## VIEWS AND REVIEWS

### Individuality, abortions, distortions, and continua

**When Did I Begin?** Conception of the Human Individual in History, Philosophy, and Science.
**Norman M. Ford**. 1988. Cambridge Univ. Press. xix + 217 pp. ISBN 0-521-34428-X. Hardbound. $32.50.

Politics distorts, and absolute politics distorts absolutely. This book, an honest and careful inquiry by a Catholic moral philosopher, focuses on the question of when in human development a single individual is determined. It isn't at fertilization, because, e.g., identical twins often separate a good deal later. (Newman's old work on identical armadillo quadruplets, which form by sequential splittings of the inner cell mass and retain statistical traces of this sequence, would have made a nice foil.) Individuality is irrevocable, i.e. developmentally determined, when the primitive streak develops at about two weeks. That is when, for someone like the author, a soul enters. One doesn't need this perspective to find the matter of interest, though. Individuality is a tricky question because our criteria for it, which are based on organisms we commonly interact with, sometimes fail to coincide with each other in their application.

The author doesn't wiggle quite enough on the matter of souls. He deals more or less adequately with conjoined twins (unless they have one-and-a-fraction brains), hydatidiform moles, and perhaps anencephaly, but there are classes of cases which are even more awkward. Take a mutant or other teratological individual which is inviable just after forming its primitive streak. It is an individual of our species, but for the author the possession of a soul requires the potential for human rationality (which he ascribes even to the anencephalous). There is obviously no such potential without a nervous system, so by the author's criteria we should have a soulless human individual. Perhaps he would agree. But where, exactly, in the continuum of rationality to and past anencephaly does the threshold for soul invasion occur? Rationality comes in degrees, with some degrees present in other animals. (Surely a great ape is more rational than even the potential of an anencephalic human.) These questions are not addressed and make the stated criteria suspect for their purpose.

The book itself isn't distorted except, from another perspective than the author's, in what he perceives as its context. He thinks the book bears on the question of when the developing embryo should be given protection by society. Perhaps it does have such relevance to a believer in souls, although one could argue that even a soul gains in value as its body develops. Other criteria, such as capacity for independent life or human intellectual function, are relevant for other people. (Moral value depends on a mind capable of seeing that value; the value of an embryo isn't to itself but to others, including God for a Catholic.) And the whole dispute is distorted by the common implicit assumption that value suddenly appears. Except for the hypothetical invasion of a soul, though, the criteria used are gradational rather than sharp. Must we dichotomize everything? Is it too much to ask that the value of an unborn human be regarded as a continuum, more or less logistic-shaped? Such a conceptual shift would make it easier to incorporate the differing contexts in which real problems live. -LMV

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[1]Contribution 92, Lothlorien Laboratory of Evolutionary Biology
Evolutionary Theory 9: 111-166 (July, 1989).

## Untangling adaptation and phylogeny

**Frontiers of Comparative Plant Ecology.**
**Edited by I.H. Rorison, J.P. Grime, R.Hunt, G.A.F. Hendry, and D.H. Lewis.** 1987 (November). Academic Press. Reprint of New Phytologist 106 (Supplement). ii + 317 pp. ISBN 0-12-595960-5. Softbound. $31.50.

Comparative plant ecology usually refers to a search for patterns among ecologically different species. That is what most of this book is about. The subjects of the patterns are diverse, from seeds to costs of flowers, from metal tolerance to herbivory, from root topology (applying and extending results from streams in geomorphology) to genome size. Other things too, but of course not everything. The papers are mostly conceptual and empirical reviews with original work inserted; they are uniformly good and the book is thus an appreciable contribution.

Phenomena which are evolutionarily plastic make notoriously poor characters in taxonomy, but for comparative ecology they are the sort one wants. Just how to sort out phyletic and adaptive effects is less than obvious, especially because these classes overlap broadly, and there is now a small and internally contradictory literature on the subject. The problem itself, though, seems to be misstated. There are two quite distinct aspects involved, and these can be dealt with sequentially. First, determine as well as possible which cases of the similarity are homologous and which are homoplastic. This may of course involve similarity of more than one character state, or continua. Then consider the matter of adaptation for all cases. Just how to do all this will vary among phenomena and taxa; one can justifiably be skeptical of universal cookbook methods which assume no knowledge of the organisms themselves. -LMV

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## A Tail of Two Hierarchies

**Macroevolutionary Dynamics**: Species, Niches, and Adaptive Peaks.
**Niles Eldredge.** 1989 (24 April). McGraw-Hill. xii + 227 pp. ISBN 0-07-019472-2 hardbound, $28.95; ISBN 0-07-019476-9 softbound, $14.95.

One can take a good idea too far. That there are two biological hierarchies, the genealogical and the ecological (or economic, as Eldredge has it), interacting in definable ways but without much actual overlap, was a real conceptual advance. What Eldredge does in this book, though, is to make the hierarchies rigid, prohibiting as well as describing, and constituting a dogma on which other views are to be judged. He is not successful in this.

For the rest, what there is of it, it's a pretty good book, at least for presenting a coherent viewpoint. Eldredge remains strictly punctuational, and he adopts Futuyma's explanation for stasis: populations wink in and out, and the dispersal which keeps the metapopulation extant averages out local adaptations, so there is negligible net evolution until the species splits. I find this implausible. Even if there are enough populations that such metapopulational drift approximates stasis, sib species prove that stasis can extend beyond species boundaries (and thus beyond the possibility of homogenization) even in very sedentary species. Moreover, any general adaptations would accumulate even with homogenization, and (as with the currently fashionable stabilizing-selection explanation) it is implicitly assumed that the average effective environment is constant through the duration of a species. There aren't any other really plausible general explanations for stasis, though, and this one does implicitly emphasize our ignorance, to orders of magnitude, of the frequency of local extinction in widespread species.

The book does have some reasonable and partly original discussions of some kinds of macroevolutionary patterns. Dogma keeps intruding, though. Eldredge thinks that geographic variation within species is overestimated (maybe so, and it is awkward for stasis, but there is no review). He denies, though, that whole species (as distinct from their local avatars) have niches, because other aspects of their ecosystems differ geographically! Such playing with words and the consequences of arbitrary definitions, rather than biological causes and patterns, pervades the book. Thus only holophyletic taxa are supposed to have internally similar ways of life, but even they don't occupy adaptive zones because they aren't "economic" units and have no process of origination. A broader view would see why such assertions can fail.

Eldredge realizes the importance of the evolution of communities as well as of related organisms, and such recognition is not common. His rigid categories, though, restrict such evolution to local ecosystems; adaptive landscapes are supposed to have no broader scope. Geneticists have monopolized adaptive peaks in recent years, and Eldredge uncritically accepts their framework, where the landscape is supposed to be fixed and the problem is to shift among peaks. In the real world, though, the environment is in flux and the locations and heights of peaks change in complex ways. I think that the real problems come mostly here and in the internal developmental coadaptation and external adaptive ratchets which we jointly call constraint. Perhaps drift is relevant here too, but if so not as directly as in a fixed landscape.

Eldredge is also a cladist, and (unusually for this religion) he takes some notice of other views. However, he thinks that synthetic systematists welcome polyphyletic taxa, taking an aberration by Simpson as representative. The cladistic blinders make Simpson's clear discussion elsewhere of horse evolution "difficult to interpret". The evolution of taxa is supposed to be just an epiphenomenon of the evolution of traits. And Eldredge actually accepts Stanley's argument that the low rates observed in most phyletic evolution show that high rates must be by splitting, neglecting that high rates are equally hard to observe in each case.

"Species arise and are maintained through the reproductive activities of organisms" [sensu individuals] (p. 95). He thinks they have no other properties, and argues that reproductive change is necessary but insufficient for appreciable adaptive change. That lineage ("species") selection is non-Darwinian is a common claim these days, but the claim is still false. Darwin explicitly discussed it on pp. 109-110 of the Origin (ed. 1). It is also pre-Darwinian (Lyell in 1832) and neo-Darwinian (e.g. Simpson, Wright).

A curious result of the rigid hierarchies is the conclusion that the evolution of the soma and that of reproduction are sharply distinct. A corollary made from this is that the notion of fitness is misleading because it conjoins the two. Fitness has enough problems not to be stuck with this one, though. That both soma and reproduction are aspects of the same thing is sufficiently shown by the trade-offs which very commonly occur between as well as within them.

The unintended message of this book is that it is dangerous to let metaphors determine our thoughts. Exceptions do probe the rule, and they can destroy it when its structure doesn't correspond to that of the phenomena it attempts to explain. Ideologies don't recognize this because faith in them is stronger than trust of evidence. Ask yourself from time to time: What could in principle cause me to abandon this general viewpoint? If there isn't a real answer, it is perhaps useful to inquire why there isn't.

-LMV

## Biotal Evolution and Reefs

**The Evolution of Reef Communities.**
**J.A. Fagerstrom.** 1987 (30 October). Wiley. xv + 600 pp. ISBN 0-471-81528-4. Hardbound. $77.95.

Communities and other biotas evolve too, although this isn't apparent to many species- or individual-centered biologists. Because many biotas aren't communities I propose the term **biotal evolution** as a general way to refer to the evolution of biotas. The distinction between biotal and organismal evolution is partly like the distinction botanists have between vegetation and flora. I don't want to explain, or defend the existence of, such evolution here, but reefs provide an unusually well bounded domain and Fagerstrom's book won't easily be equalled.

A reef is a more or less permanent mound of autochthonous skeletons and their debris, large enough to modify the local environment. That's nearly what Fagerstrom says more obliquely, and his treatment of this abused and fuzzy concept is exemplary. Not all reefs are dominated by corals even today, and most in the past haven't been.

Reefs are of unusual interest for several reasons. They provide oases of high productivity in vast oceanic deserts (oligotrophic waters, for the purist.) They are the largest organically constructed geomorphic features. They have unusually high diversity. Space is in most cases the resource in shortest supply, with intense competition for it and quite complex partitioning. The energetically dominant animals are mostly colonoids. In near-surface reefs, the predominant ones (others occur to northern Norway, and over a kilometer deep), sessile animals are commonly photosynthetic, being multispecies with diverse algal as well as animal origins, the latter being soft and hard corals, anemones, jellyfish, sponges, large foraminiferans, and the giant clam. (The algal component is sometimes predominant in mass, and under stress the components may separate, to their mutual disadvantage. On the basis of estimates of growth rates Fagerstrom concludes that pre-Cenozoic corals weren't symbiotic. However, assuming that the ridges used for estimating rate are correctly interpreted, it is still entirely plausible that the algal promotion of coral calcification evolved after coral-algal multispecies were established for their mutual trophic advantages. And even the growth-rate data are partly inconsistent.)

Reefs have been around since the early Proterozoic, but not continuously nor with even similar taxonomic composition. Fagerstrom reviews this evolution in detail, with conclusions a bit different from previous reviewers and from Talent (1988, Senckenbergiana Lethaea 69: 315-368.) Mass extinctions and other aspects of evolution sometimes affect reefs in a rather different way from level-bottom communities. Conclusions on overall increase in diversity within guilds, and decrease of overlap among them, may well be correct but need better documentation as to continuity vs. re-evolution in successive avatars of reefs in the Phanerozoic.

Reef communities have guilds, as other communities do, and Fagerstrom makes an extensive and original analysis of their nature and evolution. He appropriately emphasizes spatial aspects, but in doing so he places primary emphasis on contribution to the function of the whole reef rather than to the adaptations of the organisms themselves. This is a serious problem only for his distinction between constructor and baffler guilds, but it does obscure other distinctions important to the organisms themselves. The book gives little attention to the flow of energy and nutrients, or to the many organisms of a reef which don't directly affect its structure. It does note, though, that most reefs are net exporters of nitrogen and that they survive by being able to recycle within the reef community the great preponderance of materials used. The index is poor, and so are several aspects of the treatment of the photographs, but the book itself is first-rate. -LMV

## Macroevolution under our noses?

**Evolution, Ecology and Epidemiology of Antibiotic Resistance.** Reprinted from Journal of Antimicrobial Chemotherapy, vol. 18, Supplement C.
Edited by **Bernd Wiedemann, Peter M. Bennett, Alan H. Linton, Olla Sköld,** and **David C.E. Speller.** 1986. Academic Press. vi + 264 pp. ISBN 0-12-451170-8. Softbound. $30.00.

All the examples of very rapid evolution I can think of are of resistance to human effluvia of one sort or another: antibiotics, insecticides, pollutants, and such. They have nevertheless attracted a surprisingly little amount of interest by evolutionary biologists. I had never even heard of the journal where the papers in this volume first appeared (in 1986), nor of any of the authors, but most of them have interesting things to say to us. A substantial part of the Earth's environment is now partly or entirely transformed by human activities, with entirely new habitats created and others severely modified. The global disaster so created is mitigated slightly for us, if not for most people, by adaptation of various organisms to the new conditions. We are part of the natural world, and evolution occurs around us and even within us. Rapid evolution normally follows or accompanies a mass extinction, and it is occurring with the current mass extinction if in a rather different way from past crises. So why not study it?

There are 32 short papers, They discuss mechanisms of antibiotic resistance and its transfer, and case histories. Even the more clinically oriented papers have evolutionary implications. There is a different world here, and it could benefit from evolutionary input just as we can from what has been studied by others.

-LMV

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## Phylogeny and behavior

**A Primate Radiation.** Evolutionary Biology of the African Guenons.
Edited by **Annie Gautier-Hion, François Bourlière, Jean-Pierre Gautier,** and **Jonathan Kingdon.** 1988 (September). Cambridge Univ. Press. viii + 567 pp. + 8 color plates. ISBN 0-521-33523-X. Hardbound. $120.00.

One is apparently supposed to know what a guenon is; I didn't. Maybe <u>Cercopithecus</u> is more familiar; the "common" name is coextensive with the genus, although the genus is sometimes split into as many as four. Green monkeys and their relatives, if you prefer. The group is undergoing appreciable evolution at the species level, and several papers discuss aspects of this evolution without, mostly, making the connection explicit. Visual recognition is important and the plates show the diversely colored faces of most species. Patterns differ even racially. The existence of such divergences raises the question of the detailed mechanism of their origin, a matter not as trivial for recognition marks as for most characters and one not dealt with in the book. Lernould discusses the taxonomy of extant taxa in a discursive way, without characters; one species or subspecies still apparently lacks a name. Several authors estimate the phylogeny of the genus, by methods ranging from vocalizations to isozymes to paleontology (the latter not intrageneric). There is broad-scale agreement, although the editors' conclusion that arboreality is secondary in the genus is inadequately supported.

The best evidence comes from chromosomal rearrangements, where evolutionary polarity is clear. They are unusually numerous in the genus. Dutrillaux, Muleris, and Couturier show from them that the genus has two branches. A remarkable feature of their graphically displayed phylogeny is that in many cases different rearrangements have intersecting distributions. This is impossible in a simple tree phylogeny without massive parallelism, and the authors reasonably conclude that hybridization or even polymorphism (as Throckmorton proposed in 1962 in a forgotten paper) is to blame. Only the former is now observed, and it is common enough to make the intersections even expected. Note that intersections also occur in the ape-human tritomy, giving evidence for hybridization (intraspecific or interspecific) which may reconcile the several sorts of evidence which tend individually to point more or less strongly to mutually incompatible tree topologies here too.

The second half of the book is devoted to reviews of behavioral studies, by topic or by species. These are good discussions but almost uniformly ignore phylogeny. Rowell does document several differences in social systems between Cercopithecus and other Cercopithecinae. She finds that the two species which are chromosomally most primitive are also rather intermediate socially, and gives reasons why this is likely to be from phylogenetic position rather than from ecology directly. Such comparisons are important in testing evolutionary ideas and in formulating new ones. They obviously depend on both a reasonably good phylogeny and a broad sampling of species for what is being compared. The work reported here will, when extended, be a good basis for similar comparisons for other traits. -LMV

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## Sociobiology comes of age

**Evolution and Human Kinship.**
**Austin L. Hughes.** 1988 (11 February). Oxford Univ. Press. xi + 162 pp. Acid-free paper. ISBN 0-19-505234-X. Hardbound. $29.95.

Sociobiology has seemed to me, looking at it from the outside, to be not evil, not false at the core, but rather trivial, not bringing real new insights but lending itself to facile pseudoscientific expoundings of one's own preconceptions. I was wrong. Hughes has used the central sociobiological motif of inclusive fitness to turn cultural anthropology on its head. He treats Homo sapiens as another primate with social behavior which can be treated as a set of partial adaptations. "Complex patterns of social behavior can be seen as the result of each individual's effort to maximize his or her inclusive fitness, subject to the constraint that compromise with others will be necessary" (p. 19).

Hughes asks why cultural elements and patterns are as they are, itself a somewhat unusual question in recent anthropology. This doesn't assume that there is a single optimum, but vast regions of the potential cultural x environmental hyperspace prove to be adaptive valleys. In the examples reported, all being cases where the nonsocial environment can apparently be ignored, the fit to the theory is surprisingly good and often involves drastic reinterpretation of previous work and of the functionalist (naive group selection) overview. Hughes develops a fairly detailed mathematical theory which, as he notes, potentially has wider application.

I don't know of any more or less viable competing theory except (1) the null theory, which seems to be widespread as an implicit perspective, of change which is random with respect to known variables, and (2) enlightened individual selfishness. Both have major empirical difficulties; kin selection is automatic and is not a hypothetical force to be brought in only where necessary. Critics of sociobiology will have to do better than these competing theories, or show that the difficulties are only apparent, if they are to have a coherent position. They might alternatively show that inclusive fitness fails empirically as an explanation for cultural patterns, leaving us with nothing. I await the response with interest; it would be nice to have more than one real theory available.

A remarkably impressive book. -LMV

Where the mollusks really came from (and other stories)

Oxford Surveys in Evolutionary Biology. Volume 5.
Edited by Paul H. Harvey and Linda Partridge. 1989 (January; stated 1988 in book). Oxford Univ. Press. (iv) + 259 pp. ISBN 0-19-854236-4. Hardbound. $65.00.

This volume contains two important papers, but I will note the others first. Bulmer evaluates tradeoffs among accuracy, speed, and cost in the several component processes of protein synthesis. Ohta considers gene families as partly integrated units of evolution. Winkler and Wilkinson make a theoretical and empirical analysis of how much parental effort birds and mammals should and do make. Ewald treats the evolution of virulence in parasites of humans and others as affected by what humans do -- an interesting analysis which has obvious extensions. Bull and Charnov review the evolution of sex ratios from a more basic viewpoint than is common. (Their use of "gender" for biological sex is deplorable in retaining, under a different word from before, the confounding of biological sex with cultural gender. That pervasive confusion is why the word was imported from linguistics only a few years ago.)

Pomiankowski sets the little world of sexual selection on its head by showing, in a way persuasive to me, that a quantitative-genetic approach to Zahavi's handicap principle rescues it and thereby solves, in principle, the awkward problem of the evolution of female preference. The paper extends one he published a bit earlier. Females should prefer males with some common and general classes of handicaps, because they have shown that they can nevertheless survive. That's what females should do. How much they actually do so is a matter still to be shown convincingly.

Finally, one of the more important papers of recent years. In a feisty, yeasty, and scholarly piece of work, Ghiselin establishes the origin of mollusks from proto-annelids. I do mean establishes -- the overall argument is compelling and the conclusion goes beyond what has been proposed before. Part of the evidence is from his collaborative work on 18s rRNA sequences. The published work of this group is provocative but not compelling because their phylogeny relies on a computer analysis which, from a conversation with Raff, doesn't give the intermediate stages which are necessary to see how it gets where it does. One can re-analyze the whole set of data, but that shouldn't be necessary. Ghiselin gives here a small subset of the data, purportedly of the most informative nucleotide positions and treated with pairing taken into account, but I do wonder if I would choose the same subset if I took the time to try. Ghiselin fortunately agrees with much evidence, rather than with the original paper and the computter, that coelenterates are metazoans. He analyzes the positions for which he gives the data as characters, in an adequately sophisticated way which is not yet at all common. This is one source of support for his derivation of mollusks. As a byproduct, there is similarly strong evidence for sipunculans and brachiopods, at least, being even closer phyletically to modern annelids, and arthropods diverging a bit earlier. As he notes, these (and other: e.g., Snodgrass defeats Manton in that crustaceans and insects seem to have diverged later than chelicerates) conclusions bear on diverse evolutionary topics.

