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book review

The ecology of place

The Ecology of Place, by Ian Billick and Mary V Price (eds.)
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As a long-term student of the ecosystems of South Sinai, I have come to appreciate myself the immense value of being in for the long haul. The new insights brought by just working there every year constantly amaze me. The first 15 years were pure biology, but more and more I appreciate the subtle contributions that the Bedu have made in renewing, enhancing and maintaining the mountain ecosystem. In fact, I have gradually come to believe that maintaining Bedouin traditional practices is the single most vital action required to conserve its arid and punishing landscape. So it isn't hard to convince me of the value of the 'ecology of place', the aim of this book.

What is the value to ecological science of long-term studies of one particular location? The 75th birthday of the Rocky Mountain Biological Station in Colorado created an opportunity to reflect on the contribution that such studies have made, and their position in ecological science in general. Edited collations of papers are always a risk, but here the book succeeds in its aim remarkably well, making it well worth reading.

Biological scientists adopt one or more of three main modes of working: studying questions, taxa or places. Proponents of each mode deride the others, often in the most vitriolic manner. Question-driven science has had the upper hand for the last 20-30 years, and in particular the purse-strings of grant makers, enabling them to cast whole groups of researchers into the wilderness. It's a game of power, not science. Those who study taxa are mere 'stamp collectors'; those who study places are merely 'monitoring', or doing science that shows 'intellectual weakness', a mere 'inductive boiling of hypotheses, with too little effort to place the work in a more general ecological context' (Peckarsky et al. chapter). In this way the eradication of taxonomy and systematics as an intellectually respectable discipline in the UK has been achieved.

Whether a hypothetico-deductive (H-D) approach actually is more productive, and really does move a discipline forward more quickly, is a question crying out for an answer. Biology needs definitively to junk its 'physics-envy', and embrace the idea—virtually unacknowledged until recently—that it is a fundamentally different kind of science from others because of the importance of contingency, variability and history. Nearly all of its theories are exemplar-based attempts at generalising based on very limited sampling of the biological universe. Is it really better to work in a top-down mode, the H-D testing of theory? Or in a bottom-up mode, searching for broad generalisations from studying units, either taxa or places? It isn't clear to me that either has logical primacy, and I know of no evidence from biology that either is a quicker mode of progression.

Ecologists in particular seem to spend a good deal of time and effort navel-gazing about the alleged slow progress we are making. There is no shortage of suggested solutions: we suffer from

> "weakness of method: there is too much natural history; there is too little natural history; we need more experiments; we should be looking for patterns; we should be studying mechanisms; we should be integrating; we should be using strong inference and rigorous hypothesis-testing; we do too much hypothesis-testing and not enough model-fitting; there is too much theory; there is too little theory, or theory of the wrong sort" (p. 2 of the book).

We forget that we are studying the difficult questions of Nature. Physics, molecular biology, genetics, physiology—all are simple sciences relative to the study of ecological communities. How organisms perform in Nature is so contingent, variability so essential to the result, it is hard to see pattern and mechanism. The role of chance is large. The role of history is very large. Thus ecology is either

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as simple as any other discipline but for our stupidity in not looking at problems in the correct way, or it is a different kind of science altogether from those hitherto considered by nearly all philosophers of science.

This book suggests that the long-term study of a single system, the *ecology of place* represents a third mode of study to the top-down H-D and bottom-up study of units. I am not convinced it is different from the latter mode. The addition of 'long-term' is not the issue, since many biologists spend their lives studying in great depth a single question, or a single taxon, or indeed, a single place. Surely there are two axes here: the type of study (question, taxon, place) and mode (short-term studies of many units to obtain breadth, long -term study of a single unit to obtain depth). In terms of community ecology, macroecology is an example of the former mode, and the *ecology of place* the latter. The book is written by North Americans about the study of American places. It is a pity to have such a narrow focus, and the attempt is made to claim the whole topic as an American invention stemming from Aldo Leopold. Well, this is news to most European ecologists, where long-term studies of particular places have been the norm rather than a novelty for a very long time.

Nevertheless, the contributions are remarkably good. The unitary purpose of the book creates a thought-provoking and stimulating read, with an entirely convincing message of the fertility and importance of the ecology of place to the science. Chapters are divided into five sections: the history of the idea; the insights gained; the development of local models; the generalisations that emerge; and encouragement to build capacity. All contributing are interesting, with some really excellent, heuristic and thoughtful explorations of the various aspects of the topic.

