VIEWS AND REVIEWS

Molecular Chauvinism and its Cure

Molecules and Morphology in Evolution: Conflict or Compromise? Edited by **Colin Patterson**. 1987. Cambridge Univ. Press. x + 229 pp. ISBN 0-521-32271-5 hardbound, $49.50. ISBN 0-521-33860-5 softbound, $15.95.

Joel Cohen's felicitous term "physics envy" has its counterpart in what can be called molecular chauvinism. Most molecular chauvinists are of course cell biologists and such who think that the evolutionary half of biology is trivial because they have never looked at it. Most, but not all. Sibley and Ahlquist, for instance, say in this book that molecular evidence is necessarily superior to morphological evidence because the latter is subject to convergence, is subjective, and is not quantitative. While the paper by Andrews more or less adequately disposes of these criticisms by an unusually careful consideration of much of the evidence of all kinds then available on the phyletic relations of extant hominoids, it is interesting that later work by others shows that molecular and morphological characters have about the same average propensity for convergence. The real advantage of molecular data is that they provide many characters which are otherwise unavailable: each nucleotide or amino-acid position is a character. This isn't to say that all characters have the same value, even for molecules, although a preliminary analysis of the data can justifiably be made this way. Several papers note ways in which one molecular change can be more informative than another.

Jerrold Lowenstein (not represented in the book) has conveniently set himself up for target practice by repeatedly responding to all substantive criticism of molecular phylogenetics with merely the claim that the critic obviously doesn't believe in chemistry. What his trivialization shows, of course, is just that he doesn't understand biology, including and especially molecular evolution. This book as a whole is one of the best antidotes for such allelopathic ploys. There is the expected quota of potboilers, but even these are useful as mostly interrelated reviews. In the only paper not focused on vertebrates Woese characteristically ignores criticisms of his phylogeny of prokaryotes, an attitude which doesn't help his credibility. In addition, as an extension of his proposal that the relatively great divergence of mycoplasmas is a result of their high mutation rate (itself related to small genome size), he suggests that major events in metazoan diversification may have resulted from locally high mutation rates. Even the mycoplasma story has an unconsidered alternative interpretation, though: it is entirely possible that their loss of cell walls made many other changes necessary, and that the currently high mutation rate is a byproduct of such a largely adaptive reorganization, via the small genome size. (The same general hypothesis has been made for the even more divergent Archaebacteria.)

Most of the papers do consider different sorts of evidence seriously and evaluate each datum on its own merits. In this way molecular chauvinism can be cured unless the patient insists on regarding the illness as a virtue. A closed mind is no better in a biologist than in a fundamentalist, and the mind that marks us from the gibbering apes retains the easy path toward prejudice. Quasi-objectivity doesn't imply correctness, though; one can't use effectively unrooted trees to determine the direction of evolution, as Bishop and Friday try to do.

One paper is a major contribution, McKenna's comparison of estimates of mammalian phylogeny derived from two proteins separately and from morphology. He explicitly treats each position as a character and gives the changes which

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Contribution 81, Lothlorien Laboratory of Evolutionary Biology.
Evolutionary Theory 8: 209-279 (May, 1988).

characterize each node for the proteins as well as for morphology. The treatment is original and fully critical for all relevant aspects, including his own past work. That all reasonable alternative phyletic topologies aren't evaluated is a necessary result of the complexity of the data already available; it could have been noted for the diagrams as well as in the necessarily incomplete character analyses. One general result worth noting is that the direction of variation in rate of protein evolution can depend on the protein; if not an isolated case (and primates seem to provide another example for two proteins not used here, and Goodman et al. suggest others) this automatically removes generation time and the like from being an overall explanation. McKenna gives a good argument for doing his molecular phylogeny by hand in complex cases. The argument applies even more strongly to morphological data. There have been too many cases of computterized nonsense in the literature. "One should follow the computer only up to the edge of an intellectual cliff."