The other sort of evidence is morphological. Ghiselin gives brief but adequate reasons why this can be interpreted in a reasonable way only as having the Mollusca coming from a highly metameric, annelid-like ancestor. I hope that he will treat this evidence in more detail elsewhere, as it deserves. There are even fossils which, as I interpret the Machaeridia in a paper on phyla elsewhere in this issue, have this view as their simplest phyletic interpretation. The adaptive change involved is also not difficult, as Ghiselin notes but which also could stand elaboration. His uninhibited review of earlier work shows the value of being able to read more than English. It was a remarkable experience to go through an entire paper by Ghiselin and find myself agreeing with everything in it. He's always provocative, but too often I find him barking at a secure tree or a phantom. Not here.

Can't Oxford bring this series out in paperback? It would reach more of the people it needs to. -LMV

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## Real Extinctions

**Extinction and Survival in the Fossil Record.** Systematics Association Special Volume 34.
Edited by **G.P. Larwood**. 1988 (21 July). Oxford Univ. Press. x + 365 pp. ISBN 0-19-857708-7. Hardbound. $90.00.

25 years ago extinction was just something that happened eventually to most lineages, and paleontologists often didn't believe that mass extinctions had occurred. And that extinction could contribute to the nature of evolution, or to ecology -- absurd! So I gradually started studying the subject, from various perspectives, and others came in too. It's now somewhat of a bandwagon, except in ecology, and I tend to avoid bandwagons. One advantage of bandwagons, though, is that they generate lots of background information, some of which can be used in ways the generators didn't foresee if the user is careful.

What we have in this book is a consideration of mass extinction in relation to most major fossil groups, and a couple of less major ones, individually. The authors were supposed to consider their groups especially in relation to the five mass extinctions which Raup and Sepkoski identified in 1982. The latter analysis, however, doesn't give mass extinctions. It is based on absolute numbers of taxa, so when there are fewer taxa around to be at risk mass extinctions are likely to be overlooked (as happened), and the statistical basis for the whole analysis is thereby biased. I corrected this a couple of years later, but the authors (and most others) continue to rely on the original picture, pretty but wrong.

The authors know their organisms, and it shows. (Most groups are too broad for such knowledge throughout their scope, so one must remain critical even here. Yes, I did find some small errors for mammals in two chapters.) Several provide original tables of ranges of taxa, and almost all use their knowledge to interpret in an adaptive framework what patterns, or lack thereof, they find. Nobody finds periodicity, but its adequate detection (if it exists, as it does seem to from rawer data) may require larger numbers of taxa than most or all of those discussed in a single chapter here. Paul confirms that echinoderms suffered greatly in the Permian extinction but not detectably at the end of the Cretaceous. In a chapter on birds, Unwin makes a reasonable adaptive case for birds outcompeting pterosaurs, but his evidence for this is simply irrelevant because it compares contemporaneous proportions of the two groups with respect to each other only. One would get a similar picture if pterosaurs declined because of, say, racial senescence, and bird diversity remained constant. I do suspect he is largely right, but that's by inserting evidence he leaves out.

Several chapters give evidence of unusual selective effects from mass extinctions. This isn't a new phenomenon, although it has been touted as such; Newell showed it long ago for reefs and it was known before that (first by Romer?) for late Pleistocene mammals. Briggs, Fortey, and Clarkson claim, without specific analysis, that the extinctions which delimit the Cambrian biomeres are to a considerable extent artifacts, at least above the species level, caused by taxonomic feedback from recognition of the biomeres themselves. This question deserves detailed exploration in a combined phyletic, adaptive, and stratigraphic context; biomeres are too important to dangle in limbo.

There is one crusty curmudgeon (Ager) who still questions the very existence of the Cretaceous/Paleogene mass extinction. I hope that recent work on brachiopods (his group) has converted him. He also thinks that supraspecific taxa are useless because arbitrary, by which he apparently means that both categories and taxa have fuzzy boundaries. If so, it exemplifies what may be called the Adolescence Fallacy, that reality of subdivisions requires discrete boundaries. This fallacy also plagues cladists, notably Benton in this book, when they claim that paraphyletic taxa are artificial. An additional difficulty here is that we still lack an adequate measure for adaptation, change in which delimits (within a phylogeny) the boundaries of paraphyletic as well as holophyletic taxa, but none of this prevents useful approximations.

We do, though, need to watch out for what I have called pseudoextinctions (Simpson's taxonomic extinctions). These are less common the higher the category, although we lack a quantified study, and it is easy to exaggerate their importance. (It is also easy to ignore them, as is done in most chapters.) Because an adaptively unified group may give rise to a successor group well before its own last member, paraphyly isn't the same as pseudoextinction. (Only a cladist would claim that dinosaurs are still with us because birds are.) An extinction is real unless the last species of a group gives rise to a derived group. This is often hard to tell, but some internal phyletic analysis of the pregroup is usually necessary. For instance, Smith and Patterson's claim of a high frequency of pseudoextinctions among families of echinoderms and fishes is based merely on the earliest record of the exgroup being later than the latest record of the pregroup. How much within the radiation of the pregroup the divergence actually occurred isn't discussed.

Anyway, a valuable book. -LMV

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## Stress on the small

**Microbes in Extreme Environments.**
Edited by R.A. Herbert and G.A. Codd. 1986. Academic Press. (viii) + 329 pp.
ISBN 0-12-341460-1 hardbound, $75.00; -341461-X softbound, $42.00.

The world of the small isn't like ours, in too many ways to recount. That mostly doesn't matter for this book, which deals with macroscopically obvious environmental extremes such as high pH or pressure. Except for a chapter on fungal responses to "heavy" metals, prokaryotes take the stage. By approach, physiology predominates, with a little ecology and no consideration of phylogenetic aspects. The chapters are reviews and give good treatments of what they discuss, but even within the book's real scope there is a lot not covered (e.g. acidity, dryness, or redox stress). Sulfide is apparently most toxic when as undissociated $H_2S$, and the biochemistry of the toxicity is unclear. Many bacteria adapted to low-nutrient environments "fragment" to produce tiny ultramicrobacteria under these conditions. These have very low metabolism (an interesting reversal of the usual size dependence, not commented upon) and, if I may extrapolate from the account given, seem to be at least mostly a means of enhancing effective body size in order for the clonalized individual to profit from spatiotemporally local pulses of nutrients.

Microbial populations and communities have great potential for stimulating advances in much of the evolutionary half of biology. I hope that the reductionistic training necessary for technical competence doesn't continue to inhibit imaginative study at higher levels. There's a lot there, it's different, and it may often prove more generally illuminating. -LMV

## Where do constraints mostly live?

**Genetic Constraints on Adaptive Evolution.**
Edited by **Volker Loeschcke**. 1987. Springer-Verlag. ix + 188 pp.
ISBN 0-387-17965-8. Hardbound. $49.50.

Are there genetic constraints, distinct from developmental ones? Well, yes. After all, sustained evolution does require genetic variation, and there are phenomena like sex ratios and kin selection which have an irreducible genetic center. Geneticists have an unfortunate tendency, though, to try to incorporate into genetics anything that is related to development. The interface between development and genetics is fuzzy and important, and the diverse genetic approaches to this interface are worth attention by most sorts of biologists. Disciplinary imperialism nevertheless can, and does in this case, hide the very existence of major phenomena and problems which the imperialists can't incorporate without distortion. Distortion comes, e.g., in treating the adult phenotype as the result of an interaction between genes and environment, or in regarding the latter interaction itself as synonymous with genetic correlation or pleiotropy. In this volume only Scharloo shows sensitivity to this problem.

The book is really, for the most part, an interesting review (with new analyses and even experiments) of much of evolutionary quantitative genetics. One paper rediscovers the original focus of quantitative genetics (single phenotypic characters) and treats it as a new approach, which really isn't as inane as it sounds because of intervening advances. Another paper, the only one outside quantitative genetics, considers some kinds of nonrandom mutation in relation to molecular evolution. Most of the papers focus in one way or another on ecophenotypic variation (a term from a different literature which means the same as the highly misleading term "reaction norm"), a central topic often ignored, and they make some real progress. I hope a next step in one direction will be to consider developmental noise explicitly; it sometimes interacts nonadditively with both genotype and environment (singly and together) and has nontrivial consequences.

In the preface the editor states: "This volume is addressed to all evolutionary biologists and aims at making them suspicious towards a number of today's popular evolutionary concepts: the adap[ta]tionist program, the 'advantage to the species' argument and optimization theory. Although useful in some contexts these ideas are based only on phenotypic characteristics and thus ignore genetic constraints on adaptive evolution." Whatever may be said of these concepts from other perspectives, the work reported in this book does nothing to undermine my provisional conclusion that, outside of mechanisms in microevolution, genetics has only marginal or occasional significance (say part of the evolution of sex) in evolutionary phenomena. The phenotype remains central, with an ecological motor and driver (not a passive theater), and the phenotype is development. -LMV

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## Major phylogenies, adaptive phacies, and pharts

**Archaebacteria 85.** Pp. 1-397 reprinted from Systematic and Applied Microbiology, vol. 7.
Edited by **Otto Kandler** and **Wolfram Zillig**. 1987 (April; stated 1986 in book). G. Fischer Verlag (Stuttgart and New York version). viii + 434 pp.
ISBN 3-437-11057-8. Hardbound. $103.00.

Archaebacteria occur commonly in environments which are lethal, in one way or another, to most other bacteria. (Miller and Wolin, though, discuss the occurrence of a methanogen which lives in our colon and which in some people produces four liters of methane a day. Farts [prudes note: a good Chaucerian word] also contain swallowed air.) So maybe they are outcompeted elsewhere and have so far managed to survive the sporadic incursions of eubacteria into their refuges. But it seems more likely that adaptation to heat and acidity, at least, was primitive for the group and quite possibly involved in its origin. Searcy makes a reasonable adaptive case for this and for a similar stage in eukaryote ancestry. Diverse evidence, mostly not discussed in this volume, now strongly suggests that the eukaryote host cell arose from a common ancestor of archaebacteria. Searcy shows how thermoacidic adaptations may have produced some eukaryotic traits. It is rather striking that, on the whole, the eubacteria, archaebacteria, and eukaryotes correspond respectively to the major adaptive facies of colonizers, stress-resisters, and competitors distinguished by several ecologists.

The book has lots of mostly little papers on DNA, ribosomes, phylogeny, lipids, enzymes, metabolic paths, and other topics. 33 pages of previously unpublished abstracts are included too. Brock and others emphasize the importance and relative ease of work on real ecology of archaebacteria, but this remains a hope. Large, eukaryotic-size ribosomes are apparently primitive in the Archaebacteria, converging on eubacterial-size ones in a large clade. It often is obscure to what degree the various resemblances of specific archaebacteria to eukaryotes are convergent, primitive, or indicative of a common clade. In the two latter cases the resemblance would have been present in the initiator of the Archaebacteria and then lost in some members of the group. The known data are hard for a nonspecialist to extract and few specialists are sophisticated enough about phylogeny to avoid traps. So collaboration seems useful, except that the existence of a problem here is hardly recognized yet.

For instance, several phyletically distant archaebacteria are aerobic, with good cytochromes and whatnot. If this condition was ancestral for Archaebacteria it implies an unexpectedly late divergence of the Archaebacteria from within the Eubacteria, and also a primitively aerobic eukaryotic host cell. If the instances of it are convergent, the close similarities to derived eubacterial proteins imply several cases of lateral gene transfer, in rather large quantity. I don't even know just what occurs where; the subject seems never to have been reviewed. -LMV

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## Population Ecology: Moribund or Just Depressed?

**Mammal Population Studies**. Symposia of the Zoological Society of London, 58. Edited by **Stephen Harris**. 1988 (11 February; stated 1987 in book). Oxford Univ. Press. xvii + 350 pp. ISBN 0-19-854006-X. Hardbound. $98.00.

Almost all the work reported in this book could have been done around 1940. We have reviews of, and minor advances in, methods; we have studies of mortality and reproduction and numbers, some of which actually shed some light on the regulation of the populations considered; and, nearly, that's it. I don't mean to disparage the careful work which is reported here (really, I don't: it is needed, and problems aren't trivial just because they are old). But does everything have to be set in the framework which Elton created? Mollison does give a critical overview of aspects of the epidemiology of parasites; this, in its realistic form, is modern. Tapper's suggestion that cyclic and noncyclically fluctuating populations may have basically similar dynamics also depends on work after 1970.

Berry, in the introduction, argues for a consideration of genetics. One may extend this to variation in general, within and among populations (how about mammalogists looking at their populations somewhat as Taylor has for insects?). Another direction, dear to my own heart, is energetic (how about seriously looking at energy tables, where energy flow is incorporated into life tables?). How about comparing the population dynamics of various coexisting species, actually an Eltonian legacy from Wytham Wood? How about physiological aspects, such as the slow rate of senescence likely for bats as inferred from their commonly low mortality? Anyone with some imagination can continue this list; there are even real and interesting population-dynamic aspects to interactions of cells in a mammal's body. It wouldn't be funded, though. -LMV

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Der eozäne Messelsee -- **Eocene Lake Messel.**
Edited by **Jens Lorenz Franzen and Walter Michaelis.** 1988 (31 December). Forschungsinstitut Senckenberg, Senckenberganlage 25, D-6000 Frankfurt/M., W. Germany (their Courier 107). 0 + 452 pp. ISBN 3-924500-44-4. Softbound. DM 78.00 (about $45).

The magnificent Eocene deposits of Messel continue to yield discoveries. This volume is the result of a symposium in 1987 and, while not reviewing the biota, gives insights into its nature and preservation. The rocks are an unusual oil shale, and several papers discuss aspects of its geochemistry. The fine retention of intermediate compounds permits some new general results as well as furthering knowledge of the deposit itself. Many bacteria are preserved in a phosphatized manner which seems not to be understood microbiologically. (The authors don't mention possible similarities to the phosphatized late Cambrian arthropods of Västergötland, Sweden.) Gut contents and coprolites are discussed, and an interesting analysis of what sorts of animals haven't been found, or have been found in unexpectedly low abundance, provides information on the nature of the lake itself.

Fruits and several classes of vertebrates, especially mammals, receive specific treatments. The omission of printing on 8 partly nonconsecutive pages in the review copy created some problems here, but one paper gives a large-scale revision of the Gruiformes while discussing its Messel representative. Another bird may be closest to rheas -- there is a surprising amount of specifically South American affinity to the tetrapods, quite a puzzle if it's mostly or entirely real. A final set of papers deals with fossils from elsewhere which have some bearing on the Messel biota. These range from a review of the soft tissues preserved in some small specimens from the Geiseltal, to Paleogene Lauraceae, to astute reviews of some mammals (and a whole new fauna from France), to partly original reconstructions of skeletons of some early and middle Eocene mammals. (I note here that one family, the Paroxyclaenidae, which I originally put together myself, almost certainly doesn't belong to the Condylarthra, as stated.)

Messel is one of the most significant paleontological sites anywhere. The volume concludes with a resolution opposing the decision to convert the site into a garbage dump. -LMV

**Climate**: History, Periodicity, and Predictability.
**Edited by Michael R. Rampino, John F. Sanders, Walter S. Newman, and L.K. Königsson**. 1987 (November). Van Nostrand Reinhold. xvii + 588 pp. ISBN 0-442-27866-7. Hardbound. $67.95.

There should be climatic cycles of some sort or another. There are a bewildering number of astronomical periods which give a nudge one way or another. There are problems, though. The mechanisms, on their face (faces?) don't seem strong enough to do much, and inferring periodicity from noisy evidence like fossils has severe statistical difficulties. The relevant tests are subject to unusually subtle biases, and what is tested is often a mere departure from uniformity rather than periodicity or a particular period. As I remarked more than 20 years ago the agreement between "Milankovitch" cycles and the dating of Pleistocene climatic fluctuations from fossil marine plankton gave support to each by the consilience. So there must be amplifiers of the signals, and that is another problem because of its greater distance both from data and from known mechanisms.

This book is a Festschrift for Rhodes Fairbridge, who contributes a couple of chapters himself. I went to graduate school partly in the same building he was but unfortunately have never met him. The book focuses mostly on phenomenological evidence for climatic periodicity and on astronomical cycles which may nudge (I can't say "force" because of the amplification needed) them. Quite different time scales are discussed. It's difficult to tell how many, and how much, of the various putative cyclicities are real, and causal intermediaries between the astronomy and climate remain elusive. That isn't to say they aren't there, but I wish they were more a focus of real study than is indicated by the book. It's good for predictions, though. Maybe the little ice age one paper predicts to start in the 1990s (denied by another) will partly offset greenhouse warming for a while. Any takers? -LMV

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**The Story of Life on Earth**: Tracing its Origins and Development Through Time.
**Michael Benton**. 1986. New York: Warwick Press. 0 + 93 pp. ISBN 0-531-19019-6. Hardbound. $13.90.

As a book for youngsters (and other beginners) this is one of the better science books despite a rather forced-cheerful and didactic tone. It passes through (one can hardly say that it covers) the requisite topics, including relevant geology, and adds a few pages on things like extinction and evolutionary rates. There are no suggestions for further reading. The figures are attractive and complement the text well. Both they and the text are mostly accurate; I noticed a few text inaccuracies for mammals, and some of the reconstructions of Paleozoic communities are internally inconsistent in scale. Two copyrighted figures from a paper of which I was a co-author are included without acknowledgment; I wonder how many other apparently original figures originated this way. -LMV

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**Silurian and Devonian of India, Nepal and Bhutan: Biostratigraphic and Palaeobiogeographic Anomalies.**
**John A. Talent, Rajendra K. Goel, Arvind K. Jain**, and **John W. Pickett**. 1988 (1 November). Forschungsinstitut Senckenberg, Senckenberganlage 25, D-6000 Frankfurt/M. 1, W. Germany (its Courier 106). (ii) + 57 pp. ISBN 3-924500-43-6. Softbound. DM 14.00 (about $8).

This is a partial but detailed documentation of the now infamous apparent paleofraud, as discussed recently in Nature. It looks pretty damning. -LMV

**Before the Indians.**
**Björn Kurtén.** 1988 (30 June). Columbia Univ. Press. (v) + 158 pp. plus 28 plates. Acid-free paper. ISBN 0-231-06582-5. Hardbound. $29.95.

I suppose this is Kurtén's last book. It deals with Pleistocene North America up to the megafaunal extinction about 8000 B.C. The treatment is by communities and fossil sites rather than by taxa, and this permits a natural integration of adaptation and ecology. The book is an offshoot of his technical one with Anderson on the same subject but is a few years out of date in minor ways. (There is no indication of when it was written.) As one may expect from a novelist, the writing is clear, flows well, and extracts points of general interest. The plates, mercifully almost uncluttered, are unusually good community reconstuctions from single sites. They were prepared in color and the legends assume that this would be preserved in publication, but it wasn't. One reproduced on the dust jacket, to be discarded by libraries, shows the beauty which was lost. I hope that some vehicle can be found for their proper publication. -LMV

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Neue Daten zur Biostratigraphie, Paläogeographie und Paläontologie des Devons in Eurasien und Australien. **(New Results in the Devonian Biostratigraphy, Paleogeography, and Paleontology of Eurasia and Australia.)**
**Rolf Werner & Willi Ziegler** (ed.) 1987. Forschungsinstitut Senckenberg, Senckenberganlage 25, D-6000 Frankfurt/M. 1, W. Germany (their Courier 92). 273 pp. ISBN 3-924500-30-4. Softbound. DM 78.00 (about $43).

Ten papers, five in English. A revision of a new ammonite subfamily is noteworthy; other groups studied are tentaculites, corals, and conodonts, with geography and the Gedinnian/Siegenian boundary receiving most attention. One paper indicates intermediate strata where species have not been found as well as those where they have been: a feature worth emulating. -LMV

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**The Elements of Palaeontology.** Edition 2.
**Rhona M. Black.** 1988. Cambridge Univ. Press. xi + 404 pp. ISBN 0-521-07445-2 hardbound, $59.50; -09615-4 softbound, $24.95.

This is a competent and updated introduction to the major groups of fossils, requiring little background. Mammals have 20 pages to 3 for bryozoans, and the edrioasteroids and Gnetales (e.g.) aren't mentioned at all. The rather numerous examples are British whenever possible and each is briefly discussed. An unwary reader may be excused for thinking that a specimen which looks rather like a genus figured does belong there. -LMV

**Fossil Invertebrates.**
Edited by **Richard S. Boardman, Alan H. Cheetham,** and **Albert J. Rowell.** 1987 (July). Blackwell Scientific. xi + 713 pp. ISBN 0-86542-302-4. Hardbound. $49.95.