Sharon Kingsland writes beautifully on the history of the *ecology of place*, tied up as it is with the establishment of field stations. The claim is that after Darwin and the voyage of the Beagle, British and European biologists resolutely retired to the laboratory with their field-collected specimens, leaving the establishment of true field-based research to the North Americans. She documents the interesting theory of Margalef, who thought that ideas could be traced to the influence of the places whose study gave rise to them. Thus desert ecologists emphasized the role of the weather; Alpine ecologists, the plant communities; Scandinavian ecologists, population biology; and ecologists working in the American MidWest, the idea of succession.

Ronald Pulliam and Nickolas Waser's very thoughtful article asks whether there are ecological invariants, using the fundamental niche as an example. Like Vincent Dethier's failed attempts to iron out neurophysiological variation in a feeding blowfly to use invariant responses as a baseline, Pulliam and Waser show just how difficult it is to establish anything invariant about species and communities.

A masterful summary of work done over more than 30 years on Darwin's finches on islands in the Galapagos by Peter and Rosemary Grant shows how exceptionally useful deep long-term study of one place can be. The results are not at all parochial, but on the contrary, illuminate and advance important generalities of evolutionary ecology. In particular, the rare events only picked up by such long-term dedication turn out to be vital structuring elements of the whole system.

Another exceptional contribution by Stephen Hubbell documents the series of hypotheses tested in his 29 years of studies on Barro Colorado Island about how tree species coexist. In this case the study was planned as long-term because the longevity of many tropical trees made it likely that mechanisms of coexistence might only be evident over several decades. Interestingly, the tree composition on the 50-ha plot turned out to be far more dynamic than anyone had imagined.

These are but a sample of the excellent set of contributions, drawn together by insightful and valuable prefaces to each section, and a final summary, by the editors. The volume is timely because of the lamentable state of field stations in Europe, casualties of government cut-backs and the bean-counting culture that knows the price of all but the value of nothing. The hope is that books like this will renew confidence in field sta-

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tions, and underline the value of their outputs in long-term field studies. These might restore them to their rightful place in the ecological pantheon, places where students can learn to appreciate the insights they offer, and hence maintain them.

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book review

Macro-ecology of the world's savannas

Ecosystem function in savannas: measurement and modelling at landscape to global scales, by Michael J. Hill and Niall P. Hanan (eds.)
CRC Press, 2010, 623 pp. ISBN: 978-1-4398047-0-4
Price: US$159.95 (Hardback)
http://www.crcpress.com/

A quarter of a century has passed since the 'legacy work' of Tucker and collaborators (e.g. Tucker et al. 1985) characterizing the spatio-temporal dynamics of vegetation using temporal series of NDVI data from the NOAA AVHRR sensor, notably over the Sahel region. In the meantime, thanks to advances in Earth Observation (EO), an impressive array of technologies and methods has emerged, multiplying the number of biological and physical variables that can be measured, mapped and monitored over broader spatial and temporal scales than ever before. Progress comes from the development of new sensors, but also thanks to numerous initiatives aiming at facilitating data accessibility, the most recent of which is the launch of the Google Earth Engine© platform.

Therefore, although a number of books exist on savanna ecology, the time is definitely ripe for a general survey of where we are, after these crucial developments, in our understanding and monitoring capacity of savanna ecosystems. This is what Hill and Hanan, along with an impressive team of renowned contributors, have achieved in this volume, notably by bringing together different communities working respectively on field measurement, remote sensing assessment and modelling of savanna structure, dynamics and biogeochemical fluxes at landscape to global scales.

A central question, which continues to stimulate the scientific community, concerns the determinants of tree–grass coexistence in savannas. Mainstream hypotheses are reviewed (chapters 2 and 13), such as bottleneck models, in which perturbation by fire, herbivory or drought tend to reduce tree density, as opposed to the so-called Walter niche-separation hypothesis, according to which trees are maintained by access to deeper soil resources. Of course, real mechanisms are a great deal more complex, and a number of feedbacks and interactions between plants (facilitation and competition), and with grazers or browsers, are detailed. We could further argue that the question itself of 'tree–grass coexistence' might be stated in over-simplified terms, because not all tree and shrub species, for instance, show the same dynamics and strategies with regard to fires and herbivory (see e.g. Beckage et al. 2009).

Given the number of variables interacting in different soil and climate contexts (well presented in chapter one), and the spatial and temporal scales involved, it is clear that empirical/experimental approaches are difficult to implement. The combination of 'natural experiment' approaches with modelling studies is therefore a good way forward. In the former, one investigates multiple correlations and interactions between biological variables and potential physical drivers, through space and time, using EO data; in the latter, these correlations can be tested in 'controlled' conditions *in silico*.

This brings us back to the technological developments of remote sensors. In a synthetic table (table 27.2), the authors list a range of biophysical variables that can be estimated at different reso-