The argument is made in this book and especially elsewhere that distance data, such as from Sibley's DNA hybridizations and Woese's oligonucleotide catalogs, can't give phylogenetic information because the data lack a primitive-derived polarity. Thus if A and B form a clade having sister-group C, and the distances of B and C from the common triple node are equal but less than that of A, then B and C will be interpreted as the clade. The problem is not in the topology, which distance data do estimate usefully, but in the rooting. The topology, though, is a large part of the phylogeny and itself excludes most possibilities. Roots can often be estimated by other means, as with Sibley's birds. Methods such as DNA hybridization average the effect of many characters; although one has to be careful, as with taking too little care with error of estimation, the results are often valuable and are always worth consideration. Chauvinism works both ways. -LMV

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Fisherian and Wrightian Species

Mammalian Dispersal Patterns: The Effects of Social Structure on Population Genetics.
Edited by **Diane Chepko-Sade** and **Zuleyma Tang Halpin**. 1988 (copyright 1987). Univ. Chicago Press. xviii + 342 pp. ISBN 0-226-10266-1 hardbound, $55.00; ISBN 0-226-10268-8 softbound, $19.95.

Despite its title, the focus of the book is on effective population size (N_e), the geometric mean over time of approximately the number of individuals who will mate in a population or neighborhood. Why should one be interested in this? The usual answer is that it gives an indication of inbreeding effects, as with currently endangered species. But usually this is a rather unimportant result; even for species near extinction much the greatest risk is from variation in mortality and reproduction unrelated to degree of homozygosity. More significantly, N_e determines the relative evolutionary importance of drift and natural selection. For selection coefficients less than about $(2N_e)^{-1}$ the allele is effectively neutral and drift rules; for higher values selection is of overriding importance. The difference is fairly sharp and marks the border between the neutralist and selectionist worlds.

We can extend the dichotomy, more fuzzily, to species. An average dispersal of one breeding individual per generation converts two populations or neighborhoods to one, insofar as N_e is concerned. Less dispersal is needed for integration of locally advantageous alleles and more for locally disadvantageous ones, and the relation is nontransitive, so there can be isolation by distance if there isn't enough dispersal between distant populations. But we can see here a distinction between what may be called Fisherian and Wrightian species. In a Fisherian species mass selection predominates, including selection for local adaptations if it is strong enough to outweigh immigration, including the founding of new populations by dispersers from two or more old ones. In a Wrightian species, one where the shifting-balance theory applies if epistasis is appropriate, drift is more important and is the only restriction on local adaptation to a single adaptive peak. As drift facilitates most chromosomal evolution, the latter should be more prevalent for Wrightian species. (Of course some species can be Fisherian in one time or place and Wrightian in another, and some species don't fit the classification, not even being intermediate.)

Some mammals (caribou) and others (Drosophila melanogaster) are clearly Fisherian. Are any mammals Wrightian? Probably, although this is harder to show. In fact most of the book is devoted to detailed investigations of single species. A multiauthored concluding chapter tries to estimate N_e for each species considered and concludes that some mammals are indeed Wrightian. The difficulty is, as Templeton notes in another chapter, that they would then be expected to lack the inbreeding depression observed in similar species; deleterious recessives would be selected out, as they are in inbreeding angiosperms. It is not obvious which line of evidence has taken a wrong turn; one can argue against each with a little ingenuity. Come back next decade and find out. -LMV

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Our Molecules Evolve Too

Molecular Biology of Homo sapiens.

Cold Spring Harbor Symposia on Quantitative Biology 51. (P.O. Box 100, Cold Spring Harbor, N.Y. 11724.) 1987 (stated 1986 in book). xxiii + xv + 1229 pp. in 2 volumes. ISBN 0-87969-052-6 hardbound, $160.00. ISBN 0-87969-053-4 softbound, $80.00.

