have copy upon their shelves because the book does illustrate the status quo in the discipline, blemishes and all, but I still would refer serious practitioners to Pielou 1975 and 1977. Authors of books are duty bound to cover their topic responsibly, and I felt both saddened and frustrated that Gotelli and Graves missed an important opportunity to unify ecology and set the historical record straight.

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MISCELLANEOUS

John Muir: Apostle of Nature

By Thurman Wilkins. 1995. Oklahoma University Press, Norman. xxvii + 302 pp., illus. U.S.$24.95.

In this most recent volume of the series Oklahoma Western Biographies, Wilkins provides an excellent account of the life, work, and contributions of this towering conservationist. Muir was a complex and contradictory individual, with an early childhood in Scotland until his tyrannical father took the family to Wisconsin. Muir's early initiatives included a diversity of curious inventions, university studies in botany, and a deep attachment to the first of his several surrogate mothers. As a pacifist, he avoided the draft of the Civil War by botanizing in Ontario, followed by work in saw mills in Indiana. Muir's ever deepening interest in nature led to his thousand-mile walk from the Midwest to the Caribbean. During his famous first summer as a shepherd in the Sierra, Muir reacted to the scourge of the sheep as "hoofed locusts" but responded to the dramatic scenery with a pantheistic spiritualism. Millwrighting in Yosemite enabled study of the impact of glaciers (amid some dangerous mountaineering) and subsequent geological controversy. Muir's efforts brought contact with scientists, offers of jobs, and academic recognition. He expanded his travels in the West and North, and became more serious and successful about his writing and speaking. Muir's marriage, two daughters, and horticulture at the family ranch became large facets of his life. Beyond his conservative ethic, his lasting specific contributions involved Yosemite National Park, the National Forestry Commission, protection of the petrified forest, and the Sierra Club, of which he was long first president.

Accompanying Wilkins' clear and flowing narrative is incisive analysis on issues such as the contrast of Muir as mystic (although not a transcendentalist) yet man of action, his effectiveness in legislative action for conservation, and the influence of his father. Wilkins appropriately imbeds Muir's life within the historical context of the closing of the American frontier, expansion into the West, and the role of nature in the American mind. He explains supreme moments such as Muir's discovery of a rare orchid in Ontario and times with Emerson and Roosevelt. The seminal *Studies in the Sierra* is well reviewed, as is the split among conservationists of the utilitarians, advocating "wise use", and the preservationists like Muir, recognizing an intrinsic value to wilderness. Wilkins demonstrates how Muir's biocentric view and emphasis on the flow of ecological process makes him an important forerunner of Aldo Leopold. The continuing relevance of Muir is clear: his opposition to the damming of Hetchy Ketchy echoes through the Three Gorges, and as our century ends with as much rampant greed as the Gilded Age of a hundred years ago, it is piquant to hear him say of his friend the tycoon Harriman "He has not as much money as I have. I have all I want and he has not." For everyone interested in understanding why John Muir remains a major influence in conservation this book is highly recommended.

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