24 years for a good new introduction to invertebrate paleontology? The book has 27 authors, but that doesn't account for most of the delay. Anyway, it is indeed a pretty good book. After some fairly perfunctory introductory chapters on general matters, although without macroevolution, it gets down to the major phyla. These include fossil protists and are mostly well done (although one could quibble about this for, say, the smaller foraminiferans. It is not reasonable to assume, as the book does, that a separate course on micropaleontology is available or will be taken if it is.) There is a real attempt to impart a realization that the fossils are remains of real animals as well as stratigraphic markers. Lots of good photos and drawings. Soft-bodied groups are omitted, and some others, like myriapods and silicoflagellates, seem to have slipped through the cracks. There is a whole chapter on the Hyolitha, here treated as a phylum, but a photo of the comparably important Tentaculites in the chapter on preservation may lead a reader to wonder where its relatives are treated. (They aren't.) But all in all a well-done survey of the most necessary elememts of the subject. -LMV

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**Devonian-Carboniferous Boundary** -- Results of Recent Studies.
Edited by **Gerd Flajs, Raimund Feist,** and **Willi Ziegler.** 1988 (2 May). Forschungsinstitut Senckenberg, Senckenberganlage 25, D-6000 Frankfurt/M. 1, W. Germany (their Courier 100). (vii) + 245 pp. ISBN 3-924500-37-1. Softbound. DM 54.00 (about $30).

This volume exists to document candidate sections for the stratotype of the boundary, in one of which an imaginary golden spike will be fixed to define the boundary as a type specimen does for a species. The reviews are of more use than this, though, and indeed not all are even relevant to the official topic. There are careful descriptions and figures of several sections and of fossils relevant to correlation in the interval around the boundary. Detailed correlation is a notorious problem except when some event can reasonably be regarded as synchronous. One paper considers this problem and concludes that there are no unambiguous criteria for choice here: everything is diachronous if some other standard is used. Part of the problem is the absence of a crucial lineage from parts of the relevant sections, so there is room to hope that progress even with existing criteria is possible. -LMV

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**7. Beitrag zur Fauna und Flora der Kapverdischen Inseln.**
Edited by **Wolfram Lobin.** 1988 (15 October). Forschungsinstitut Senckenberg, Senckenberganlage 25, D-6000 Frankfurt/M. 1, W. Germany (their Courier 105). 0 + 220 pp. ISBN 3-924500-42-8. Softbound. DM 42.50 (about $25).

Mostly this volume is a source for original work on plants and arthropods of the Cape Verde Islands. There is also a paper reconstructing the biomes of Africa in the latest glaciation. -LMV

Vertebrate Paleontology and Evolution.
Robert L. Carroll. 1987 (stated 1988 in book). W.H. Freeman. xiv + 698 pp.
ISBN 0-716-71822-7. Hardbound. $52.95.

Since 1933 books on vertebrate paleontology have been measured in fractions of a Romer. Carroll was a student of Romer, but this is a new book rather than a revision. It is remarkably good, in both content and writing. Advances in knowledge since Romer's last edition in 1966 involve major changes, even some reconceptualization, in addition to the expected discoveries and revisions. These are surprisingly well covered. Yes, I don't agree with everything, and there are the inevitable factual errors too. (Particularly unfortunate is the mislabeling of some foramina on p. 405.) But what does the book do?

It is above all a straightforward presentation of the diversity and anatomy and phyletic interrelationships of extinct vertebrates; everything else is derivative. It is accessible to undergraduates, if not entirely so to absolute beginners, but any professional will find personally new information here too. Mammals have traditionally occupied about half the length of books on fossil vertebrates; here they are exceeded by reptiles alone. Even teleost fishes, usually abandoned in despair, have a whole chapter for the ancestors and aunts of their 20,000 recent species (there are 4000 recent mammals). Birds get a rather cursory (but modern) treatment compatible with our relatively poor knowledge of their easily destroyed bones. An appendix updates Romer's classification to the generic level, with approximate stratigraphic ranges; it has occasional inconsistencies with the text. There are many figures, mostly line drawings, well chosen and usually well done. The phylogenetic diagrams usually balloon out to indicate approximately the changes in relative diversity.

A lot of tidbits on function are included, and Carroll usually gives some attention to adaptive phyletic transitions. Various topics get singled out for treatment; human evolution is the only place where individual species are named. Molecular evidence on phylogeny is included here and elsewhere. A final chapter deals dispassionately but critically (as always) with aspects of evolution at the species level and above. Here and elsewhere opposing views and evidence are often cited. An initial chapter considers the fossil record, how to estimate phylogenies, and methods of classification. Carroll advocates the synthetic approach to classification but gives a good introduction to cladism; oddly, in the text he sometimes abandons the synthetic approach, with predictably awkward and adaptively unrealistic results.

There is even some attention to physiological evolution, as to biogeography, but not much to development, ecology, or faunal evolution. The last is a major aspect of paleontology and I find its virtual omission no less than shocking. Romer gave three chapters to it. Taxa don't evolve by themselves, and as with other subjects there are modern approaches. One can't include everything, and Carroll's book is appreciably larger than Romer's, but as with budgets it is all a matter of priorities.

Even a little humor creeps in. It was me, though (in a four-page paper published in 1970, complete with erudite footnotes), not Novacek, who derived the Rhinogradentia (extinguished in a nuclear test) from elephant-shrews. This relates to probably the single really maladroit feature of the book, its treatment of references. Usually only a recent paper is given, which is fine if the paper refers to earlier work and there is no implication that the discovery was by the author of the paper. However, usually the reference is given so as to imply or even state that the reviewer (including, again, me) made the discoveries reviewed. Some papers referred to don't even include reviews or appropriate earlier references. Carroll's occasional reference to earlier work only compounds the problem with respect to topics which have only a recent reference.

These are quibbles. The book is superb. -LMV

**The Vertebrate Body.** Edition 6.
**Alfred Sherwood Romer and Thomas S. Parsons.** 1986. Saunders. vii + 679 pp. ISBN 0-03-058443-4. Hardbound. $46.75.

This is the standard text and low-level reference on comparative vertebrate anatomy. It retains its advantages of clarity, coverage, overall accuracy, and evolutionary emphasis. Phylogenies rather than cladograms are appropriately retained. Function is covered but is subordinate to the thoroughly comparative emphasis which unifies the book. Unfortunately the parts bearing on mammalian evolution have not been updated adequately; a mammalian paleontologist should go over these aspects for the next edition, although Parsons, as the surviving author, has responsibility. A good choice despite a bit of remediable fading. -LMV

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**Functional Anatomy of the Vertebrates:** An Evolutionary Perspective.
**Warren F. Walker, Jr.** 1987. Saunders. xiv + 781 pp. ISBN 0-03-064239-6. Hardbound. $46.75.

Like Hyman's classic, this book evolved from a lab manual. Unlike hers, it doesn't purport to be one any more; the lab manual still exists as a separate book. It's a good treatment overall -- clear, integrally comparative, and almost as comprehensive as Romer and Parsons. It's a bigger book than the latter, and it uses the extra room and a bit more to give greater attention to functional aspects. However, the emphasis does remain, appropriately, on the anatomy. An attempt to survey the vertebrates at the beginning doesn't do very well; the phylogenies are unsure at best (did you know that primates are closest to elephants?) and extinct orders are omitted only for placentals. The rest of the book is mostly accurate but with an occasional odd extrapolation of a human character to other mammals (e.g., a unified temporal bone). The remarkable history of mammalian cheek-tooth pattern gets a confusing treatment which misses the main surprise but is still better than Romer and Parsons's. A useful and promising book. -LMV

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**Type and Figured Specimens of Fossil Vertebrates in the Collection of the University of Kansas Museum of Natural History.**
University of Kansas Museum of Natural History, Occasional Publications. No ISBNs. Softbound.
**Part III. Fossil Birds.**
**John F. Neas and Marion Anne Jenkinson.** 1986 (5 February). Vol. 78. 14 pp. $5.00.
**Part IV. Fossil Mammals.**
**Gregg E. Ostrander, Assefa Mebrate, and Robert W. Wilson.** 1986 (21 November). Vol. 79. 83 pp. $18.00.

These are actually somewhat useful lists. There are appropriate annotations, especially for the birds, and each list is preceded by a brief history of that segment of the collection. A new generic name is provided for a Miocene mole. A close and naive observer might think that charadriiform families are in the Ichthyornithiformes, and the arctocyonid condylarth Deltatherium is placed for the first time in the Hyopsodontidae, I hope (but am not sure) inadvertently. -LMV

**Form and Function in Birds.** Volume 3.
Edited by **A.S. King** and **J. McLelland**. 1985 (October). Academic Press.
xiii + 522 pp. ISBN 0-12-407503-7. Hardbound. $126.50.

This is the standard treatment of the functional anatomy of birds, to be completed in four volumes. Comparative aspects, especially variation among different bird groups, are also emphasized, and there is enough descriptive anatomy (adequately figured) to make each overall account self-contained. Not much development, though. Original interpretations and observations are included by the authors as appropriate; each review is a critical account. The authors indicate the sometimes extensive amount of work which remains to fill in current perspectives. This volume treats the integument, especially feathers; the locomotor system; the spinal nerves; and the several sense organs.

Rails tend to become flightless on islands. They are terrestrial birds, with relatively high wing loading, and perhaps they participate in the island rule (small mammals get larger and large ones get smaller). If so, their ontogenetic allometry may not get the wings large enough to pass the threshold for flight, and there wouldn't be strong selection to change the allometry against the saving of unused mass. I'm sure there is enough information to see how leaky this hypothesis is. -LMV

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**The Birds of Africa.** Volume 3.
Edited by **C. Hilary Fry, Stuart Keith,** and **Emil K. Urban**. 1988. Academic Press.
xvi + 611 pp. + 32 colored plates. ISBN 0-12-137303-7. Hardbound. $129.00.

It's good to have a group that is well studied. This volume treats the nonpasserines from parrots on, in a usual sequence. The treatise itself is a beautifully produced and scholarly work, with new research (from the specimen level) having been done for it as needed. There is a map and painting for each species, and often one or more drawings. The information for each species is tersely presented and includes its names, range and status, brief description with subspecies and measurements, field characters, voice, general habits, food, breeding habits, and references. Higher taxa have a brief description and usually comments. The classification is modern but not hypermodern and is justified as needed. I am particularly glad to see weights given for all species and subspecies where that is known, which is most of them. As late as the middle 1960s weights were editorially deleted from the manuscript for a Bulletin of the American Museum (on New Guinea birds) as not worth publishing. The treatise is a remarkable achievement, both authoritative and attractive. Imagine a book like this for African mites. -LMV

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**Neogene Avian Localities of North America.**
**Jonathan J. Becker**. 1987 (December). Smithsonian Inst. Press. (v) + 171 pp.
ISBN 0-87474-225-0. Softbound. $14.95.

A useful little book. Neogene is used in the narrow sense, excluding the Pleistocene, but even so there are a fair number of birds and localities for them. Most of the book is a set of lists of the birds reported for each locality, often with new identifications but no new taxonomy. Evidence on age is given or referenced, and there are often comments on one aspect or another of the locality. There is a good index and correlation charts, and two maps should be helpful as a survey. Anybody want to try something like this for the mammals? It's a benchmark, and a good one. -LMV

**Neural bone patterns and the phylogeny of the turtles of the subfamily Kinosterninae.**
**John B. Iverson.** 1988 (1 October). Milwaukee Public Museum, 800 W. Wells St., Milwaukee, Wis. 53233 (their Contributions in Biology and Geology 75). 12 pp. ISBN 0-89326-159-9. Softbound. $3.95.

There is a nice morphocline in the neurals, but by weighting the loss of clasping organs more heavily it gets broken up. Why one should prefer this result isn't clear to a noncheloniologist. In each of three cladograms, having either Sternotherus or Kinosternon evolve from the other would make the derivative genus at least triphyletic with conventional species allocations; I suppose we really will have to give up Sternotherus. Two species are mistakenly interchanged in Fig. 4. -LMV

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**Vorkommen, Taxonomie und Massnahmen zur Erhaltung der Forelle Salmo trutta labrax Pallas 1811 in der NO-Türkei.**
**Anton Lelek.** 1988 (16 May). 0 + 44 pp. Forschungsinstitut Senckenberg, Senckenberganlage 25, D-6000 Frankfurt/M. 1, W. Germany (their Courier 101). ISBN 3-924500-38-X. Softbound. DM 13.20 (about $7.50).

The fairly small populations of sea trout in northeast Turkey resemble those of northwest Europe. Some are landlocked, and the author suggests conservation measures. One doesn't need to put the author of a taxon, much less the date, in the title of a paper. -LMV

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**Définition et Origines de l'Homme.** Morphogenèse du crâne et Anthropogenèse.
Edited by **Michael Sakka.** 1987 (stated 1986 at heads of papers). Paris: Editions du Centre National de la Recherche Scientifique. 0 + 375 pp. ISBN 2-222-03859-6. Softbound. 350 F (about $60).

Not very much meat here. There is an unusually high proportion of potboilers, among both (supposedly) original papers and reviews. There is some rather dull work on things like the function and growth of the skull (and even a review of work on amphibian brain size!); the quality of other papers may be inferred from those I mention as best. Sakka and Aguirrebengoa show that the meningeal circulation isn't fully represented in intracranial grooves, as claimed by others; variation still isn't discussed. Wood and Dean suggest that robust australopithecines form a clade with Homo because their foramen magnum is also forward (and two other characters which, as they don't mention, more or less follow from this). Martin gives a useful treatment of allometric aspects of entire human brains.

And de Bonis, after showing that all European specimens referred to Homo erectus share derived characters with neanderthals (an important result), removes them from H. erectus despite their age of as much as 700,000 years. He doesn't even consider the possibility that they represent racial variation in H. erectus, which seems much the most likely situation. Perhaps, as at least Krantz (1980, Evol. Theory 4: 223-229) has advocated, even neanderthals are late survivors of H. erectus in part of its original range, with continued intraspecific evolution of a larger brain. Or maybe by $10^5$ or so B.C. they do deserve species status, whether or not they evolved in broad evolutionary continuity with (earlier) H. erectus. In either case there is an interesting evolutionary picture. As I suggested more than 20 years ago (1966, Persp. Biol. Med. 9: 377-383), data for humans may provide the most detailed example of geographic evolution within species. -LMV

**Whales, Dolphins and Porpoises.**
Edited by **Richard Harrison** and **M.M. Bryden**. 1988 (1 November). Facts on File. 0 + 240 pp. ISBN 0-8160-1977-0. Hardbound. $35.00.

Books on shrews tend to be small and books on whales tend to be large. Shrews really are interesting, but this (big) book is about whales. Don't be fooled by its semipopular orientation and accessibility to the nonspecialist. It's a first-rate effort, by people who know what they are doing and put together into an unusually attractive package. Lots of colored photos, good to wonderful, and other dressed-up figures. There is even some original work here, notably in Klinowska's chapter on strandings. Besides the expected topics of the kinds of whales (including extinct ones), aquatic and other adaptations, and behavior, there is a dispassive treatment of intelligence (to me too critical in some ways and not enough in others) and a large chunk on interactions with people. Among other things the latter includes whales in art and literature (I'm glad to see that my view of Moby Dick resembles that of some official critics) and a remarkable tame population of free-living dolphins in Western Australia. I didn't see any actual errors, but a table of relative brain weights is misleading because it omits allometric relations -- if it had included a shrew the shrew would seem brainiest. Long may they thrive. Whales too, and maybe they will now, but the prospective balance is more fragile than the book implies unless we stop crowding the rest of our fellow earthlings off the planet. The book is surprisingly good. -LMV

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**Handbook of Marine Mammals**. Volume 3. The Sirenians and Baleen Whales.
Edited by **Sam H. Ridgway** and **Richard Harrison**. 1985 (September). Academic Press. xviii + 362 pp. ISBN 0-12-588503-2. Hardbound. $84.00.

This treatise provides authoritative accounts of species (or sometimes sets of closely related species) of extant marine mammals. With about 30 small pages per species, including figures, not everything can be covered adequately. Behavior (including food) and population status are the aspects emphasized, with other things brought in mostly insofar as they affect these. Some attention is given, though, to other matters, including anatomical peculiarities, and references are provided for those in need of more detail. A serious difficulty with the format is that comparative discussion is nearly absent. Thus, what is known about how baleen whales coexist? The relevant paper is cited in one account, but not for this purpose. For prospective users, there is a good index which lets specific subjects be found, which can be done otherwise only by plowing through. -LMV

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**Mammals in Wyoming.**
**Tim W. Clark** and **Mark R. Stromberg**. 1987. Lawrence: Univ. Kansas Museum of Natural History (distributed by Univ. Press of Kansas). xii + 314 pp. ISBN 0-89338-026-1 hardbound, 25.00$; -025-3 softbound, 12.95$.

Charles Long published a scholarly review of Wyoming mammals in 1965. Clark and Stromberg's is semi-popular, with a photograph of a living member of most species and brief accounts of the natural history of each species. Distribution and characters are also emphasized, although in less detail than Long gave, and the authors consider conservation aspects. No less than 16 species have been added to the state list since 1965. Neither book mentions our own species or its domestics, as is traditional in books like these, but commensals are included. The authors seem unaware of advances in house-mouse taxonomy, though. One of the better state mammal books. -LMV

**Untersuchungen zur Systematik der rezenten Caenolestidae Trouessart, 1898.** Unter Verwendung craniometrischer Methoden.
**Johannes Bublitz.** 1987. Bonner Zoologische Monographien, 23. (Zool. Inst., Adenauerallee 150, D-5300 Bonn 1, W. Germany.) 96 pp. ISBN 3-925382-23-2. DM 22 (about $12).

These are marsupials, which might have been mentioned in the title instead of the author of the family name. In fact they are relicts of a small Paleogene radiation of small ferocious insectivores in South America. Before Bublitz's revision there were seven species in three genera; after it there are seven species in two genera, but two of the species are new. Much of the revision relies on canonical (and discriminant) analyses of a large number of skull measurements, but exactly what these measurements are is not stated despite careful specification of landmarks on the skull. The numerical results probably estimate morphological distances adequately, though. An unusual and desirable feature is the presentation of drawings of several specimens of single species to indicate variation. There is no consideration of phylogeny. The new species of <u>Rhyncholestes</u> is justified only by the craniometric distance; the diagnosis is restricted to a crest on the upper canine in females, but there is only one specimen of each sex for each supposed species. Island populations, as one of these is, are notorious for rapid divergence; because the evidence is inadequate to support specific distinction I return <u>R</u>. <u>continentalis</u> to <u>R</u>. <u>raphanurus</u>. -LMV

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**Primate Evolution and Human Origins.**
Edited by **Russell L. Ciochon and John G. Fleagle.** 1987. Aldine de Gruyter. xii + 399 pp. ISBN 0-202-01175-5. Softbound. $23.95.

This is a nice compilation of 44 papers (occasionally extracts) from about 1960 to 1983. Most are paleontological, and the focus throughout is on phylogeny and adaptive history. The text of the papers has been reset onto large pages; some figures didn't reproduce well. There is no index, but a large and valuable joint bibliography occurs at the end. Various maps showing fossil occurrences are only approximate, which is not stated. As is customary, the editors introduce the several groups of papers with commentaries. All main groups of primates are represented, with an appreciable emphasis on hominids. There is an explicit and largely successful attempt to include a diversity of views, but no explanation is given for the four-year delay in publication. It's good to have at last, though.
-LMV

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**Mammalogy.** Edition 3.
**Terry A. Vaughan.** 1986. Saunders. vii + 576 pp. ISBN 0-03-058474-4. Hardbound. $46.75.

Mammalogists are lucky to have this text; many subjects aren't as well off. It does mostly ignore phylogeny, and the errors I noted were restricted to paleontology (expanded in this edition) and references to figures. The treatments of physiology and behavior are also enlarged, and overall updating is well done. Half the book is on the orders and families, half on taxonomically general subjects. -LMV

**The Mollusca** (K.M. Wilbur, ed.) Volume 10. **Evolution.**
Edited by **E.R. Trueman** and **M.R. Clarke**. 1985 (December). Academic Press.
xx + 491 pp. ISBN 0-12-751410-4 hardbound, $98.00; -728702-7 softbound, $54.50.

Evolution covers a multitude of sins and virtues. What this volume treats is adaptive radiation, plus what is probably the first extended version of Vermeij and Dudley's argument that freshwater clams are less evolved in defenses against predators than are contemporaneous marine clams. A discussion of some sorts of adaptations of limpets, a life form convergently evolved many times, also doesn't deal explicitly with phylogeny.