The evolutionary half of biology used to be moderately prevalent among the Cold Spring Harbor symposia, but even 100 pages sandwiched between sections on genetic diagnosis and drugs isn't altogether trivial. The papers are rather heterogeneous, from phylogeny to malarial selection on thalassemia, and some are even interesting. At the other extreme, the Bernardis conclude from a similarity in base composition between coding and noncoding DNA that the latter is functional. Mutational bias is unimportant for functional genes, though, only if there is not much more than one adaptive peak. Similarly, in the symposium's introduction Bodmer thinks that all (and only) nonfunctional DNA is selfish; he also regards some animals as higher than others and has chordates extending well into the Precambrian. On the positive side, Russell Doolittle et al. note neutrally that introns may not be primitive after all, with two or three reasonable arguments. Stoneking et al. calibrate the average evolutionary rate of human mitochondrial DNA in a suitable way; they note the existence of a very large sampling error but then proceed to ignore it. Andrews gives a characteristically balanced evaluation of evidence bearing on human phylogeny, noting that there is quite good evidence for both a chimpanzee-gorilla clade and a chimpanzee human clade. Other evidence in this volume and elsewhere only reinforces this conclusion. A way around most of the difficulty here would be for an ancestral polytypic species to evolve partly in each direction before finally breaking up. Most parallel evolution could then in principle be avoided. Perhaps, e.g., a proto-human-chimp subspecies didn't find the diverse components of knuckle-walking and thinner enamel advantageous until it itself had split up. This is a somewhat awkward resolution, but on present evidence it is probably less awkward than the alternatives. -LMV

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Community Selection, an Immunological Paradox, and Us

Ecological Imperialism: The Biological Expansion of Europe, 900-1900.
Alfred W. Crosby. 1986. Cambridge Univ. Press. xiv + 368 pp. + 16 plates. ISBN 0-521-32009-7 hardbound, $22.95. ISBN 0-521-33613-9 softbound, $10.95.

The pervasive importance of group selection in the history of our species is taken for granted, although the term isn't yet. We also know. in a more general way, the expansion of various nonhuman members of the European biota into foreign lands. Crosby gives us a picture of what can perhaps be called community selection, in this case the expansion of a variously bounded set of adaptively related species at the expense of some of the indigenes.

He writes of Neo-Europes, the temperate regions overseas which were settled and became like Europe. He doesn't deal with areas like Hawaii (or indeed the rest of Polynesia) which are tropical but had similar experiences, although not always from Europe. The Canaries, though, receive a chapter. Humans, domestic animals often becoming feral, commensals like rats or weeds, mutualists like honeybees, and viruses and other parasites. The movement is mostly unidirectional. How come?

The key, to Crosby, is the ecological relationships among the invading species rather than their properties apart from each other. Humans are the key species, but even they have major and even decisive help from accompanying parasites. Local humans supposedly hadn't been around long enough for many indigenous diseases to evolve. Domestic animals overrun regions which had been much more than decimated by the megafaunal extinction a few thousand years ago, itself perhaps caused or aided by (in these regions) earlier invading humans. In forested areas clearing trees creates much more edge and openings than had been available. More generally, the plow creates major disturbance of the soil, and domestic animals contribute their bit to that also. Weeds and other early-successional, edge, and prolific species find an environment on the whole often more like Europe than like the local environment before European invasion. The indigenes have to be pretty good to invade the source, so the process is one-sided. The Great Plains of North America succumbed only after the buffalo did.

It makes a good story, told in a fascinating manner, and it hangs together pretty well. But is it right? This is less obvious, but I suspect that it will prove to be so on the whole. What we need is more detailed ecological evaluations of specific cases, samples on which the edifice itself can rest (or founder, as the case may be). It isn't enough that the picture makes sense and that there are no real competing hypotheses, particularly not enough when we can probably really understand some cases with more work. I hope the book stimulates it. Another way to go, although the evidence will probably be sparser, is to consider other human expansions, situations in which some of the variables differ from the set of cases Crosby uses. How and why do the results then differ, exactly? Crosby does consider cases like the Crusades which failed for reasons amenable to his theory.

