antisemitism, but also troubles arising from his alleged contacts with communists. For instance, after he had accepted his appointment to the biochemistry study section of the National Institutes of Health he received a letter in July 1952 from the chairman of the board of inquiry on employment loyalty of the Federal Security Agency detailing his contacts. He was asked to complete a notarized "interrogatory" but refused. He was not asked again to serve on any committee responsible to a government agency. For such reasons he does not feel able to describe himself as a patriot of the United States or indeed of any other country.

Fruton describes his time at Columbia and then at the Rockefeller Foundation under the direction of Bergmann. In 1945 he moved to Yale where he spent the rest of his career. He became head of the department of physiological chemistry in 1951, the name of which was soon changed to biochemistry, and retired in 1967, although he stayed on in other capacities. Much of the text is taken up with a blow-by-blow account of the academic politics at Yale. For me this serves to emphasize how much tougher university life is in the United States than in the United Kingdom. Although I, and no doubt others, have had our difficulties as heads of departments, they pale in comparison with those described at Yale. I wonder to what extent the Yale experience is typical — the situation may well have been exacerbated by features of Fruton's character that he describes in the preface and to which I have already alluded.

Other chapters describe the travels of Fruton and his wife in Europe, which often came as a welcome relief from the trials in New Haven. Apart from his descriptions of art treasures to be seen, which demonstrate his catholic taste, and a few intriguing personal comments about his contacts, these accounts are of limited interest. The writing has some amusing touches in that he takes pride in repeatedly stating that "we were taken in a chauffeur-driven car to Heathrow" or some similar phrase. He seems to be unexpectedly interested in zoos and the account of the transfer in 1962 of his Rover 2000 by the SS *Queen Elizabeth* to England and then around Europe is entertaining.

As expected, the text is, so far as I can tell, accurate, which is a tribute considering the age of the author. I noted only two errors: it is Bill, not Bob, Whelan (p.174) and the gas used to whiten flour was agene not agine (p.112); odd that in quite different ways I should have been involved with both of them. □

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The variety of life

John H. Lawton

Biological Diversity. The Coexistence of Species on Changing Landscapes. By Michael A. Huston. *Cambridge University Press: 1994. Pp. 681. £60, $100 (hbk); £24.95, $34.95 (pbk).*

ALTHOUGH biologists do not know how many kinds of animals and plants there are on Earth, we do know that some places and some habitats hold many more species than others. The difference between the number of tree species in a hectare of tropical forest (often more than 200) compared with a species-rich temperate forest (rarely more than 20) is well known. But many other equally striking patterns exist in the diversity of other taxa along tropical–temperate transects, up mountains, from shallow inshore waters to the ocean depths, along gradients of productivity, and so on. Most of these patterns have been well known for some time, but it is probably true to say that not one of them has a universally agreed explanation .

Huston's book is a bold attempt to find a common link, a grand unifying theory, underpinning many of the major patterns in the way in which organisms assemble themselves into communities on the face of the planet. The core idea is the 'dynamic equilibrium model' of species diversity, which has two elements: the rate at which species are excluded from assemblages by interspecific competition and the frequency and magnitude of mortality-causing disturbances. Local diversity is greatest where neither process dominates. A unique feature of the model is that the same absolute change in the frequency or intensity of disturbance (by fire or hurricanes, for example) can have totally different effects on species diversity depending on the rate of competitive displacement. The model is an amalgam of two previous attempts at a grand synthesis, J. H. Connell's 'intermediate disturbance hypothesis' and J. P. Grime's 'intermediate productivity hypothesis'. To take one of many examples from the book (Fig. 13.1, p.415), Huston suggests that tropical rainforests are so rich in tree species because they have relatively low rates of plant growth compared with temperate hardwood forests (a surprising conclusion but, averaged over the growing season, one that seems to be true) and lower frequencies of disturbance. Boreal forests have fewer species still because they have comparable rates of growth and exclusion to tropical forests but much higher rates of disturbance.

A feature and strength of the text is the huge amount of literature, much of it published decades ago, synthesized and summarized within its 15 chapters. Huston takes the view, with which I agree, that if we are to understand major patterns in the diversity of insects, or birds, or zooplankton, it is sensible to start by trying to understand plant species richness on land, and the diversity of similar structurally important organisms in the sea, such as coral reefs. A key feature of *Biological Diversity* is the distinction it makes between 'structural' taxa that give shape and form to assemblages and 'interstitial' taxa that hitch a ride on, in and under the backs of structurally important trees and so on. The main emphasis of the book is therefore on plant communities on land, although a whole chapter is devoted to marine ecosystems. Huston also distinguishes between the roles of interspecific competition and disturbance that operate primarily within functional groups and the principal consequences for diversity of adding entirely new functional groups, although how one is supposed to identify members of different groups is not made clear.

Does it work? Yes and no. No single, unifying theory is likely to account for all major patterns of species diversity; as Robert MacArthur reminded ecologists more than 20 years ago, we will need contingent theories tailored to taxa and habitats. Huston's is a brave attempt to forge a key element of that emerging contingent theory, and he is undoubtedly correct to emphasize competition and disturbance among structurally dominant taxa. The weakness of the book, which Huston freely admits, is the purely qualitative test of theory; there are no hard numbers on the axes of disturbance and rates of exclusion. A second problem is the weak link between the local processes of competition and disturbance, and regional, landscape levels of diversity; somehow the scales seem ill-matched. A third difficulty I had with the book is Huston's propensity for making grand generalizations, with no supporting evidence, for instance: "maximum diversity of the high trophic levels occurs at levels of productivity higher than those at which lower trophic levels reach their maximum diversity" (p. 538).

It is easy to carp, and to do so is unfair. I prefer to see *Biological Diversity* as full of interesting, untested ideas, underpinned by a unifying theory that now requires rigorous quantitative examination. Therein lies its major challenge to ecologists, faced with understanding the variety and distribution of life on Earth, before human beings obliterate most of it. □

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■ Just published is *Linking Species and Ecosystems* edited by Clive G. Jones and John H. Lawton. Chapman and Hall, £39.50.