As with other groups, knowledge of the adaptive radiation of mollusks has recently undergone, if not a renaissance or revolution, at least considerable upheaval and progress as a result of the widespread adoption of the viewpoint that phylogeny really is something appropriate to study. This book draws much of the work on mollusks together and provides what is probably the best treatment of adaptive radiation for any group outside vertebrates and arthropods. It should be read by many people other than malacologists, but its inclusion in this series may inhibit that.

All groups except cephalopods (treated elsewhere) are considered, with attention to both fossil and recent representatives and evidence. There is a discussion of the origin of mollusks. Not surprisingly, authors whose chapters partly overlap in coverage don't agree on everything, nor do I agree with the treatments of molluscan origins (see my essay elsewhere in this issue). Pulmonate land snails, which Solem concludes are probably polyphyletic although estimation of phylogenies remains unattainable, and anomalodesmatan clams, an adaptively diverse but declining group, receive chapters of their own. There is obviously a long way to go yet, but it's well done. -LMV

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**The Mollusca** (K.M. Wilbur, ed.) Volume 11. **Form and Function.**
Edited by **E.R. Trueman** and **M.R. Clarke**. 1988 (January). Academic Press.
xxviii + 504 pp. Acid-free paper. ISBN 0-12-751411-2. Hardbound. $115.00.

That is to say, form in relation to function. It would be difficult to compress a reasonable comparative and functional discussion of all molluscan structures into one volume, and that desideratum isn't even approached. What we do have is a group of treatments of some of the major structures, with "function" varying from ecological adaptation to biomechanics. Some chapters are explicitly comparative, others less so or not at all. Extinct mollusks are mostly ignored even in chapters on the shell, including one by a paleontologist. The editors provide a masterly if brief introduction; not much of the rest of the book is up to their standard, but I should mention a wide-ranging treatment of macroscopic aspects of the digestive system, by von Salvini-Plawen. -LMV

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**The Mollusca** (K.M. Wilbur, ed.) Volume 12. **Paleontology and Neontology of Cephalopods.**
Edited by **M.R. Clarke** and **E.R. Trueman**. 1988 (January). Academic Press.
xxiv + 355 pp. Acid-free paper. ISBN 0-12-751412-0. Hardbound. $110.00.

Cephalopods are the only group to get a volume to themselves in this series, and aspects of them have been considered in most of the other volumes. This volume is unusual, though, for a treatise, in that most of it represents original research rather than review of earlier work. Even Teichert's review of fossil cephalopods differs in approach from what is done elsewhere for this general subject in being chronologically and adaptively oriented.

The rest of the book concentrates on recent coleoids (octopuses [not "octopi"] and squids) and the fossils in their radiation. Apart from an orphan chapter on Mediterranean biogeography, the chapters examine in some detail a variety of features which are, or might have been, useful in estimating the phylogeny of the group, including hard parts as yet little used in paleontology. Several chapters consider specific groups, and there is an integrative concluding chapter. No mention is made of using molecular evidence, which may not be available yet but which has usually been helpful for other groups studied in this way. The volume is a major resource and must be consulted in any work related to coleoid evolution.

Cephalopods lack any trace of spiral cleavage in their development, but they are clearly derived from ancestors which had it. Even flatworms have it, and this complex and nearly stereotyped cleavage has been a block to regarding flatworms as close to prototypical bilateral metazoans. Cephalopods got out of the stereotype by evolving eggs with yolk (as arthropods did); perhaps this happened more frequently, with yolk subsequently lost. -LMV

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**Cephalopod Life Cycles**. Volume 2. Comparative Reviews.
Edited by **P.R. Boyle**. 1987 (June). Academic Press. xxi + 441 pp. ISBN 0-12-123002-3. Hardbound. $140.00.

Without the more sedate Nautilus around to complicate things, one could think of cephalopods as the jet-set of invertebrates in their lives as well as in their evolution. They grow about as fast as mammals and do so at lower temperatures. they live for a few months to three years or so (maybe twice that for the giant squids), reproducing in a burst or a fairly short dribble before succumbing to a possibly programmed death. Programmed death is a heresy these days, as Calow notes in a theoretical chapter, but it isn't as mysterious as he thinks. Postreproductive suicide can evolve in the same way as any other form of senescence. Whether cephalopods really do it, and if so just how, seems not really resolved, but in any case one shouldn't say that it doesn't exist because it can't.

Oddly, cephalopods apparently can't digest lipids. (Can other mollusks?) Some squids may fuel a long migration to the warm waters where they breed by eating others in the same school. The absolute abundance of most cephalopods is best (!) estimated by their occurrence in stomachs of their predators. There are interesting similarities with nudibranchs, which leads Boyle to suggest that they lost their shells because the shell can't grow as fast as the rest of the animal. Anybody out there know what limits maximum growth rate of shells? This would suggest fast growth for belemnites too. Ward and Bandel review evidence for smaller eggs by ammonites than for nautiloids, although Nautilus itself has the largest ones. (This evolution should be decipherable with earlier Cenozoic nautiloids.) There are lots of things like these in the book, which has mostly new syntheses of most of the topics one might expect from its title as well as some others. -LMV

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**Acarology**. Mites and Human Welfare.
**Tyler A. Woolley**. 1988 (4 January). Wiley. xix + 484 pp. ISBN 0-471-04168-8. Hardbound. $59.95.

What a marvelous book! Mites are mostly too small to empathize with easily (and who wants to empathize with a tick anyway?), too big to treat as animated reaction chambers like bacteria. They get a glance, if that, from those attuned to their more conspicuous cousins the spiders and insects. But there are a lot of them. They don't encompass the globe like nematodes but they are still all around us (and in us and on us). I don't know how it could be done, but it would be nice to have an estimate of the number of species in a small patch of rain forest like Erwin's for beetles. Would that give us an estimate of $10^7$ species of mites too? And how about energy flow?

The book doesn't deal with real ecology, which is a shame because interesting work has been done by Mitchell and others. One needs real ecology in order to understand a mite's problems, what it's like to be a mite, why a mite does what it does rather than just how. One can't fault an author for not writing about something he doesn't know, and I don't mean to do so (no, I don't), but there is a large lacuna. Other questions could be raised too, such as whether the origin of mites was accompanied by paedomorphosis in connection with their small size, as probably happened with insects from hexapod larvae of extinct myriapods. How does the minute size of some mites (120 microns) affect the number and size of their cells and the immediately supracellular organization? Does the haplodiploid sex determination in spider mites (Tetranychidae) affect the nature of their aggregations?

What we have is a broadly comparative treatment of mite anatomy and some physiology, from a functional perspective, plus a once-over of the groups of mites as more or less living animals. This is a masterly account and is by far the best available. (Information given on non-mites, including outdated phylogenies, is less sure.) The book should be of interest to nonacarologists, if any ever see it, and the writing makes it accessible to any naturalist. Did you know that water mites still have tracheae, but these are closed tubes under the cuticle? There is a poem by Frost about a mite, excerpts and figures from Hooke's Micrographia, and humor. The world is full o'mites, to look upon and cherish, with apologies to no one in particular. -LMV

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**Evolution and Adaptation of Terrestrial Arthropods.**
John L. Cloudsley-Thompson. 1988. Berlin and New York: Springer-Verlag.
x + 141 pp. ISBN 0-387-18188-1. Softbound. $33.00.

When we think of terrestrial arthropods we think of insects, maybe spiders, maybe myriapods and mites after a little thought. There are lots more, though. Several groups even of crustaceans have come out; an isopod is one of the commoner animals in the Sahara, and amphipods are more numerous than collembolans in the litter of many rain forests.

You won't find that sort of information in this book. There isn't even a survey of what groups of terrestrial arthropods exist, much less a consideration of how many times an emergence has occurred in evolution. Cloudsley-Thompson is an environmental physiologist, and that is the strength of the book. There is a little consideration of phylogeny and paleontology, and more of behavior and functional anatomy as extensions of physiology. Adaptive trends on land are emphasized rather more than adaptations to a terrestrial existence itself.

Lots of topics get brief but usually authoritative once-overs. Insects have a large but not overwhelming place, and the errors I noted are mostly for them. These are too frequent, in relation to the limits of my own knowledge, to warrant real trust in at least non-physiological topics. The author has a flair for the interesting, though, and the book is a joy to read. -LMV

**Evolutionary Biology of Orthopteroid Insects.**
Edited by Baccio M. Baccetti. 1987 (14 December). Ellis Horwood (distributed outside Britain by Wiley). 0 + 612 pp. ISBN 0-470-20882-1 for Wiley. Hardbound. $126.00.

This is a collection of mostly humdrum papers on what is usually called the order Orthoptera, not on all orthopteroid insects except in some cases for comparison. There are papers on aspects of biogeography, on polymorphisms, including chromosomal, on comparative and functional anatomy, including eggs, on systematics, on calls, and so on. No real genetics, development, ecology, adaptive strategies, etc.: the focus, if there is one, is on interrelationships of these insects. Hewitt argues from the structure of hybrid zones that stasipatric speciation is unlikely; he makes a good case and should repeat his argument somewhere else where workers on other groups may see it. Bullini and Nascetti find a number of cases where orthopteran species have originated from hybrids, sometimes then displacing a parental species.

The longest paper is by the editor and is probably the most valuable in the book. He gives evidence from sperm structure, extensively and attractively illustrated, on the major phylogeny of most groups of extant orthopteroids. He concludes (among other things) that the Orthoptera sensu stricto, and even the suborder Ensifera, are polyphyletic. The conclusion may perhaps be correct but the argument for it is not. It is based merely on the lack of shared derived characters in the sperm. He gives no evidence that different orthopterans originated from different members of an ancestral group. (The Protorthoptera being extinct, and so not mentioned, he uses the Dictyoptera as ancestral.) It is entirely consistent with his evidence that the Orthoptera is monophyletic and diverged in its sperm some time after its origin. This resembles the opinion of Paranjape et al., who discuss evidence from feeding and the gut, but their evidence on this point is no better. -LMV

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**Sponges of the Burgess Shale.** Palaeontographica Canadiana 2.
**J. Keith Rigby.** 1986. Canadian Society of Petroleum Geologists. (iv) + 105 pp. No ISBN. Softbound. Can.$25.00 (about US$21).

The grand master of sponges has taken on the Burgess Shale (and other localities) and won again. There is a general and critical review of middle Cambrian and earlier sponges and reports thereof, in addition to the expected comparative descriptions with line drawings and photographs (and with 8 new genera and 2 new families). Phylogenies too, with some major new aspects. The almost basal radiations of the Demospongia and Hexactinellida get documented and sorted out, with several early trends isolated. Fine stuff. The glue on the "perfect" binding, though, lets some pages fall out. -LMV

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**Bibliographie lichenologischer Bibliographien.**
**Wolfgang Korth and Doris Rückert.** 1988 (30 September). Forschungsinstitut Senckenberg, Senckenberganlage 25, D-6000 Frankfurt/M. 1, W. Germany (their Courier 104). (ii) + 153 pp. ISBN 3-924500-41-X. Softbound. DM 25.50 (about $30).

A list of papers, and series of papers, on lichens which themselves have more than 50 references. There is an index to the papers listed. -LMV

**Palaeobiology of Conodonts.**
Edited by Richard J. Aldridge. 1987 (27 January). Ellis Horwood (USA: Wiley). 0 + 180 pp. ISBN 0-470-20788-4. Hardbound. $72.95.

Not surprisingly, mineralized tissues are ordinarily poor in cells. Bone does have a few but most kinds have none at all. It now turns out, from work by the Fåhraeuses reported here, that decalcified conodonts are highly cellular. Some subcellular structure is even preserved, with nuclei and nucleoli visible and brought out by a retained reaction to normal histological stains. The cells themselves are rather diverse, and the tissue is unique overall. That the cells were active is indicated by a high vascularization. Although some sections were only partly decalcified, it is not stated just how the mineralization occurs in relation to the cells. The remarkable preservation within the protective mineralization suggests that molecular, histochemical, and immunological studies could possibly be productive, the only reasonable case I know before the late Cenozoic.

For not everyone yet agrees that conodonts came from jawless vertebrates. The chaetognath and cephalochordate theories are supported by authors in this book, and the nemertean theory may also survive. A new conodont animal, Silurian this time, is rather different in shape from the Carboniferous ones but is poorly preserved.

Other papers discuss the reconstruction and functioning of conodont apparatuses, again with different conclusions. A final group looks at topics such as extinctions and fluctuations in diversity with repeated adaptive radiations of different scope. The top of an early Ordovician biomere has the conodonts going out rather abruptly, but doing so a few meters above an equally abrupt extinction of trilobites. Evidence for progressive shallowing of the sea accompanies this.

-LMV

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**1st International Senckenberg Conference and 5th European Conodont Symposium (ECOS V). Contributions.**
Edited by Willi Ziegler. 1988 (20 July). Forschungsinstitut Senckenberg, Senckenberganlage 25, D-6000 Frankfurt/M. 1, W. Germany (their Courier 102 and 103). Softbound.
Courier 102: 0 + 307 pp. + corrected reprint of pp. 263-307. ISBN 3-924500-39-8. DM 60 (about $35).
Courier 103: 0 + 245 pp. ISBN 3-924500-40-1. DM 50 (about $30).

Courier 102 contains field guides to the type areas of the Eifelian to Famennian stages of the Devonian, and the Rhenish and Harz Mountains, also mostly Devonian; abstracts of the conodont meeting; and a longish review by Sandberg et al. of the late Devonian mass extinction, with various new evidence. The field guides are of the best sort, with analyses and comparisons as well as descriptions. The extinction paper concentrates on Euramerica, especially the conodonts thereof, but in that scope takes a wide view. (An abstract by Ji gives similar results for the South China semicontinent.) The evidence is well presented, for a small set of sequential extinctions and associated environmental perturbations. With no visible justification whatever, though, as far as I can see, the authors then attribute the whole set of extinctions to a possibly single bolide. It's vaguely conceivable, but why should anyone believe it?

Courier 103 is a thorough bibliography of the Conodonta through 1986, with some bibliometric analyses.

Everything here is in English.

-LMV

**Atlas du Phytoplancton Marin.**
Paris: Editions du Centre National de la Recherche Scientifique. Softbound.
Volume 1. Introduction, Cyanophycées, Dictyophycées, Dinophycées, et Raphidiophycées.
**Alain Sournia.** 1986. 0 + 219 pp. ISBN 2-222-03823-5. 150 F (about $25).
Volume 2. Diatomophycées.
**Michel Ricard.** 1987. 0 + 297 pp. ISBN 2-222-03987-8. 200 F (about $35).

A tour de force. (At least that much French should be understandable to most readers.) This treatise, to be completed in three volumes, is the first worldwide review of marine phytoplankton to the generic level. It is remarkably well done. The coverage is of extant free-living marine protists, planktonic for at least most of their life cycle, in groups at least some of which have chloroplasts. (Their achloroplastic relatives are included.) Each class has a general treatment, longer for the more diverse classes, while families and orders receive about the same space apiece as genera. Diatoms even have a glossary. Keys allow identification to genus, and each genus has at least one figure: more than 1500 figures altogether. These are photographs (including scanning electron micrographs, especially for diatoms) or drawings, many being original (especially, again, for diatoms). They are clear and show what should be shown; some are also minor works of art, in Haeckel's tradition. For each genus there is relevant nomenclature, a diagnosis, the number of marine species (including synonyms), and discussion of aspects appropriate for the genus. Current taxonomic problems receive due consideration, although not all alternatives to the classification used can of course be discussed. Accuracy, clarity, and comprehensiveness are uniformly excellent. The treatise should be in all libraries which include natural history; it is by far the most useful basic work in its burgeoning area. -LMV

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**The Biology of Marine Fungi.**
Edited by S.T. Moss. 1986. Cambridge Univ. Press. xii + 382 pp.
ISBN 0-521-30899-2. Hardbound. $49.50.

One doesn't usually associate fungi with the ocean, and in fact fewer than one in a hundred of the described species occurs there. That doesn't in itself mean that they are unimportant element of the marine biota. One paper concludes hopefully that they may well be important in regulating populations of marine organisms, but the evidence for this is merely the occasional finding of fungal-caused mass mortalities. The authors could have guessed right, though, because so little is known. Another paper claims to distinguish between ascomycetes which have never evolutionarily left the ocean and others secondarily marine. Oddly, no phylogenetic evidence is used in doing so, just an assertion of a generalized Farenholz's Rule (for parasites, that they evolve with their hosts and don't transfer among them). This rule has so many exceptions as to be nearly worthless as a basis for phylogeny; it would be good if someone would give evidence that any Eufungi are really ancestrally marine. Overall, the book is a useful progress report on quite diverse aspects of marine fungi; unfortunately there doesn't seem to be much happening with them yet. There are also a couple of good papers on two apparently related (unlike earlier views) nonfungal protists, the Labyrinthulales and Thraustochytriales, which happen to be studied mostly by mycologists. The broader affinities of these groups remain obscure although they may be an early branch of eukaryotes. -LMV

**Foraminiferal Genera and Their Classification.**
**Alfred R. Loeblich, Jr., and Helen Tappan.** 1988. Van Nostrand Reinhold. 2 volumes: x + 970 pp; viii + 212 pp. + 847 plates. ISBN 0-442-25937-9. Hardbound. $199.95.

Wow. The authors weren't content to review all the genera and higher taxa of forams for the Treatise on Invertebrate Paleontology in 1964 -- they do it again here. The part of the Treatise that isn't strictly taxonomic has no counterpart here. 2455 genera are now recognized, more than twice as many as in the Treatise and with actually fewer synonyms despite an increase by half in the number of available names. Higher taxa also suffer an apparent inflation, even more marked. Despite this, there are some anomalies. Thus the Globorotaliidae (a major "family" of planktonic forms which I would, among others, include in the Globigerinidae but which is here separated at the superfamily level) is said to occur throughout the Cenozoic, despite the well-known occurrence of separate radiations of its forms from globigerinid-like ancestors in the Paleogene and Neogene, separated by a total extinction. The authors don't even comment on this. Presumably there is reasonable uniformity in criteria applied throughout the book, which makes it valuable for comparative purposes, but a narrow focus does tend to magnify real differences. Is there any other order, of anything, which has 12 suborders and 74 superfamilies? (Yes, maybe it shouldn't be just an order, but that's a problem also not addressed.)

The book is nevertheless a remarkable achievement. The authors are known for their attention to detail in both nomenclature and characters, and the depth of their coverage of the literature. Quite a bit of original work from specimens went into the book, and various changes are based on this as well as on the literature. There are even several new taxa, although the authors were able to publish most elsewhere first. There are, as before, full synonymies, adequate diagnoses (although comparable characters aren't always indicated for related families), often taxonomic or nomenclatural comments, and geographic and stratigraphic ranges. The latter are now given to stage or less when possible, which it usually isn't for the Paleozoic. There is a glossary of morphological terms and an index, but the lack of a table of contents means that the higher taxa for a particular genus may be hard to find. Every genus has one or usually more good figures (unfortunately separated from their legends), usually from type species and often from their type specimens, and most generic synonyms have the same treatment. That isn't available for any other group, even birds. -LMV

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**Gasteromycetes.** Morphological and Developmental Features, with Keys to the Orders, Families, and Genera.
**Orson K. Miller, Jr., and Hope H. Miller.** 1988. Mad River Press, 141 Carter Lane, Eureka, Cal. 95501-9528. x + 157 pp. ISBN 0-916422-74-7. Softbound. $24.95.

These are the puffballs, stinkhorns, and related basidiomycete fungi. Some adaptively similar genera from other orders are also included. The coverage is worldwide, and the book is a gem. It tells what the various taxa are and do, gives careful drawings for most genera and even some distinctive species, and discusses character diversity. One doesn't need to be a mycologist to read it; the writing is clear throughout and is accessible to any naturalist. In addition to ten pages of glossary there are ten for a bibliography of original papers and reviews. Some genera are omitted, notably many of those which fruit underground and those too poorly characterized to meet the standards of the book. This wonderful little press makes a habit of putting out books like the one here, and it is one of their best. They should be better known. -LMV

**How to Identify Mushrooms to Genus. VI: Modern Genera.**
**David L. Largent and Timothy J. Baroni.** 1988. Mad River Press (as above).
(viii) + 277 pp. ISBN 0-916422-76-3. Softbound. $22.95.