There is one potentially major difficulty, though. According to current immunological theory the past history of a population should be irrelevant to the ability of its present members to form particular antibodies. Each individual generates a new set in a way with respect to which selection among individuals is apparently impotent. Perhaps, then, the apparently high susceptibility of unexposed populations is due to the often greater effect on adults than on children; the invaders would have been exposed when young. I don't think that this will work, though. Crosby, and also William H. McNeill (1976, Plagues and Peoples) cite cases where children were also susceptible. I hope to return to this problem as such in more detail elsewhere. The relevant historical cases need to be collected and examined specifically from this perspective, but the historical evidence is already persuasive enough to warrant direct immunological experiments also. Can specific resistance be selected? Do inbred strains of mice differ in relevant ways? And so on. As the situation stands the evidence from history seems to contradict that from immunology; this is an unresolved paradox. -LMV

How Not to Conserve the Natural World

Advances in Animal Conservation.
Edited by **J.P. Hearn** and **J.K. Hodges**. 1985. Oxford Univ. Press. xx + 282 pp. ISBN 0-19-854002-7. Hardbound. $69.00.

How best to minimize the crushing impact which our population growth has on other species? Try to make relevant people care, perhaps, and provide those who are grindingly poor with alternatives? Deal with the root cause itself? The book hardly considers matters like these, and it doesn't even mention the demography and biogeography of reserves or the short-term economic advantage of overexploitation. It focuses on mostly peripheral matters, the sorts of things governments want us to be concerned with so that we don't get in their way and maybe accomplish something. I don't know if much can be accomplished, but it won't be by things like this book discusses. They are palliatives and will help a little here and there, mostly for the cuddly and singing favorites, while the world becomes an agricultural and urban wasteland until it collapses from misuse of resources. I am not exaggerating; this seems to me our most likely future if we escape the bomb. There is no imagination in this book. There are some imaginative things being done; some were known at the time of the symposium the book comes from; and two papers do allude in a general way to the nature of the real problem and to relevant political considerations. But we won't modify the juggernaut by embryo manipulation and otherwise being docile to the overriding short-term perspective of economics and most governments. -LMV

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Information and Entropy in Evolution

Entropy, Information, and Evolution: New Perspectives on Physical and Biological Evolution.
Edited by **Bruce H. Weber, David J. Depew**, and **James D. Smith**. 1988. MIT Press. xi + 376 pp. ISBN 0-262-23132-8. $37.50.

I have never seen thermodynamics (as practiced by physicists) as having any particular bearing on evolution — one has to see how they are consistent, which is a trivial exercise once one understands the concepts, and that's about it. Chemical thermodynamics does have some relevance to the origin of life in indicating the probability and direction of reactions and stability of products under various conditions, but this is more chemistry than biology. Otherwise, it has seemed even less relevant than the information theory of communication scientists. The latter is, after all, applicable to polymers of defined components, like DNA. Nonphysical applications of entropy, or something like it, have been important in ecology and elsewhere, and some are discussed a bit in this book. The same is true for nonthermodynamic applications of energy.

This book only reinforces the above perspective. Here we have a number of apparently intelligent people trying to relate evolution in a nontrivial way to thermodynamics and a thermodynamically expressed view of information, and none of them show that the enterprise even has promise. Yes, the Earth is an open system. Yes, organisms are dissipative structures (what are perhaps more familiarly known as [dynamic] steady states). So what? Nothing evolutionary follows from this except by forced analogies and by ignoring the conditions of the physical theory being applied. At least nothing has so far. Hull (called Hall on the dust jacket) notes the existence of many not-quite-equivalent uses of the term "entropy". And that's just the tip of the iceberg.

Even an astrophysicist can get caught in the mess. Layzer repeats his definition of information, which Wiley also uses in a central way. This is the difference between the maximum potential entropy (for the same number of units) and the realized entropy. There are contexts where this notion is useful, but it isn't general. For instance, one consequence is that a big perfect ice cube has more information than a small one, and each has much more than the DNA in any genome.