Another exquisitely useful book from what is really the premier publisher of books for serious naturalists. This one doesn't have figures except to decorate the covers, but they can be found in earlier books in this series. What it does do, no less, is to provide the best and most accessible ways to identify any mushroom. If it is growing on dung, or on sand dunes, or in greenhouses, or in various other unusual situations, there is a special key for the few genera involved. More generally, one can start with the overall form and proceed fairly rapidly. To be most accurate there are keys to families and then to genera using characters which may be awkward to observe but which the mushrooms themselves find valuable. Each genus has a clear description, with references to keys to species in other books and with comments which often give critical views on the taxonomy. There should be an index; the classification (awkwardly placed in the middle of the book) and the organization of the book help but aren't enough. If you really want to know what mushroom you have, start here. -LMV

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**Fundamentals of Palaeobotany.**
**Sergei V. Meyen.** 1987. Chapman and Hall. xxi + 432 pp. ISBN 0-412-27110-9.
Hardbound. $110.00.

Fundamentals, yes. The book goes about as deep as paleobotany goes and gives a dense survey of its center and some peripheries. There are rather brief treatments of algae, fungi, bryophytes, and (only) early angiosperms, while the other vascular plants get fuller coverage. The illustrations are clear and informative line drawings, better for a nonspecialist than the photographs more usually found. The text itself, though, isn't for an absolute novice: one ought to have an orientation from elsewhere or be prepared to extract it from the informative but fairly compendious treatment here. That isn't to say that concepts are murky. They are clearly and sometimes incisively given. The late author had his own viewpoints and the book presents them; they are usually worth pursuing. He has an appropriately paraphyletic division Propteridophyta for the basal vascular plants, but his Pteridophyta then becomes polyphyletic without this being discussed. Higher taxa named from foliage genera are renamed; we lose even Glossopteridales. Asian material has an appropriately fuller treatment than in any other book. The translation from Russian is unusually good, although the characteristic Russian heaviness of language can't be avoided. There are separate chapters for such topics as palynology (unusual), cuticles (a first), paleoecology, and relation to other subjects, of which latter I found an essay on morphology most valuable. A highlight of the book is its extensive treatment of the evolution of floras, with those of the Cenozoic done by M.A. Akhmetyev. The emphases for floral evolution are on paleogeography and climatic zoning, with only sporadic integration with phylogeny. It is somewhat difficult to get an overall picture of the evolution and rates of change of the floras. Overall the book is the best available for finding information in paleobotany. -LMV

**Origin and Early Evolution of Gymnosperms.**
Edited by Charles B. Beck. 1988 (14 December). Columbia Univ. Press.
xiv + 504 pp. Acid-free paper. ISBN 0-231-06358-X. Hardbound. $65.00.

A gymnosperm is a plant with seeds but without flowers. Or is it? In this book Rockwell and Scheckler note that the difference between pteridophytous and gymnospermous reproduction involves several groups of traits, such as (?wind) pollination, which evolved before fully developed seeds. As with most of the other chapters, they give a fairly detailed and illustrated account of relevant fossils. It isn't clear yet whether gymnosperms are monophyletic or diphyletic, seedferns and conifers being central to the two relevant groups, but the ancestors in any case are included in the progymnosperms, a group recognized only twenty years ago and also reviewed here. (The foliage of seedferns does seem to be convergent on that of ferns rather than directly derivative.) Another recently recognized group with its own chapter, the Cheirolepidiaceae, has proved to be the most diverse family of Mesozoic conifers. It seems to be characterized only by its pollen, which is partly convergent on that of angiosperms, while its vegetative parts are diverse and had in many cases been assigned to several extant conifer families. There is a lot more in the book on phylogeny, diversity, adaptation, and structure of most groups of gymnosperms. -LMV

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**Phytolith Analysis: An Archaeological and Geological Perspective.**
Dolores R. Piperno. 1987 (December; stated 1988 in book). Academic Press.
xii + 280 pp. Acid-free paper. ISBN 0-12-557175-5. Hardbound. $49.00.

Phytoliths are bits of mineral (here unfortunately restricted to the more diagnostic ones made of opal, i.e. hydrated silica) which a good proportion (maybe half?) of vascular plants secrete in some of their cells. Those phytoliths in cell walls may substitute for lignin, but their main function appears to be as a defense against chewing herbivores, including and perhaps especially insects, although the book doesn't go into this. Their abundance in <u>Equisetum</u>, omitted in a table showing occurrence, led to the former use of this pteridophyte in scouring dishes and to its common name. The high-crowned teeth of many mammals permit longer life in a gritty environment, the major grit often being phytoliths, and teeth which grind grit get charasteristic scratches.

Opal phytoliths come in quite a diversity of shapes, often diagnostic even to the species level, and (after a short discussion on physiology) that is what the book is about. They lack the beauty of diatoms and radiolarians but may prove to be as useful in stratigraphy (also not discussed) and environmental reconstruction. Piperno enthusiastically, and perhaps correctly, regards them as potentially as useful as palynomorphs for this. She includes keys for identification (but be careful because of large gaps in knowledge) and a tutorial on technique for newcomers. I hope that some of these newcomers will concentrate on times earlier than the late Pleistocene (still with us, pending the next glaciation not long after the temporary greenhouse goes away), where Piperno's interests lie. There could be surprises. -LMV

**Plant Evolutionary Biology.**
**Edited by Leslie D. Gottlieb and Subodh K. Jain.** 1988. Chapman and Hall. xv + 414 pp. ISBN 0-412-29290-4 hardbound, $95.00; -29300-5 softbound, $37.50.

This is the second Festschrift for Stebbins, who is still intellectually lively. He has a chapter here, with new perspectives as usual. He concludes a list of differences between plants and animals related to the nature of their species (which doesn't overlap a recent one by Carson elsewhere) with the comment that "the complexities raised by these differences will be explored in another essay."

The papers do in fact represent a good sampling of current work in the broad area covered, at least if one interprets plants as angiosperms. There are no potboilers, unless Grime's reiteration of his classification of adaptive strategies could be considered such (but even it is partly original), and some papers even have original experiments reported. The papers are solid (not stolid) to bright (not flighty) and are complemented by valuable commentaries by the editors. In fact, unusually, these commentaries are one of the highlights of the book. Others are Birky's review of evolution of plant mitochondrial and chloroplast genomes (including the best available comparison of rates) and a paper by Antonovics, Ellstrand, and Brandon, the latter officially a philosopher. It presents the latest experiment on the evolutionary significance of sex in the context of an original and sophisticated analysis of the nature of the environment. What of the environment is relevant depends, though, on what it is relevant to, i.e. on just what questions are being asked, so the analysis is less generalizable than claimed. -LMV

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**Four Billion Years**: An Essay on the Evolution of Genes and Organisms.
**William F. Loomis.** 1988 (May). Sinauer. xvi + 286 pp. ISBN 0-87893-475-8 hardbound, $39.95; -476-6 softbound, $22.95.

A developmental biologist writing a book on evolution? That this seems strange comes from the historical lack of accommodation between these disciplines rather than from a lack of real overlap in what they study. Actually Loomis's approach is even more toward molecular genetics, but what value the book has is probably developmental. I don't really see a point in a balanced evaluation of the book, though. It's just a bad show and there are better books available at its level. Perhaps one shouldn't be surprised at the author's unsure treatment of paleontology, but to implicitly treat evolution as a scala naturae, get major phylogeny grossly wrong (did you know that the eukaryotic host cell was a photosynthetic bacterium?), explicitly say that natural selection occurs only on viability, and so on -- really! There are two more or less original themes through the book: that major evolutionary steps depend on very few genes for their whole evolution, and that the concentration of atmospheric oxygen has had a major evolutionary role even in the Phanerozoic (including the origin of placental mammals [in the Jurassic] and a new off-the-cuff hypothesis for dinosaur extinction). Like the book, neither is worth pursuing from the arguments given. -LMV

**Gene Activity in Early Development.** Edition 3.
**Eric H. Davidson.** 1986 (September). Academic Press. xv + 670 pp. + 2 folded charts. Acid-free paper. ISBN 0-12-205161-0. Hardbound. $49.50.

A rather extraordinary book. It's restricted to animals, but within that scope it is fully comparative, using evidence from any available organism and making explicit and appropriate comparisons as syntheses. (Developmental biologists commonly do this only implicitly, which makes the message harder for others to decipher.) The phylogeny on which some of these comparisons rest is less up to date than the subject of the book itself, though. Davidson uses any source, classical or modern, if of adequate quality; the bibliography occupies 90 pages. When does embryonic transcription replace maternal transcripts in different organisms, and why? (There is a functional relationship with lampbrush chromosomes in oocytes.) How does gene control differ between predominantly regulatory and mosaic development? Where doesn't methylation of DNA occur? How does localization of maternal transcripts in the egg relate to determination of embryonic axes in different groups? And so on. Questions like these deserve more attention by comparative biologists, and this book is a good place to start. -LMV

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**Evolutionary Processes and Theory.**
Edited by **Samuel Karlin and Eviatar Nevo.** 1986. Academic Press. x + 786 pp. Acid-free paper. ISBN 0-12-398760-1 hardbound, $69.00; -398761-X softbound, $34.50.

The contents are much narrower than the title implies. Every paper in the book has a focus on genetics except for a first report on adaptive strategies of Suez Canal migrants and perhaps an evaluation of molecular evidence on ape-human phylogeny. Within the genetical scope the papers are diverse and constitute a good sampling of theoretical approaches, with a moderate amount of laboratory analysis and a nod toward the natural world. Molecular aspects receive appreciable attention. The papers are mostly suitable for journals except for the usual proportion which report on previous work of the author without a real review. Even many of these are useful, though. I must mention Turelli's critique of the normal-distribution assumption of quantitative genetics (from a very different perspective from Thoday's oligogenes, which he doesn't even cite), with a different model which has much less variation maintainable by mutation-selection balance. -LMV

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**Time Frames:** The Evolution of Punctuated Equilibria.
**Niles Eldredge.** 1989 corrected reprint of 1985 book. Princeton Univ. Press. 0 + 240 pp. ISBN 0-691-02435-9. Acid-free paper. Softbound. $8.95.

A nice and very readable exposition of Eldredge's ideas on punctuation, with a reprint of the 1972 Eldredge-Gould paper. The latter didn't originate the theory, common belief to the contrary; in addition to a 1971 paper by Eldredge, a Dutchman (MacGillavry) and, less clearly, a Russian (Ruzhentsev, in a paper translated in the International Geology Review) had given the idea earlier. It seems to be a natural law that only Russians cite Russians, even when they make major advances (Schmalhausen's discovery of *r*- and *K*-selection is another example), and do the Dutch really do science? Stereotypes hide diversity and smother thought. In that vein, don't blame all punctuationists for the excesses of some, and it is still the case that what the world needs is an adequate theory of stasis. -LMV

**On the Economy of Plant Form and Function.**
Edited by **Thomas J. Givnish.** 1986. Cambridge Univ. Press. xvii + 717 pp. ISBN 0-521-26296-8. Hardbound. $84.50.

There ain't no such thing as a free lunch. With a gain comes a cost, although the nature of the cost may vary widely. This book explores the costs and benefits of plant (and some algal) growth from the perspectives of growth form and the physiology of acquisition of resources. There is a little on competition and herbivory, but this is not emphasized despite attention by the editor in introductory sections. His efforts don't lead to a real synthesis; the book is mostly a set of disparate reviews and original studies, more comprehensive than one might expect but still requiring the reader to supply the context. The quality of the papers is unusually good, though, and the combination of theory and the real world in most of them is salutory. One author even includes a useful review of plant form in the fossil record. The publisher should try to use dashes of different lengths. -LMV

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**Coevolution and Systematics.** Systematics Association Special Volume 32.
Edited by **A.R. Stone and D.L. Hawksworth.** 1986 (18 December). Oxford Univ. Press. xi + 147 pp. ISBN 0-19-857703-6. Hardbound. $39.95.

The book is too narrowly focused for its title. All papers but one deal exclusively with Farenholz's Rule, that the phylogenies of parasites and their hosts are congruent because the organisms stay together. (The consensus is that sometimes they do and sometimes they don't, in each group examined. This confirms much work elsewhere.) Only Estop, for aphids, considers why interlineage transfers occur; he finds that plant defenses tend to fall into several classes, convergently evolved, and that aphids adapted to one sort of defense find it easy to transfer to other plants with the same sort. Nobody considers diffuse coevolution at all. (This term has been objected to, by people who seem to have forgotten Mac Arthur's diffuse competition and who can't see the roots of both in everyday language.) Thompson has a reasonably synthetic treatment, noting in particular that interspecific interactions can vary among different environments and giving conditions for some of the different outcomes of host-parasite evolution. -LMV

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**The Nutrition of Herbivores.**
Edited by **J.B. Hacker and J.H. Ternouth.** 1987 (November). Academic Press. xi + 552 pp. + erratum sheet. ISBN 0-12-750052-9. Hardbound. $89.95.

Entomologists and ecologists beware -- The herbivores here are farm livestock. That doesn't make the book irrelevant to ecological or evolutionary matters, because the economic importance of the animals involved has meant that they are intensively studied in ways ordinarily impractical with an agromyzid leaf-miner or even a muntjac. (In fact some wild Australian mammals and even a few birds do get mentioned here and there.) Much of the intelligent work in this area is quite recent, so large gaps are still visible. That they are actually visible is a real conceptual advance. Interaction effects have become known; thus "Ticks depress appetite and reduce feed-conversion efficiency, biting flies appear mainly to divert energy from growth to defensive behaviour while tick-borne protozoan parasites cause losses of nutrients by altering the host's biochemical and physiological functions" (p. 206). The book concludes with an interesting allometric treatment of some aspects of mammalian life histories. -LMV

**The Mammalian Herbivore Stomach.** Comparative Anatomy, Function and Evolution.
**Peter Langer.** 1988 (March). G. Fischer Verlag (distributed in USA by VCH Publishers). xvii + 557 pp. ISBN 0-89574-254-3 in USA. Hardbound. $186.00.

It isn't easy to be a herbivore. Well, one's food is usually easier to catch than is a predator's, but once it's caught it needs to be digested. Plants don't find it good to be eaten and so they defend themselves in various ways, from mechanical to toxic to just being non-nutritious. Mammals need microbial help if they are to digest the celluloses anyway (and what about lignins for browsers?). A lot of work has been done on all this in the last few years, and Langer's book is an interesting complement to a recent one by Demment and Van Soest. The latter emphasizes variation in adaptive strategies among mammalian herbivores, while Langer emphasizes comparative and functional anatomy and, of all things, biogeography. The books do overlap, but not as much as one might expect. Both concentrate on the larger folivores and give some digestive physiology, and Langer does emphasize stomachs over hindguts.

The groups treated in detail all have complex stomachs or stomach-like regions: ruminants, camels, Suiformes, sirenians, colobine monkeys, tree sloths, and kangaroos. Only <u>Bradypus</u> represents the sloths; Langer hasn't heard that <u>Choloepus</u>, whose name he consistently misspells, comes from a different family of "ground" sloths and so would help in inferring the biology of extinct groups. Phylogeny is somewhat of a problem overall in the book, as are related matters. Langer emphasizes two scales of herbivory, one for the food itself and the other for morphological adaptation. The former is unusually detailed and I found it useful, but the latter tries to compress multidimensional variation into a single scale and it works less well.

The basic anatomical information is beautifully and carefully worked out, and there is a surprising amount of information, often original, on quite a diversity of topics. One that surprised me is that more species of large herbivores live in forested areas than in open areas. This is so surprising that I won't really believe it until I check it myself, but it indicates what a mine is available here for diligent readers. And <u>Tarsipes</u>, the nectar-eating honey possum (p. 367, not to be found in the poor index) retains a divided stomach from its herbivorous ancestors but the gross structure has become variable, -LMV

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**Biology of Anaerobic Microorganisms.**
Edited by **Alexander J.B. Zehnder.** 1988 (24 August). Wiley. xii + 872 pp. ISBN 0-471-88226-7. Hardbound. $89.95.

More accurately, metabolism and biogeochemistry of anaerobic bacteria. A few eukaryotes get mentioned in passing in three chapters. Our ancestors were all anaerobic until a few of them killed off most of the rest by oxygen pollution, and our cells still have various ways to inhibit the effects of this poison.

Anaerobes don't necessarily live in what we would consider anaerobic environments, and anaerobic environments can turn up in surprising places. The book doesn't go into such matters, though. Nor does it consider questions like why the anaerobic degradation of "resistant" compounds like lignins occurs so slowly. (Wy doesn't the population of degraders just expand until its substrate is gone, like any well-behaved bacterium does? Maybe all such degraders are regulated by a shortage of some other nutrient. This is a real anomaly, of the same sort as why the world is green, if perhaps more tractable, and it deserves careful investigation.)

What the book gives us is a well-done synthesis of the biogeochemistry of anaerobic bacteria, and aspects of their metabolism which relate to this. The information is hard to come by and I know of no other adequately detailed review.
-LMV

**Evolutionary Physiological Ecology.**
Edited by P. Calow. 1987. (vii) + 239 pp. ISBN 0-521-32058-5. Hardbound. $34.50.

Environmental physiology, it is said, shows that organisms can live where they do live. This isn't a trivial result, if one substitutes "why" for "that", but physiology can contribute more to evolutionary ecology than that general approach. This book is a sampling of some of the better recent work, the papers being more or less original research on topics of some general interest. The organisms range from violets to mammals to Escherichia, and the predominant theme is the nature of specific allocations, of trade-offs, of constraints. Kirkwood regards the evolution of senescence as a matter of how much energy should be used for maintenance and how much for reproduction, there being a threshold above which no senescence occurs (colonoids, and also germ lines and most unicells). If so, our immortality should be feasible in the foreseeable future by changing the allocation, but I suspect that there are other kinds of pleiotropy which will be more awkward to deal with. At least there doesn't seem to be a reason for such genes not to accumulate. McNab relates the origin of mammalian endothermy to the apparent large decrease in body size accompanying the origin of mammals. Unfortunately it isn't yet clear that there really was such a decrease; the relative contributions of phyletic decrease in size, and decrease in maximum or predominant size of contemporaneous relatives (the larger ones being more easily found) can't yet be untangled. But he may be right. -LMV

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**Animal Energetics.**
Edited by T.J. Pandian and F. John Vernberg. 1987 (December). Academic Press. Acidfree paper. Hardbound. $193.00 for both volumes together.
Volume 1, Protozoa through Insecta. xiii + 523 pp. ISBN 0125447914. $110.00 separately.
Volume 2. Bivalvia through Reptilia. xiii + 631 pp. ISBN 0125447922. $110.00 separately.

Mammals and birds aren't deferred; they are omitted because their energetics has been reviewed elsewhere. For that matter, fishes get restricted to topics not adequately covered elsewhere. So. What we do have are fairly detailed surveys of energy budgets and their components, and various related topics which vary among chapters, of the larger groups of animals and protozoans. Most smaller phyla are omitted, as are cephalopods. The syntheses are original and for the most part are the first good reviews for their groups. Even some analyses are original. For instance, bryozoans forage by filtration, and we have here the first estimate of its energetic cost (comparable to total metabolism, they say). Maybe someone will be motivated to estimate it for, say, tunicates and clams, both of which groups have been more extensively studied. At least some homopteran honeydew has two thirds as much amino acids as sugars, a ratio which I found startling. As noted for amphibians, ectotherms can't concentrate nonenergetic nutrients as much as endotherms can because they use much less energy and so eat much less overall; therefore such nutrients rather than energy may more often be limiting for ectotherms. Lots of tidbits like these, in addition to the expected energetic components. An unusually valuable treatise. -LMV

**Temperature Biology of Animals.**
**Andrew R. Cossins and Ken Bowler.** 1987. Chapman and Hall. ix + 339 pp.
ISBN 0-412-15900-7. Hardbound. $57.50.

Adaptive strategies include physiology, and that is the approach taken in this useful book. By now environmental physiologists apparently find it hardly necessary to apologize to their supposedly more hard-nosed colleagues for considering teleology, which in Aristotle's original usage included adaptation as a major aspect. Temperature of course pervades all life; one can't even shut it out by forming a cyst or spore. The book proceeds directly from the thermal environment and biochemical effects to problems of ectotherms and endotherms, adaptations to changed and potentially lethal temperatures, and the dependence of such phenomena as reproduction and growth on temperature. The authors regard deviations from a proportional ($Q_{10}$) response as indicating regulation above the biochemical level. How this may operate for the most extreme case, biological clocks, isn't discussed. The authors hesitantly accept the view, cogently argued some years ago in a book by Hamilton (not cited), that animals try to reach the highest temperature they can which isn't lethal. This viewpoint is important and deserves more discussion than it has had. If it is correct, it provides a potentially unifying theme for much of thermal physiology and even, as Hamilton emphasized, some other biological topics. Of course there is cold too, and the book here does equally well with both. -LMV

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**Kin Recognition in Animals.**
**Edited by David J.C. Fletcher and Charles D. Michener.** 1987 (14 July). Wiley. x + 465 pp. ISBN 0-471-91199-2. Hardbound. $81.95.

Why recognize kin? Because they share our genes (some of them) and so if we act better to them than to others our genes will do better in selection. (But why are tadpoles so good at it?) Recognizing individuals is a further step and has other functions, as in reciprocal altruism. Social interactions depend on distinguishing us from them, and often on degrees of us-ness. The recognition doesn't need to be perfect, but the selectional noise provided by such imperfections means that a larger ratio of benefit to cost will be necessary at each level of relatedness. For instance, kin may just tend to be closer together and no further recognition is available. Because kin selection is automatic, a sacrifice for one's neighbors in such a case will have an expected benefit for one's kin, but the benefit will be diluted by some advantage to nonkin. Major genes are sometimes involved (best studied in plants), but most cases are probably more complicated in ways not yet adequately untangled.

The book is mostly a review of what is known in most groups of animals where recognition has been studied. It has provided a focus for hordes of workers; almost all that is known comes from the last 15 years. One chapter, on an isopod crustacean which is actually dominant in deserts, gives extensive original work; the other chapters, and part of this one, are reviews. One is on humans and refers to work where monozygotic twins cooperated more than dizygotics. It would be instructive to know how dizygotics who think they are monozygotics behave; the class isn't rare, and overinterpretation is easy. -LMV

**Proceedings of the Second International Conference on Quantitative Genetics.**
**Edited by Bruce S. Weir, Eugene J. Eisen, Major M. Goodman, and Gene Namkoong.**
1988. Sinauer. xii + 724 pp. ISBN 0-87893-900-8 hardbound, $60.00; -901-6 softbound, $38.50.

Mendel founded quantitative genetics in the usually overlooked second half of his original paper, but the development of the subject on a Mendelian basis was until recently mostly for agricultural improvement. There were exceptions, such as experimental testing on <u>Drosophila</u> or the increasingly sophisticated (in better work) analyses of human behavioral traits, but its recent burgeoning and influence in evolutionary matters comes largely from work a decade or so ago by Lande. A moderate part of this volume is his intellectual progeny, with the expected amount of parent-offspring conflict.

The volume contains reviews of various subfields and many more restricted papers over the whole domain. Molecular genetics is beginning to have an effect, partly in the predictable way of incorporating new discoveries theoretically and experimentally. The paper of Dean et al., though, wasn't predictable. Microbial populations and communities have long seemed to me to have major potential for imaginative work, but not many people have been willing to bridge the biological cultures in order to do so. Dean et al. give theory and chemostat results for short-term evolution of interacting metabolic pathways in <u>Escherichia</u>. This mechanistically focused approach, like that of Tilman and Kilham in ecology, deserves attention. The edifice of quantitative genetics isn't really shaky, as that of mathematical ecology is, but it does incorporate simplifications which affect extrapolated applications (like that of Arnold in this volume) when not adequately addressed. In this volume Turelli gives a critical discussion of such matters, as does Barker in a more conventional way, and Lande criticizes Turelli's own approach. Of such is progress (sociologists please note). -LMV

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**Genetic Variation and its Maintenance.** Society for the Study of Human Biology, Symposium 27.
**Edited by D.F. Roberts and G.F. De Stefano.** 1986. Cambridge Univ. Press. xi + 286 pp. ISBN 0-521-33257-5. Hardbound. $39.50.

One might expect a book with this title to concentrate on <u>Drosophila</u>, and to that extent it is misleading. The expectation itself is wrong, though. There is more known about quasi-natural variation and its dynamics in our species than in all drosophilas together, although probably less about its maintenance. It gets published in mostly different places, and the scientific cultures differ enough that methods of analysis often differ.

The book itself is a disappointment -- I don't know how accurately it reflects the state of the more original work on this species, but the papers here are almost uniformly humdrum. Even one on the variation in red-green colorblindness among more or less aboriginal populations discusses kinds of colorblindness and doesn't mention Reed's hypothesis on selection relaxation in civilization. Mostly the papers deal with ways to estimate genetic relatedness among different populations, and there is early work on mitochondrial DNA here. One paper notes that poor women in tropical areas can often raise children under physiological stress which would preclude that result among the affluently raised, but then muddies the water by proposing naive group selection. Another paper finds the allele(s) for sickle-cell hemoglobin to have originated independently in different regions, as has been known for the more heterogeneous thalassemias. That's about it, though. -LMV

**Population Genetics and Evolution.**
Edited by **Gerdina de Jong**. 1988. Springer-Verlag. xi + 282 pp.
ISBN 0-387-18452-X. Hardbound. $89.50.

Is it because Dobzhansky lived in the United States that Americans even now tend to treat population genetics as the single basic field of evolutionary biology, maybe with a dollop of behavior thrown in? That this is less so in Europe is well indicated by the papers here. Even though the book focuses on evolutionary genetics, it mostly maintains an awareness of, and even occasionally an emphasis on, other aspects. The papers are rather diverse for a book with the title it has, with a fashionable (and appropriate) emphasis on evolutionary aspects of quantitative genetics. Most papers summarize work which has appeared in more detail elsewhere, and these research programs are well enough selected that the book gives a valuable overview. De Jong notes that one paper, by Templeton and Johnston on the abnormal abdomen polymorphism in Drosophila mercatorum, gives a realistic model for a hopeful monster. It marches with earlier theory and examples, showing that the phenomenon shouldn't be ignored even though most known cases are, at least developmentally, disruptions rather than novelties. -LMV

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**Avian Genetics**: A Population and Ecological Approach.
Edited by **F. Cooke and P.A. Buckley**. 1987 (December). Academic Press. xvi + 488 pp. ISBN 0-12-187570-9 hardbound, $86.00; -187571-7 softbound (1989), $35.00.

There's nothing surprising in this book, but rather there are mostly solid and often critical reviews of most of the studied aspects of the evolutionary genetics of birds. Many birds are easier to study in natural populations than are most other animals, and with the prevalence of bird-watchers this has led to an accumulation of a better overall understanding of their ecology than we have for any other group. That's hardly true for their genetics, but the book shows that there is even now a reasonable base, and practical problems are less than for most groups. Topics range from isozymes to speciation, and most (including these two) can be studied in an integrated way. There are also reviews of the current state of work on four long-studied species, and these, with Buckley's concluding synthesis, are among the book's highlights. Not everyone, though, realizes even yet that the amount of gene migration necessary to effectively transfer genes between populations varies over orders of magnitude depending on whether a gene is disadvantageous, neutral, or advantageous in the recipient population. -LMV

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**Textbook of Human Genetics**. Edition 3.
**Max Levitan**. 1988 (11 February). Oxford Univ. Press. ix + 475 pp. Acid-free paper. ISBN 0-19-504935-7. Hardbound. $37.50.

From the magnum opus of the subject to a fairly standard text now. It isn't common that a text gets shorter from one edition to the next, much less cut to a half. A fifth of the loss is from the once valuable bibliography, but a lot more went too. I suppose it had to. There is some expansion in molecular aspects and in polygenic inheritance, and (of course?) there is a fairly good updating. Topics related to pedigree analysis still dominate the book, as is appropriate for understanding the subject. The book remains the most useful one on the subject I have seen except for pure clinical interest, and even clinicians ought to know some science. -LMV

**Plant Reproductive Ecology: Patterns and Strategies.**
Edited by **Jon Lovett Doust and Lesley Lovett Doust.** 1988 (7 July). Oxford Univ. Press. xiii + 344 pp. Acid-free paper. ISBN 0-19-505175-0. Hardbound. $49.95.

Sociobotany (as the editors sometimes call it)? There is a subject sometimes called plant sociology, which studies the patterns and causes of co-occurrence of different species, but interspecific interactions among plants are conspicuous by their omission from this book. That's because the book isn't really about reproductive ecology as such, which occurs in communities, but about the short-term evolutionary strategies of plant genes. Inclusive fitness is therefore a central motif, one that has only recently managed to invade botany. The soil is fertile there, and a ground-up approach like this has good branching potential. In a striking chapter, Haig and Westoby apply the approach to angiosperm seeds, most of which have no less that four genotypically different components. This gives added subtlety to parent-offspring conflict both currently and with respect to the origin of the phenomenon (apparently in the Gnetales, as more recently discovered); in other gymnosperms the nutritive tissue is from the female gametophyte. In apomicts a parent-offspring conflict should be absent, and apomictic dandelions have evolved enough that embryos get food directly from maternal tissue rather than via endosperm as in even their sexual congeners. Other chapters discuss topics like sex determination, pollination, and crowding from a broadly similar perspective. Because all this is about angiosperms, it's good to have a chapter apiece for macroscopic marine algae, bryophytes, and pteridophytes. The one on the algae is a notable first attempt to make adaptive sense out of the great heterogeneity found there. It, like some other chapters, could have benefitted from the greater attention to phylogeny which Mishler gives to bryophytes; this can be done without his cladistic dogma.

An attempt like this book's to turn ecology into population genetics is bound to ignore a diverse set of more or less basic questions and phenomena. Even within genetics it doesn't get to matters like the value of persistence vs. reproduction in different circumstances, the diverse time scales of natural selection, or (except for somatic mutations) selection within individuals, such as expansion of DNA families or self-pruning, which happens to roots as well as branches. We still have pleas for studies of heritability despite the well-known conclusion that effectively all variation among genets is to some extent heritable, which is all that is needed for most purposes. What the book does is nevertheless valuable and should increase the diversity, or at least its equitability component, of adaptive strategies among botanists. Perhaps fungal and animal somatogens, the study of which has depended on advances in botany about as much as botany has depended on gonogenic zoology, will be next. -LMV

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**Human Polymorphic Genes.** World Distribution.
**Arun K. Roychoudhury and Masatoshi Nei.** 1988 (26 May). Oxford Univ. Press. xi + 393 pp. Acid-free paper. ISBN 0-19-505123-8. Hardbound. $56.00.

Don't underestimate the value of compilations like this. Their very availability can suggest new problems, and gathering the data is no trivial task. There are data here for about 360 chromosomal genes, of which about 140 are monomorphic in all populations, for each of about 180 populations for which there are data. Compilation is through early 1987. Some care was taken with respect to accuracy and geographic representation, but the frequencies are given to three decimal places even where the sample size is less than 50. There are even world maps for frequencies of all the nontrivial distributions. The book supplements earlier works by others and is overall the most valuable for studies on human evolution. -LMV

**Ecological Aspects of Social Evolution**: Birds and Mammals.
Edited by **Daniel I. Rubenstein** and **Richard W. Wrangham**. 1986. Princeton University Press. x + 551 pp. ISBN 0-691-08439-4 hardbound, $65.00; -08440-8 softbound, $23.50.

Ecology is such a general causal matrix for the evolutionary play that, although present and relevant, it is not exactly illuminating about any particular case. There are too many 'players' acting against the same background in different ways while others may act elsewhere in the same way for simple generalizations to emerge. One needs a history as well as ecology to understand current social behavior. This collection of papers is unusual in that they were solicited to test a particular hypothesis about the evolutionary causation of social behavior: that ecology largely explains the differences in behavior over time and space for a species or closely related groups of species. The papers are limited to the grouping and mating patterns of well-studied birds and mammals to keep the test both diverse and yet phylogenetically restricted. The choice is obvious since birds and mammals show more frequently than other vertebrates long-term social relationships based on individual recognition, a result no doubt of their greater cognitive abilities. The goal of the book might have been better achieved by a focus on one or the other of the two classes of vertebrates, yet their joint consideration does underline the fact that ecology seems to explain much of their evolution of social behavior at and near the species level.

Although each of the 18 case studies (which have no taxonomic overlap; 7 bird, 11 mammal) is typically of high quality, they produce jointly no simple predictive model of social behavior beyond the fact that ecological factors such as predation or food availability do explain some of the variation. Much variation still eludes any causal explanation other than that of history. When ecological factors can lead to the choice of more than one path and when more than one such choice has been made sequentially by a population, the causal mechanism for the presently observed behavior may be irretrievable. Although a general predictive socioecological theory may be impossible, the ecological influence on social behavior, an influence only reliably seen in long-term studies of populations or particular social groups, is still essential to our understanding of social evolution. One may think that the many years of study represented in these papers may have given some firm conclusions, but they really are only a beginning. They tend to raise more questions than they answer, largely because they have gathered together more facts. Useful speculation relies on facts and generates specific quests for more. All of the taxa covered in this volume can continue to be studied profitably for many more years ahead. In fact, the species with the longest record of study, our own, is as far from being understood in terms of its social evolution as any of the others. -vcm

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**Animal Behavior**: A Concise Introduction.
**Mark Ridley**. 1986. Blackwell Scientific Publications. vi + 210 pp. ISBN 0-632-01416-4. Softbound. $19.95.

A brief overview of animal behavior with some unusual and appealing aspects, such as using the building of spider orb webs to introduce the topic of the machinery of behavior. The author attempts an "essay in introduction," which entails explaining necessary background as required to understand a particular behavior. He does not clutter with encyclopedic surveys of examples, but chooses those that illustrate a point particularly well. The choice is sometimes fresh, sometimes stale, but taxonomically broad. Ignorant readers, for whom the book is written, will get an enticing entry into the field and not a surfeit of examples or controversial abstract theory to turn their appetite away from trying the further readings suggested at the end of each chapter. -vcm

**Primate Social Systems.**
**Robin I. Dunbar.** 1988. Cornell University Press. vii + 373 pp. ISBN 0-8014-2087-3 hardbound, $49.50; -9412-5 softbound, $24.95.

Although Dunbar focuses on primate social systems as examples, he really attempts a treatise about vertebrate (and possibly invertebrate) social systems in general. He never gets around to telling us what is so special about primate social systems that he should limit his book to them. One does not even get a special feel for this order of mammals and much is lost if the reader is not in fact well acquainted with their natural history. Dunbar could well be writing about mammalian carnivorans. He even fails to note that Primates is the only speciose mammalian order that contains almost only social species.

The treatment ranges from expository with little criticism to detailed quantitative models of dubious value. Dunbar uses optimization models derived from demographic features of primates with an imput of environment. Quantitative models with questionable imput are seen as superior to qualitative ones which result in merely directional predictions, because the latter do not rigorously expose the shortcomings of the hypothesis. Any good computer modeller can build anything one desires with a given input of data. I fail to be convinced that computers are superior to the human brain; they only confirm what we see in our minds. Thus, I am not convinced that he has provided much new insight into primate social systems. In the final chapter he does attempt an evolutionary approach to differences in a few primates but some of his basic assumptions are questionable and the results can be viewed as one of several plausible alternatives.

Despite these objections, Dunbar has brought current quantitative sociobiology to the large majority that study primates (including humans) without much theoretical background. It's easier to learn such theory if the examples come from the group one studies. -vcm

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**Evolution of Animal Behavior**: Paleontological and Field Approaches
Edited by **Matthew A. Nitecki** and **Jennifer A. Kitchell**. 1986 (14 August). Oxford Univ. Press. (vi) + 184 pp. ISBN 0-19-504006-6. $39.95.

Paleontological approaches? Well, yes. There are several sorts of information in the fossil record which permit reasonable inferences on behavior, and as with morphological features the time dimension is, in better cases, of great value in inferring the actual pattern of evolution. Take trace fossils, which are direct records of animals' movements even if we don't always know which animals made them or just why. Seilacher gives a clear review of the rather remarkable evolution of pattern and of functional behavior programs as recorded in marine trace fossils. Or take predation. Naticid snails leave characteristic holes when the successfully or unsuccessfully attack potential prey, and Kitchell's original and innovative study of them finds, e.g., long-term stasis in both adaptive and inadaptive aspects of the behavior. Ostrom synthesizes various sorts of evidence on social and other behavior of dinosaurs.

The rest of the book is more conventional, except perhaps for Lauder's analysis of evidence bearing on behavioral homologies, especially of centrarchid fishes. Apparently there has been a view, which he adequately disposes of, that behavioral homologies depend on homologous neural substrates. This resembles the better-known fallacy of requiring homologous genes in order for morphological characters to be homologous; of course continuity of information, and thus homology, can occur at any such level. There are also competent reviews, by the expected people, on sexual selection by scorpionflies, helpers of mother jays, and baboon parenting. -LMV

**Despotism and Differential Reproduction: A Darwinian View of History.**
**Laura L. Betzig**. 1986. Aldine. xi + 171 pp. ISBN 0-202-01171-2. Hardbound. $24.95.

The thesis of this book is that "despotism, defined as an exercised right to murder arbitrarily and with impunity," occurs because despots are therefore able to breed more. The evidence is a positive correlation (especially among cultures) of despotism with polygyny. The correlation can be accepted, even if some of the data are weak, but the thesis is hardly proved thereby. One of several related alternative hypotheses, none of which are even mentioned, is that power permits gratification of desires, and that such desires commonly include, for different reasons, both polygyny and despotism. The book has interesting examples but the conclusion, even if true, is for now just wishful thinking. -LMV

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**Human Reproductive Strategies: A Darwinian Perspective.**
Edited by **Laura Betzig, Monique Borgerhoff Mulder, and Paul Turke**. 1988. Cambridge Unversity Press. vii + 363 pp. ISBN 0-521-32738-5 hardbound, $75.00; -33796-8 softbound, $24.95.

This may, with luck, be a pivotal book in linking the sociobiological and anthropological approach to human reproductive behavior. I am not sure that the sociobiologists have convincingly won their battle, although the quantitatively well-documented case histories given here are initially impressive. Numbers can impress and statistics confuse the basic issues. Correlations are not causes, and statistical confirmation of a hypothesis does not in itself mean that the hypothesis is supported. Other plausible hypothetical paths to an observed correlation may exist, and they are rarely treated here. The volume does, however, provide an amazing number of well-supported correlations that compel further thought. They are not to be lightly dismissed, but their correspondence to predictions should be viewed warily. Kin selection is not the only way to interpret patterns of mating and parenting in humans.

The 17 case histories of mating or parental behavior provide good coverage of the globe and for this reason alone the volume is an excellent introduction into the diversity of human reproductive behavior. The three 'critical summary' papers by Dunbar (on mating), Irons (on parenting), and Alexander (on future prospects for a Darwinian approach to human behavior) are provocative but will undoubtedly give rise to dissenting views. The concentration of detailed observation, terse interpretation, and challenging theorizing make this edited collection somewhat special and one to read carefully in its entirety. -vcm

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**Biodiversity.**
Edited by **E.O. Wilson**. 1988. National Academy Press. xiii + 521 pp. ISBN 0-309-03783-2 hardbound, $32.50; -03739-5 softbound, $19.50.

It is an unusual experience to find a biologically sophisticated book which is accessible to nonbiologists. There are even some original tidbits here and there. The individual chapters are mostly well thought out and present somewhat different viewpoints, although all agree that preservation is desirable. The book is well organized and covers a wide range of topics, mostly in bite-sized chunks of less than ten pages. Nevertheless, the impact of human population growth, undoubtedly the overriding aspect of the current mass extinction, has no separate section or even chapter, although some authors do comment on it in addition to their presumably assigned topics. Was this omission a political ploy to make the subject more palatable to uninformed growthmongers?

I can't summarize the book; you need it. -LMV

**Novel Aspects of Insect-Plant Interactions.**
Edited by Pedro Barbosa and Deborah K. Letourneau. 1988. Wiley. xvii + 362 pp.
ISBN 0-471-83276-6. Hardbound. $45.00.

What is supposed to be "novel" here is emphasis throughout on interactions among members of three trophic levels, primarily on how plant chemical defenses affect predation on herbivores. The existence of such effects is now well known, and interactions of members of the three trophic levels have been studied for decades (e.g., ant-acacia interactions). It is less well known, however, that microorganisms carried by insects (and other organisms), in some cases within their cells, can also affect herbivory and predation in various ways, negatively as well as positively. The microorganisms of course have potentially more rapid evolution than their larger consorts, but it is unclear whether this is actually important.

The recent attention to such realistic complexity gives a twist to "controlled experiments." One is interested in the effect of A on B and tries to control extraneous effects such as C. Unfortunately, the level at which C is controlled can affect the amount or direction of the process ostensibly being studied. Probably anyone who has thought about the analysis of variance realizes this, but it hasn't carried over enough into the design of real experiments, artificial or natural.