I don't know what measure to use for the information in a phenotype. Something like the degree of unpredictability or the minimum algorithm for construction suggest themselves, but these aren't well defined (perhaps not even in principle), part of the information comes from the environment (different environments give different phenotypes) and part is noise, and anyway the longest minimum algorithm is sometimes used to define randomness, a perhaps different notion. There aren't even any natural components of the phenotype to play combinatorial games with, since even the elemental composition and total size are aspects of it. Inherent information is probably the same as the information needed to generate something, but this may have a subjective component; a random sequence has no information but needs a lot as a specific sequence, while it needs none if regarded as merely random. Inherent information is organized complexity. I think I have more information than a bacterium does, and I would think so without knowing anything about genomes. Why, exactly? Probably a consideration of realized and unrealized developmental paths as such in their full detail, with allowance for recursion, partial and complete replication of subprograms, and continuous (including time) as well as discrete choices, will give a suitable answer. So we have a fuzzy problem waiting for a theory of measurement.

The first two editors of the book give a mostly incompetent review of the status of neoDarwinism from what they think is a thermodynamic viewpoint. Although near the end of the book it sets the tone; one could make several sorts of comparisons with a swamp. -LMV

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Adaptation and Optimization

The Latest on the Best: Essays on Evolution and Optimality. Edited by **John Dupré**. 1987. MIT Press. xii + 359 pp. ISBN 0-262-04090-5. $29.95.

Does natural selection always increase adaptation? Of course it does, on the level and time scale of the selection itself. This truism is commonly unappreciated and even denied, but it is a necessary and deductive consequence of the process of selection itself. The real problems come when we consider how far beyond this level and time scale the adaptation extends, and to what degree it does so; and how to make inferences about adaptation when, as almost always, we know the result but not the process.

Adaptation, which is a word used variously for the absolute degree of success or a process of hill-climbing, shouldn't be confused with optimality, the state at the top of the (perhaps tallest) hill. Yet people often do. The whole subject appears murky enough to have attracted the attention of philosophers, who generated this book; that the murk may be as much in our eyes and in the meanings we give to words, as in the subject itself, is then an advantage rather than a hindrance. A moderate part of the book concerns itself with matters like these, even when the authors think they are discussing something substantive. And did you know that species like our own can be variable in behavior but not in physiology, as the editor thinks?

There is a real substantive problem which applies in principle to much of biology and in practice to more specific studies: how do we determine what is, or has been, optimal? Several authors give more or less skeptical views on this, perhaps appropriately for the behavioral orientation of much of the book. Yet look at the real world of environmental physiology, say. The salt tolerance of <u>Halobacterium</u> is obviously an adaptation now, whatever its origin; it helps the organism live where it couldn't live otherwise. ATP synthesis helps it live at all. And so on and on. It isn't always so easy to decide, and that's where legitimate controversy can arise, but criteria which purport to make even cases like these problematic are thereby themselves suspect.

One subsidiary aspect is that we don't always ask for the domain, or even the state space, of an apparent optimum. Consider a local phenotypic adaptive butte. On its top phenotypic variation is neutral. Greater divergence, though, gives lower fitness. An even greater excursion might lead to an even higher adaptive peak. And all this is within a single environment or spatiotemporal distribution of biotic and physical environments (the state space); an analogous pictorial representation is obvious for environmental differences. We should try to have some understanding of the nature of this space before making conclusions. I think, though, that we usually do; adaptive compromises are still adaptive. -LMV

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Diversities

New Directions in Ecological Physiology.
Edited by **Martin E. Feder, Albert F. Bennett, Warren W. Burggren**, and **Raymond W. Huey**. 1988 (stated 1987 in book). Cambridge Univ. Press. x + 364 pp. ISBN 0-521-34138-8 hardbound, $49.50. ISBN 0-521-34938-9 softbound, $19.95.

There is a common perception, which I have shared, that environmental physiology is mostly running in place, a brown-fat part of science. It has made real contributions, notably such a widespread demonstration of special adaptations that it is surprising to see the result not recognized as such in most of the evolutionary literature. This book records a workshop which aimed at removing the subject from its apparent rut.

The major directions proposed as new are the study of individual variation, a nomothetic approach, study of adaptive compromises (costs and benefits), and use of phylogeny, The papers are followed by discussions which can be as illuminating ans the papers. (A discussion of the use of regression and coregression, though, confuses the cases of prediction and estimation, for which different methods are appropriate. Also, I note that Feder's diagram to illustrate the simple notion of species selection doesn't even work.)