The reviews in the book are good to outstanding, and there are even some original results as well as original thoughts. However, Barbosa thinks it surprising that generalists are usually more susceptible to plant toxins than are specialists. I would be shocked if they weren't -- toxins tend to be plant-specific and specialists can more easily evolve resistance. We still don't know why the world is green, and the book doesn't address this question. The induction of defenses, e.g., is claimed elsewhere to be costly and therefore often avoided. The solution may come, though, from studies comparable to those reviewed here. -LMV

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**The Tropical Rain Forest. A First Encounter.**
Marius Jacobs. 1988. Springer-Verlag. xvi + 295 pp. ISBN 0-387-17996-8.
Softbound. $39.95.

A first encounter, and nearly the last? The focus of this book is on understanding the nature of our folly here. Partly this is implicit in accounts of the biology, but it is also explicit here and there in these accounts as well as constituting the last third of the book. This is a view partly from the inside and not hiding warts, although it lacks the perspective of the tropical poor and has only passing mention of the exponential steamroller of population increase. Natural reserves are advocated as the only possible hope, but how then do they remain free of the burgeoning hungry?

Animals are almost as inconspicuous in the book as their proportion of rain-forest biomass. The book is about trees, with a nod to other plants; animals come in if they relate to the plants. Diverse aspects are covered at a level probably not quite intelligible to an intelligent neophyte (seedling?). The coverage is worldwide and geographical as well as topical, with some Malaysian emphasis, and the treatment is sound. Any tropical book has some tidbits which will be new to most ecologists; one for me is a species of durian tree which flowers at the base of its trunk, where the fruits are dispersed by turtles. Several people contributed to the book after the author's death. The updated translation from Dutch is almost always felicitous, although language may have kept some misprints uncorrected, as interchanging families and genera in a table. -LMV

**Phytoplankton Ecology.** Structure, Function and Fluctuation.
**Graham P. Harris.** 1986. Chapman and Hall. xi + 384 pp. ISBN 0-412-30690-5. Softbound. $37.50.

In ecology as in human affairs, the large look on the small as averages, and this may obscure what is really happening. A central theme of Harris's book is the differences between our world and that of the phytoplankton, over orders of magnitude, in spatial scale and in rates. As is well known to those who know such things, succession occurs not over decades but over weeks. A dying cell (and, as not mentioned, often a living one) releases nutrients which may mostly be taken up by another cell before being homogenized into the water mass, although here the physics of turbulence may preempt the biology. There are many phenomena like these, and they help make the world of the small both quite different from ours and a promising place to study phenomena of general as well as of specific interest. The book gives a stimulating account of such matters.

There is a second theme to the book, also well presented but to me less convincing. Harris thinks that nonequilibrial phenomena predominate for the phytoplankton. Yes, things happen rapidly; yes, nutrients often seem in excess; yes, there seem to our eyes to be too many species doing too much the same thing; yes, equilibria have been reified where they don't exist. But take, for example, his advocacy of phytoplankton growth occurring at or near maximum rates in oligotrophic waters, just with different species from a eutrophic broth. A large fraction of scarce nutrients are in the algae themselves and, over a suitable interval, cell death must approximately equal cell division. This is itself an equilibrium, with a level set by total limiting nutrients. One asks then why the death rate should be so high as to give survivors maximum growth rate. The absence of a plausible feedback in this direction makes the phenomenon itself suspect, especially because of problems with the data. And phenomena which are nonequilibrial at a very local spatiotemporal scale often turn out to be more or less equilibrial at a larger scale: oddly, this aspect of scale gets lost in the book. Environmental variation in both space and time does promote diversity, but only when there are suitable relations between growth rate and density, as Chesson has shown. Otherwise one invokes magic. -LMV

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**Plant Ecology.**
Edited by **Michael J. Crawley.** 1986. Blackwell Scientific, now at 3 Cambridge Center, Cambridge, Mass. 02142. xiii + 496 pp. ISBN 0-632-01666-3 hardbound, $78.50; -01363-X softbound, $39.50.

Move over, Harper. This book isn't an updated Population Biology of Plants, but it's more or less as good and owes a lot to Harper's redirection of thought. The emphasis is dynamic throughout; there is no phytosociology or the like. Crawley seems to think that such approaches are outmoded and useless, but a more charitable view to take would be that they don't particularly contribute to what he wants to know. The dynamics is centered at the population level; it goes down to physiology and growth and up to communities. The systems approach is ignored, and for all the authors plants are entirely or almost so equated with angiosperms. Such limits don't really detract from what the book does, although its organization precludes integrated accounts of topics like herbivory.

What we have are thoughtful and integrated treatments with enough examples to lend some verisimilitude. There are two pieces of original work: by Hubbell and Foster on regeneration in gaps in Panamanian rain forest, and by Gill on the significance of genetic variation within individual plants. The more conventionally reviewish chapters often also have insights new at least to me. First-rate. -LMV

**Bacteria in their Natural Environments.** Society of General Microbiology, Special Publication 16.
Edited by **Madilyn Fletcher** and **George D. Floodgate**. 1985 (December). Academic Press. x + 196 pp. ISBN 0-12-260560-8 hardbound, acid-free paper, $69.50; -260561-6 softbound, $24.95.

A rather large subject. What the six reviews actually deal with are adaptations to scarce and to superabundant food, to oligotrophy and copiotrophy in recent useful terminology. For instance, many planktonic marine bacteria break up into tiny fragments when starvation sets in, and the fragments have a lower, not a higher, metabolic rate. Their improved surface-to-volume ratio lets them better compete for what nutrients there are. Possible constraints on the DNA synthesis obviously needed initially aren't dealt with. The soil is a difficult environment to study, and although it appears clear that overall bacterial growth is energy-regulated it is difficult to evaluate the relative importance of feast-and-famine copiotrophs, which are relatively easy to deal with, and quasi-equilibrial oligotrophs, which aren't. (Of course these are extremes on a continuum, but even the distribution of real bacteria on the continuum is poorly known.) These are samples of the sorts of ecologically interesting phenomena which can be found in the microbial world. The book contains others and some theory too. The general subject deserves more attention than it gets. -LMV

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**The Microbiology of Terrestrial Ecosystems.**
**B.N. Richards**. 1987 (22 May). Longman (USA distributor: Wiley). xvi + 399 pp. ISBN 0-582-45022-5 (0-470-20706-X in USA). Softbound. $47.95.

Substitute "soil" for "terrestrial" in the title -- somebody goofed. The book itself is unusually worthwhile, though. Richards writes clearly and economically about matters to which he has given some thought, and his thoughts have resulted in insights which a less synthetic treatment would overlook. He emphasizes the unity and functioning of the soil ecosystem without loss of attention to the organisms and interactions which make it up. In fact it is these interactions which are the focus of most of the book, interactions of organisms with each other and with nutrient cycling. The latter is treated from the organisms' perspective. He is less sure at the population level, which is only touched on. The figures are quite helpful. One page in the review copy was crumpled into partial illegibility. -LMV

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**Microbial Ecology**: Organisms, Habitats, Activities.
**Heinz Stolp**. 1988. Cambridge Univ. Press. xiv + 308 pp. ISBN 0-521-25657-7 hardbound, $54.50; -27636-5 softbound, $22.50.

Because the author can't assume that his readers are familiar with microorganisms, the book is almost as much a survey of microbiology as of microbial ecology in particular. As this sort of book, it's OK, not more. It pays little attention to eukaryotic microorganisms or viruses and little or none to predation, to chemostats (laboratory ecology, unusually important in this area), to results of competition, to genetical and evolutionary ecology, or to what bacterial species are. The treatment of the kinds of microorganisms, even bacteria, is somewhat outdated. "Producers of highly potent antibiotics obviously have no selective advantage in their natural environment" (p. 188)! Nevertheless there is a lot here, and the treatment is good if rather formal. The emphasis throughout is on adaptations of different bacteria to their highly various ways of life, and there are lots of references for an introductory book. (Oddly, no mention of Koch.) The book may actually be the best easy introduction to its real subject. -LMV

**Nutritional Ecology of Insects, Mites, Spiders, and Related Invertebrates.**
Edited by **Frank Slansky, Jr.**, and **J.G. Rodriguez**. 1987 (9 March). Wiley.
xvi + 1016 pp. ISBN 0-471-80617-X. Hardbound. $105.00.

It was 26 years from Brues's classic Insect Dietary (later reprinted as Insects, Food, and Ecology) to the papers in Insect and Mite Nutrition (Rodriguez, ed.) So another 15 years to the present book is reasonable. But will the next one be a five-volume treatise?

The book is a remarkable achievement. The animals are divided up by general adaptive zone for treatment; of large groups only centipedes and scorpions seem to escape the net entirely, although some subsidiary nutritional modes of groups otherwise covered are also absent, as are some smaller taxa.

Ecologists are likely to pass over this book because its title sounds like backwater entomology, which is a shame. Its focus isn't who eats what when, although there are data here and cited elsewhere which give an entry to such information for some groups. (There are major compilations, though, e.g. for the Tachinidae, which lack even a reference. Interested outsiders do need a way to track down such information.)

A substantial part of ecology deals in some way with food, and that is what the book is mostly about. It defies summary. The quality of the papers shows the expected variation, and different authors treat different aspects of the subject depending (sometimes) on what aspects their group can best illuminate. The reviews are mostly of real ecology, of diverse sorts, and those ecologists who do discover the book will find much of interest. -LMV

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**Colonization, Succession, and Stability.** 26th Symposium of the British Ecological Society.
Edited by **A.J. Gray, M.J. Crawley**, and **P.J. Edwards**. 1987. Blackwell Scientific.
xi + 482 pp. ISBN 0-632-01631-0. Hardbound. $79.50.

We have here a distillation of the state of the successional art, at least as seen by the organizers. There aren't any surprises, but every paper is useful in a conceptual way as well as often in other ways, and there is little overlap although differences of opinion do exist. Primary and secondary succession are quite distinct processes; perhaps the first should be called "primary colonization" to avoid other, too closely related, meanings of "colonization". It is clear that several sorts of causal processes occur in (secondary) succession, but their relative importance isn't clear, and it isn't clear even how to (fuzzily) classify components of successions so as to form causally more or less homogeneous clusters. There can be distinctions made which lack a functional difference, such as Connell's "alternative" hypotheses for the evolutionary origin of fugitive species, the hypotheses differing only as to the earlier presence of competitors to which the species aren't adapted anyway. I did miss an explicit discussion of levels of competition, although several authors consider their results. As Williamson notes, "In the long run no community has been stable to evolutionary change." Succession thus grades into community evolution, and the ways in which communities evolve is a subject which still lacks a coherent conceptual framework. The framework more or less in place for succession may provide one place to look, although it is remarkable that an entire book on succession has not a single paper from the approach of systems ecology. There have been major contributions from this perspective, which is unfortunately more or less isolated from the rest of biology. More interaction would help in both directions. -LMV

**Ecology of Tropical Oceans.**
**Alan R. Longhurst** and **Daniel Pauly**. 1987 (November). Academic Press. xiii + 407 pp. Acid-free paper. ISBN 0-12-455562-4. Hardbound. $39.95.

Except for coral reefs and to some extent mangrove swamps, tropical seas have been surprisingly neglected in comparison to cooler waters. The authors of this book are ichthyologists, and while not surprisingly they emphasize fish, especially those of fisheries, they also treat the underlying ecosystem at length. This is done in a rather comprehensive way, with consideration to physical and geographic aspects as well as to the organisms and their interactions. As the authors note, only the near-surface water is warm, and they thereafter ignore the deep (and midwater) except for upwelling. Even here the causes of nutrient regeneration aren't considered. I found only a few dubious interpretations, such as explaining high fecundity (of individuals) as an adaptation (of a population) for exploiting small food via cannibalism of the young. There is some consideration of specific taxa, even of some invertebrates, but most of the ecology is of ecosystems, communities, and populations. The result is an informative and intelligent treatment limited mainly by the relative lack of information yet.

One chapter gives a new and intriguing theory of fish growth, not something one would expect to find in a book with this one's title. It proposes that fish growth and adult size are ordinarily limited by oxygen supply to the tissues. (A related proposal, that mortality rate depends on growth rate, seems to put the propellor on the prow.) Because of the apparently high oxygen use of endothermic tuna, which is not discussed, a nonspecialist like myself may remain unconvinced that the constraint is critical. Maturation time may be related to "food-conversion efficiency", or metabolism above maintenance, for which some evidence is also given, but how this relates to adaptive strategies is then unclear. -LMV

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**Continental Shelves.** (Ecosystems of the World, vol. 27.)
Edited by **Hendrik Postma** and **Jenne J. Zijlstra**. 1988 (April). Elsevier. x + 421 pp. ISBN 0-444-42609-4. Hardbound. $189.50.

Shelves are the sites of upwelling, when it occurs, so the most productive parts of the ocean, aside from reefs, are on shelves. ((Some regions lack appreciable upwelling.) Upwelling is important because nutrients escape from the pelagic ecosystem whenever material falls toward the bottom to feed the derivatory ecosystem there. There's light enough near the surface, except in polar winters, and not much can be done about the loss downward, so productivity depends on availability of nutrient input (especially phosphorus, but it doesn't come by itself) back from the deep and from rivers.

The book is well organized, marching from overviews of physics and chemistry (in relation to shelf biology) and geology (for its own sake) through chapters on the dynamic ecology of plankton, benthos, and fish, to a partly simplified account of energy flow, and to syntheses of four shelf areas. All the four are broad shelves, and the one tropical account is perhaps useful mainly for graphically illustrating the abysmal state of knowledge there in a subject dominated by temperate-zone biologists with local interests. A northern concentration doesn't mean, though, that the relevant processes are close to adequately known, even in the North Sea. Populations of shelf organisms are notorious for their variability, and our understanding of these changes in the pelagic realm is as notorious for its unreliability. The chapters which deal with such matters, in their different ways, do reasonably expose the nature of the difficulties. If the nature of the problem were adequately known it would probably be solved, even if deterministic chaos would enter, but the book does more or less show where we are now. -LMV

**Fungal Infection of Plants**. British Mycological Society, Symposium 13. Edited by **G.F. Pegg** and **Peter G. Ayres**. 1988. Cambridge Univ. Press. xiii + 428 pp. ISBN 0-521-32457-2. Hardbound. $89.50.

Diverse fungi infect various parts of perhaps all plants under one sort of circumstance or another. Some are pathogenic, some (as some infections of the outside of leaves) are more or less commensal, and mycorrhizae are essential to the plant's well-being. Most of the book deals with physiological aspects of these relationships, competently but without much new light. De Wit, though, considers the cell physiology of the common gene-for-gene arms race of resistance and infection, and Webber et al. show that the infection of Dutch elm disease involves a whole evolutionary cycle in each tree, with repeated competitive replacement of genotypes and occupation of different niches, in part sequentially. The process somewhat resembles the maturation of cancers in animals.

Roots are notoriously understudied, despite most of a plant's photosynthate usually going to them and their associated mycorrhizae. Deacon, in a fascinating paper, discusses the frequent programmed death of cells in roots behind the growing tip, in a repeatable pattern. Root hairs are thus very ephemeral, and the dead cells in the cortex provide relatively easy access to invading fungi. It's not good for mycorrhizae, though, which need living cells. The region around the root becomes enriched in compounds from the root, thereby providing a medium for bacterial growth. Might there even be indirect coevolution here? The whole phenomenon deserves imaginative study. -LMV

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**Tropical Ecology and Physical Edaphology**. **R. Lal**. 1987 (13 January). Wiley. xii + 732 pp. ISBN 0-471-90815-0. Hardbound. $197.00.

It's odd how macroscopic soil ecology lives in its own world, having little effect on other areas and accepting little influence in return. (Soil microbiology is yet a different world and is barely mentioned in Lal's book.) Soil communities rival even aquatic ones in ease of experimentation, better in some ways and worse in others, but hardly anyone thinks of them this way. What links soil ecologists have are mostly with agriculture, and it may be difficult to escape that perspective once one is buried in it. Work with practical application isn't something to look down one's nose at, as is often done reflexively. The problem is that concentration on the practical tends to stifle imagination, even though imaginative work without a practical focus is often eventually more valuable practically as well as scientifically. And soil ecology lacks imagination.

That's the problem with this book. It gives a great amount of information on tropical soils and the descriptive ecology of their inhabitants, with special reference to Africa. There's almost a hundred pages on termites alone, although almost nothing on roots. But what does it all mean? Surely more than the applications to agriculture and forestry which the author goes into. The subject needs its Mac Arthur; Ghilarov (hardly mentioned in this book) made a good start for those who can read Russian, and his recent death leaves a real gap. -LMV

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**Hierarchy**: Perspectives for Ecological Complexity. **T.F.H. Allen** and **Thomas B. Starr**. 1988 paperbound reprint of 1982 book. Univ. Chicago Press. xvi + 310 pp. ISBN 0-226-01432-0. $12.95.

This conceptually useful if somewhat overdrawn book looks at ecology from the perspective of interacting causal hierarchies. The perspective emphasizes effects of scale, and scales come in various forms. Mostly, though, scales aren't hierarchical, so the book has a metaphorical aspect which interferes with its message. -LMV

**Insect Outbreaks.**
Edited by **Pedro Barbosa** and **Jack C. Schultz**. 1987 (December). Academic Press. xiv + 578 pp. Acid-free paper. ISBN 0-12-078148-4. Hardbound. $75.00.

This book will be of interest to many people besides entomologists, if they ever manage to look past the title. Insects which have outbreaks are folivores, mostly feeders on trees. (Grasshoppers are, oddly, hardly mentioned by anybody in the book.) So we get at once into the Abominable Mystery of ecology.

Why (more precisely, how) is the world green? Do you think you know the answer? Most informed people do think so, but it's interesting how diverse their answers can be. The book is a treatise on this subject, although because of its official focus on outbreaks some aspects are downplayed. Nevertheless, there are diverse viewpoints and backgrounds represented, and a surprising number of the chapters have something scientifically useful to say. An unusually valuable book.

-LMV

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**General Parasitology.** Edition 2.
**Thomas C. Cheng**. 1986. Academic Press. xix + 827 pp. ISBN 0-12-170755-5. Hardbound. $65.00.

This massive book treats animals (and "protozoans") which are parasitic on other animals. Plants, bacteria, viruses, and fungi are ignored, as both hosts and parasites, and parasitoids get a rather perfunctory treatment. Brood parasitism by some birds, and analogous behavior by some bees (and slave-making by some ants) are apparently forgotten, but otherwise the treatment is as comprehensive as one could reasonably expect. After three general chapters, including one on comparative immunology, the treatment is by groups of parasites. Clinical aspects are unusually sparse, and hosts belonging to all animal phyla are considered. The phylogenetic background is weak and outdated, but otherwise the book is a mine of information. It does have some linguistic and taxonomic infelicities (e.g., "comprised of", "Superfamily Brachycera"), and an outline historical survey manages to get the titles of both the books it mentions (by Lamarck and Darwin) wrong, but such things don't really detract from the real strength of the book. LMV

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**Seed Dispersal.**
Edited by **David R. Murray**. 1987 (January; stated 1986 in book). Academic Press. xiv + 322 pp. + erratum sheet for first chapter. ISBN 0-12-511900-3. Hardbound. $49.95.

This is a book about how seeds disperse, not (except in a chapter by Howe, and peripherally elsewhere) about why they should do so, or how much would be best under such-and-such conditions, or other strategic matters. The coverage is far from comprehensive, but what is here gives a good sampling of what is known, with some original work included. The book includes treatments of aerodynamic aspects; dispersal by water and by the guts of birds and mammals; dispersal into burned areas; Australian _Acacias_; and, interestingly, a paleontological account of the evolution of dispersal since the origin of seeds, which concludes that "the present relative complexity of dispersal syndromes" is a result of the mostly Cenozoic evolution of mammals and birds. It seems to have started near the end of the Cretaceous, though, when such herbivores were small. Maybe the evolving dinosaurs did help?

-LMV

**Multivariate Analysis of Ecological Communities.**
**P.G.N. Digby** and **R.A. Kempton.** 1987. Chapman and Hall. viii + 206 pp.
ISBN 0-412-24640 hardbound, $45.00; -24650-3 softbound, $19.95.

There are diverse sorts of multivariate data in ecology, even in community ecology, and diverse applications of them. I therefore find it odd that the several books on the subject focus on ordination of species occurrences or treatments. One might, say, also want to find pattern in the life-history attributes of members of a guild, look at their density fluctuations in relation to a set of possible causes, or deal with structured data like food webs, where the directed graph of connections can also be expressed (opaquely for immediate insight but not necessarily so for calculation) as an asymmetric matrix. (How much do properties of food webs depend on the implicitly equal strength of the connections, anyway?) Ordination is valuable, though, and need not be the mindless reworking of old problems that some critics make it. The book is a competent introduction. Its considerations of transformation and display of data are unusually good, and a chapter on asymmetric matrices does even introduce structured data. Real data are used throughout, and one set gets repeated workouts. As is appropriate, inference is emphasized; tests are indeed awkward for the situations discussed, but advice on what to do when that's what one really wants would be welcome. -LMV

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**Analytical Biogeography.** An Integrated Approach to the Study of Animal and Plant Distributions.
Edited by **Alan A. Myers** and **Paul S. Giller.** 1988. Chapman and Hall. xiii + 578 pp. Hardbound. $87.50.

Biogeography isn't as bad off as systematics in the intensity of its controversies, but it's not far enough ahead to be proud of it. Part of the vitriol is a pleiotropic spillover from systematics, but there is the addition of panbiogeography, the nonphyletic method of generalized tracks (multiple congruences of the broad distributions of sets of modern organisms). Its founder, Croizat, was in other respects largely incompetent, and his remarkable scattershot attacks on anyone in sight are worthy of any cladist. There is a small dogmatic school devoted to his method, but it could really be used by others to supplement rather than supplant other methods.

There are also ordinary scientific controversies in biogeography. The purpose of this volume is to get proponents of the various views and approaches to evaluate their subjects without trying to destroy others. Surprisingly, it largely succeeds in doing so. Not all major views and topics are represented, all participants are biologists rather than geographers, and there is a strong terrestrial bias except for Amy Schoener's wonderful treatment of island biogeography from an aquatic perspective. But coverage of topics and approaches is better than one has any right to expect, and some authors are even critical of their own previous work. Scale itself is a theme of the book, and the diverse spatiotemporal scales in biogeography are included. The result is probably the best book available. It can nevertheless be improved on, by explicit synthesis and incorporating what is omitted, and one may hope that a competent and broad-minded single author will do so. -LMV

**Differentiation Patterns in Higher Plants.**
Edited by **Krystyna M. Urbanska**. 1987 (November). Academic Press. xii + 272 pp.
ISBN 0-12-709425-3. Hardbound. $39.95.

Not differentiation in development, but differentiation among populations. The book represents the fuzzy non-boundary between what are often called biosystematics and population biology. extending somewhat into each region. It is mostly a collection of rather ordinary papers and has a reasonable diversity within its scope. One author (Quinn) was unable to do the reciprocal transplants that would have helped his work; I mention this because he is quite conscious of a common failing in controlled experiments. Put the organisms in the same conditions and see how they differ. Well, maybe if you put them in some other conditions the pattern of variation would change too. This is common knowledge in his field and some others, but elsewhere it isn't just students who trip up.

There is one genuinely interesting result in the book. Cook investigated a dozen aquatic species which are serious weeds and found that most of them spread only, or almost only, vegetatively outside their native range. The aggressive vegetative reproduction turns even sexes and mating types into competitors, so that often only one survives locally. He doesn't pursue the matter, but apparently this is the result of a known but often-ignored disadvantage of sexual reproduction: if a locally adaptive genotype is produced, it doesn't get broken up again immediately. Such an advantage for asexuality supports the importance of epistatic interactions, because mass selection could give the same result in a few sexual generations if there were near additivity and the observed strong selection. -LMV

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**Quantitative Aspects of the Ecology of Biological Invasions.** Reprinted from Philosophical Transactions of the Royal Society of London (B) 314: 501-742 (1986).
Edited by **Hans Kornberg** and **M.H. Williamson**. 1987. Royal Society. vi + 240 pp.
ISBN 0-85403-302-5. Hardbound. £40.00 (about $64).

An invasion may not be successful, so what is an invasion? Call it an immigration of a species not locally present and one reaches the subject of the book. (Presumably the habitat must be potentially suitable; Darwin, and Holdgate in this book, saw many butterflies over the sea off Patagonia, and millions of individuals of a now-extinct plague grasshopper are entombed in Grasshopper Glacier, Montana, on which they presumably landed in a snowstorm. This sort of rain of waifs is only the extreme of the common disadvantage, at the individual level, of dispersing away from one's native population.) So dispersal is required. This may be merely an expansion of the boundary of an existing population, or it may involve longer distances. The requirements for establishment of a successful population differ from those involved in getting there. The book emphasizes Britain, and there about 10% of detected invaders are successful (presumably a smaller proportion of all invaders), and a further 10% or so become pests to humans.

Obviously properties of both the invader and the habitat are relevant to successful invasion. The former question is the main focus of the book, with an overall conclusion that no single attribute can be identified as applicable to most cases. A low variation in population size may be best, but it has many exceptions, such as irruptive species which may have frequent local extinctions. Anything which one can reasonably think will help an invader does so sometimes. A wide diversity of organisms are treated in the book, from birds to viruses to angiosperms, and there are lots of cases discussed or mentioned, although all are nonmarine. These leaven even the mathematical treatments, which are, unusually, used as tools rather than as answers. There are various interesting tidbits throughout; e.g., Williamson finds that the probability of establishment of an angiosperm species is higher if it belongs to a family with fewer species. And invaders aren't isolated entities; they occur with other species in communities, which their invasion has modified to a greater or lesser degree. Oddly, the book gives rather little attention to this central question. Crawley finds (although Lawton disagrees) that the probability of establishment of herbivores is greater than that for predators. He interprets this as resulting from lower competition among herbivores, but many of his examples of herbivores are seed-eaters, for which the prevalence of competition is, I think, not really controversial. Even more than with most Royal Society symposia, the published discussions enlighten the book. There is no index, a pity in a volume with so many examples. -LMV

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**Biogeography and Quaternary History in Tropical America.**
**Edited by T.C. Whitmore and G.T. Prance.** 1987 (3 September). Oxford Univ. Press. x + 214 pp. + erratum sheet. ISBN 0-19-854546-0. Hardbound. $89.00.

We now realize that climatic changes in the past million years have affected the biota of the Neotropics as well as those of the rest of the world. But how? An early (late 1960s) conclusion was that forested regions which now have many endemics were areas where forest remained during the drier intervals. The existence and importance of such refuges remains controversial, and the four authors of this book have joined together to look at the problem. They have produced quite a lot of new information, on distribution of several taxa and even with new vegetation and soil maps, as well as reviewing some previous work. The results, from species of birds and some angiosperms and subspecies of some butterflies, fit expectations from ostensibly independent physical evidence reasonably well. The analyses are made with attention to confounding factors, although often not enough detail is provided. A generally similar pattern to that expected from refuges can result from widespread parapatric speciation without refuges, as noted in the final chapter. This seems to me less likely because the forest probably has actually been fragmented during most of the Pleistocene, and allopatric speciation needs less restrictive conditions than parapatric speciation does. Perhaps enough of the biota will survive long enough to tell us. -LMV

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**The Theory of Evolution and Dynamical Systems.** Translated from German of 1984.
**Josef Hofbauer and Karl Sigmund.** 1988 (October). Cambridge Univ. Press. viii + 341 pp. ISBN 0-521-35288-6 hardbound, $65.00; -35838-8 softbound, $19.95.

If you aren't reasonably close to being an applied mathematician, this book isn't for you. Maybe it shouldn't be anyway. Mathematical biology, as she is usually practiced, takes a more or less amenable biological topic, simplifies it close to (but not beyond) the edge of recognition so as to abstract workable structure, and examines the implications of this structure. Whether the result then bears realistically on the biology, its ostensible motivation, is ordinarily not considered. In mathematical language the result is a sort of existence proof, that there are conceivable conditions under which the outcome holds. From a biological perspective the result is then a metaphor, a suggestion of reality. We have, e.g., an entire literature based on the Lotka-Volterra equations, which rarely apply with adequate precision to real biological cases. One needs to absorb the biology, or defer to someone who has, in order to escape this morass. Some people do, and their contributions can then be magnified by their grasp of the mathematical structure and its implications. Most don't, and that is the case with this book (although there are nuggets if one can find them). For instance, two versions of Fisher's Fundamental Theorem are proved without initial attention to its domain of validity or apparent knowledge of Curtsinger's generalization of it. There is even no mention of Leslie matrices, despite considerable attention to population ecology; almost all the book deals with continuous variables, more traditional in mathematics. A book like this may motivate mathematicians to think about biology, because abstracted structures can lead to nontrivial mathematics, but one is then in a world where one's results bear importantly on neither mathematics nor biology. There are whole journals for this sort of adaptive valley. It seems to have an internal attractor and to be difficult to escape from once entered. That is a true shame, because a mathematical approach can illuminate when it is internal to real biology, and much of biology could benefit.

The book itself is clear enough and a reasonable place to start if one really wants to enter the adaptive valley. Good for the sort, but the sort ain't good.

-LMV

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**Multivariate Statistical Methods. A Primer.**
**Bryan F.J. Manly**. 1986. Chapman and Hall. x + 159 pp. ISBN 0-412-28610-6 hardbound, $35.00; -28620-3 softbound, $15.95.

It isn't necessary to be intimidated by multivariate analysis. Most introductions to it are opaque and the calculations do tend to be long. Computters now diminish the second problem but the resulting solution can be worse than the problem itself if one relies blindly on somebody's canned program. The answer one gets may be to what someone else thought the question, or the form of the data, ought to be. This is a real and common problem and can be addressed only by understanding what ought to be done before trying to do it, and then seeing if the program actually does what you want.

So we are back to the first problem, the algebraic emphasis of most expositions. I suppose there really are people who think most comfortably in an algebraic mode, but they don't often become biologists. Manly has given the best overall treatment (medication?) that I recall. He does so by dwelling on what the methods mean and do, and to some extent with their problems; the reader is usually referred to accessible sources elsewhere for detailed procedures. Real data, mostly from natural history, are used as examples. The geometric transparency of principal components doesn't come through, though, because there is no comparison with the bivariate case, and the critical dependence of such methods as standard discriminant analysis on the restrictive assumption of multivariate normality isn't brought out. There is moderate attention to something which has come to be known as Van Valen's test, a robust method for testing homogeneity of variances, but this is really a trivial extension of Levene's univariate test and I'm surprised that my name is associated with it. But I suppose Kendall was surprised that his name was adopted for his similar extension of the *F* test. Multivariate arrays of data have aspects of structure which aren't yet addressed in the main statistical literature but which are important to biologists, such as effective dimensionality (equivalent number of independent characters) or tightness of the multivariate distribution. Some of these have measures (e.g. *J. Theor. Biol.* 45: 235-247 [1974]) which may yet be useful.

-LMV

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**The Background of Ecology**: Concept and Theory.
**Robert P. McIntosh**. 1986. Cambridge Univ. Press. xiii + 383 pp. ISBN 0-521-24935-X hardbound, $42.50; -27087-1 softbound, $16.95.

This is a history of ecology as written in the English language. Sources in other languages are ignored; de Candolle is mentioned in passing but not cited, Margalef is cited only in English, and Lamotte is omitted entirely, as are most others. This general lack actually doesn't make the account strongly parochial, because ecology is one of the few sciences where only occasional works of lasting significance have appeared in other languages. The author doesn't even seem to realize this.

Nevertheless, the book is a good one and is scholarly in its chosen domain. It extends from the ambiguous origins of the science into the 1980s and covers all subfields without too much bias. McIntosh doesn't hesitate to editorialize, though, and the book does have a decidedly whiggish flavor. In fact I'm not entirely sorry it does. One thing we need to get from histories is where our current ideas came from, and that's what whiggery does best. For that sort of question, and for a reasonable and clear overview, this book is the place to start. -LMV

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**The Darwinian Heritage.**
Edited by **David Kohn**. 1985. Princeton Univ. Press. xii + 1138 pp. Acid-free paper. ISBN 0-691-08356-8. Hardbound. $95.00. Reprinted 1988 softbound; 0-691-02414-6; $25.00.

"Darwin became a focus of detailed study only after the evolutionary synthesis, which enshrined Darwinian natural selection, was consolidated and widely diffused" (Kohn, p. 2). "Although historical sympathy is a great virtue, it is not clear that a degree of ignorance of the relevant anatomy exceeding Darwin's has led to compensatory insights in Darwin studies" (Rachootin, p. 156). Indeed. We honor Darwin for his biology (and, yes, his geology), but these rarely get studied in any detail as such. There are whole talus slopes on origins, reception, social aspects, philosophy, and so on, and these are all well represented in this volume, which is a major mine in Darwin's mountain. The book's title is thereby unintentionally ironic, because Darwin's real heritage is slighted although not absent. The editor even proposes that future Darwinian studies should be directed primarily toward social aspects and to what Darwin read.

Darwin was the greatest biologist of all time, despite suffering severely from brucellosis or Chagas's disease throughout his adult life. It is appropriate in a book on Darwin that Wallace comes in only as a foil for him. However, Wallace did, after all, also make Darwin's greatest discovery (filling in the same deductive argument. again after reading Malthus), and he did a lot besides. Where is the Wallace industry? Some spillover onto people like Weismann, Huxley, Lyell, Haeckel, Cope, Owen, Cuvier, the Geoffroys, and others would probably be more useful than digging Darwinian details, especially if there is some focus on what these people actually did and why they did it.

Rhetoric has an unpleasant way of substituting for evidence, especially when there are some selectively chosen tidbits of the latter included for the reader who demands verisimilitude. In this volume two authors, La Vergata and especially Young, extrapolate from Darwin's prescient realization that natural selection occurs within and among human cultures, to claiming his advocacy for the laissez-faire economics that characterized Social Darwinism. One may then ask why Darwin has been a hero in the Soviet Union. Was he also a communist? Science in itself doesn't give ideologies, and Darwin's advocacy of positive eugenics is mirrored by that of some communist geneticists in our century. Maybe the next Darwin-hater will try to make something out of this communist connection, which does have deeper historical roots than support Young's effort.

There are a lot of good things in this book, from Cohen's evidence that Bishop Wilberforce accepted natural selection (only stabilizing, though, like even Aristotle did [as Cohen omits to say]) to Mayr's analysis of what he calls Darwin's five theories of evolution. Mayr reinterprets Darwin's gradualism to show what it was, and how it was revolutionary and necessary for population thinking. And the book is all so serious, a symptom of the disease called scholarly endeavor but a contingent symptom evoked by the environment rather than a necessary one. -LMV

**The Cuvier-Geoffroy Debate.** French Biology in the Decades Before Darwin.
**Toby A. Appel.** 1987 (26 March). Oxford Univ. Press. (xi) + 305 pp. + 16 plates. Acid-free paper. ISBN 0-19-504138-0. Hardbound. $35.00.

Cuvier is sometimes referred to as the father of comparative anatomy. He did write a major treatise with that title (en français, c'est entendu) and was the first professor of it anywhere. It is ironical, though, that his comparisons were primarily of function as manifested in structure, of what we, following Owen, now call analogs. He was the first major functional anatomist. What we now call comparative anatomy is to a considerable extent the child of his protégé turned bitter rival, Etienne Geoffroy Saint-Hilaire. Geoffroy transformed the concept of homology (which he called analogy, but don't worry about that) from a curiosity into the central concept of an effectively new discipline. Goethe and others had made the form of organisms a focus of study; Geoffroy took Goethe's discipline of morphology and made it comparative. (I simplify a bit.) In doing so he made a major discovery, that homologs can best be recognized by their morphological interrelations, his "principle of connections."

Cuvier celebrated the glory of God as manifested in the close correlation between the form of a structure and its use, the origin of the teleology of the earlier biologist Aristotle. Geoffroy followed his colleague Lamarck into the heresy of evolution as an explanation for large-scale homologies, which impose real constraints on function. Cuvier regarded theories as, if anything, inductions from facts; Geoffroy looked for evidence which could be used to back up his theories. There were more personal rivalries, but these two divergences are still with us in not-too-modified form. Cuvier really didn't recognize homologies of particularly modified structures. He had a running dispute with Geoffroy (part of the debate in the book's title) as to whether the hyoid bones of different vertebrates, and even some different primates, were homologous or simply bones put in about the same place. Geoffroy, on the other hand, saw homologies everywhere, the major features of all animals being homologous in an evolutionary scala naturae.

Their successors, including Geoffroy's more lucid son Isidore, eventually adopted aspects of both views, as did Owen and Darwin. The book deals admirably with such matters too, and with others I have ignored. We have kept the synthesis, but aspects of the tension remain and the narrow-minded on both sides (or four sides: all combinations exist) still may be found to abjure their opposites. -LMV

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**The Great Devonian Controversy.** The Shaping of Scientific Knowledge among Gentlemanly Specialists.
**Martin J.S. Rudwick.** 1988 softbound reprint of 1985 book. Univ. Chicago Press. xxxiii + 494 pp. ISBN 0-226-73102-2 softbound, $19.95.

It's hard to see the forest for the trees in this book, or even to find out exactly what the problem is without plowing through the designated fields. There's a lot here, though, with short excursions in many directions and a variety of snags for the woodpeckers. -LMV

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**Charles Darwin's Beagle Diary.**
Edited by **Richard Darwin Keynes.** 1988 (July). Cambridge Univ. Press. xxix + 464 pp. ISBN 0-521-23503-0. Hardbound. $59.50.

The diary itself wasn't published until 1933, and the 1988 version uses modern standards of transcription and scholarship. There are a lot of differences from the Journal of Researches, too many and too major to make a detailed comparison feasible in a book like this. The editor has added many helpful notes throughout (occasionally inaccurate as to natural history, though) and in his introduction, and has provided a glossary of relevant people. The index is less useful than it might have been; it consists largely of places and people. Not even tortoises (e.g., p. 362) or Megatherium (p. 109) get in. There are useful contemporary maps and attractive figures, mostly from the King and Fitz-Roy volumes of the original Narrative. -LMV

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**The Southern Ark**: Zoological Discovery in New Zealand, 1769-1900.
**J.R.H. Andrews**. 1986 (1988 this publisher). Univ. Hawaii Press. xii + 237 pp.
ISBN 0-8248-1192-5. Hardbound. $39.00.

New Zealand's fauna is impoverished, although less so (to an unknown extent) than it was before the Maoris arrived. It nevertheless has its share of remarkable animals: dwarfed kiwis with their enormous eggs, the recently decapitated radiation of moas, the living fossil Sphenodon, or the small group of orthopterans called wetas. The latter, Deinacrida in the Stenopelmatidae (or Henicidae, if split) reach more than 8 x 3 cm and have a niche like that of mice, which were absent until recently introduced.

Almost nothing of this natural history itself is included in Andrews's book, which pretty much assumes that the reader knows the animals already. Knowing them isn't really necessary, because the book is about the process of their discovery, but more information would have helped us foreigners. (The book was first published in New Zealand.) In its scope the treatment is both scholarly and readable, with notes attractively occupying much of the wide margins. English names are used, usually but not always with an indication somewhere as to what they really refer to. Gorgeous color reproductions of early illustrations adorn many of the pages.

The book is organized mostly by the explorers and naturalists, although moas and the flightless rail Notornis appropriately each have a chapter of their own. The early Maoris weren't officially scientists, so what they knew isn't investigated, although Andrews does note that they were apparently the first to conclude that the introduced terrestrial mammals were causing reduction of the native birds. The earliest naturalists were not as careful as one would like; that this is not merely hindsight is shown by Linnaeus's contemporary worry that the material from the first voyages wouldn't be published. He was right to worry. Most of the specimens (even those that made it to the British Museum) were gradually lost without having been described; one jellyfish, of a group which Europeans usually consider inedible, was eaten in the field after having been sketched and named.

Andrews brings out much of the flavor of the exploratory work, that part done in Europe as well as that in New Zealand. It's a good book. -LMV

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**The Comparative Reception of Darwinism**
Edited by **Thomas F. Glick**. 1988 reprint of 1974 book, with new preface.
Univ. Chicago Press. xxviii + 505 pp. ISBN 0-226-29977-5. Softbound. $17.95.

It's good to have this rather important book again available. Glick's new preface reviews studies since 1974. (Japanese boasts the most translations of the Origin, apparently because the language slowly expanded to incorporate scientific concepts.) It still isn't really comparative, and the treatments in the book, being by specific country or religion, are in the nature of data for a really comparative treatment at the next level of abstraction. -LMV