That the study of variation is thought to be new is remarkable. Indeed, most studies are now typological, and the pattern should be changed. There has been, though, a great amount of work on intraspecific physiological variation, and the book ignores it. Part of this is on plants, which are excluded from the scope of the book but which have much to offer, conceptually and otherwise. The entire field of physiological anthropology, which is based entirely on intraspecific variation, is ignored. So is Roger Williams's book Biochemical Individuality (published in 1956!). Pharmacogenetics has been around for decades. And so on. Another way to advance would be to integrate with fields like these. Still another would be some degree of integration with population and community ecology, an integration which is now largely restricted to plants and not treated here.

Some participants seemed to think that understanding the diversity of our world by narrowly-focused studies is unimportant. I hope the view doesn't become predominant, as it partly did in ecology. Different ways of looking at the world lead to unexpected sorts of progress; let a hundred flowers bloom. Finally, the book doesn't distinguish clearly between intraspecific variation within and among populations; these are fundamentally different.

The book may help a narrowly focused field, but further expansion of the field's boundaries can help too. Although there is no substitute for imagination and intelligence, bright people can be repelled from a subject they perceive as mined-out even if it isn't. -LMV

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Naive Group Selection Revisited

Evolution by Group Selection.
V.C. Wynne-Edwards. 1986. Blackwell Scientific. xi + 386 pp. ISBN 0-632-01539-X hardbound, $66.50; ISBN 0-632-01541-1 softbound, $35.50.

Prudent predation is alive and well, or so Wynne-Edwards would have it. His 1962 book was largely phenomenological, giving an extended series of examples related to population regulation and which he thought inexplicable without group selection. He still thinks so, and he thinks that progress in the study of group selection since then has given a suitable mechanism. It hasn't. I say this despite my advocacy of group selection in general (I gave the first two widespread examples some years ago). There are two difficulties: whether the phenomena need a group-level explanation, and whether the group-level explanation is mechanistically adequate. I think that the book fails in both respects.

Take Wynne-Edwards's most extended example, Lagopus lagopus (red grouse). Its density is regulated by male territoriality at a level somewhat above that which would usually deplete its food. Dominance relations usually suffice for a male to defend a territory from a nonterritorial but upwardly mobile aspirant. So why should the loser give up? Wynne-Edwards thinks that the only reasonable explanation is that it is programmed to do so for the benefit of the group. However, an individual-level advantage also exists, as I noted in the general case in 1963 (Nature 198: 605-606) in response to the 1962 book. Bowing out avoids the costs of fighting when the probability of winning is low. The loser can then wait, alive and undamaged if in inferior habitat, to see if the territory-holder remains healthy. Wynne-Edwards doesn't rebut this and similar arguments; he simply ignores them. In my opinion every one of his cases, except the perennial problem of sexual reproduction, falls to similar individual-level explanations (including kin selection).

His mechanism of group selection for the evolution of such supposedly altruistic traits relies on the selective extinction of populations which overexploit their habitat. This usually doesn't work, though. It requires a very low level of dispersal among populations to prevent the infection of populations of altruists by cheaters, and the probability of extra-population dispersal commonly increases with density and so would be larger in cheater populations. Wynne-Edwards thinks that Wade's theory and experiments obviate the problem, but they don't. He does emphasize that individual and group selection are commonly in the same direction, and he thinks that this helps his argument, but his argument is for situations where they supposedly don't. I found all of this rather woolly and diffuse, using theory and examples from one context to support another where they don't apply.

Trophic levels, which he calls "trophic industries", come into the book too, as is appropriate. He lumps all "vivivores" (a useful term) together and says, remarkably, that they aren't regulated by resources because their food populations persist. This is indeed a perennial problem with respect to folivores, and why the world is green remains as mysterious as the persistence of sex. (In fact these two problems, and the status of their study, have interesting similarities.) But food is hard to catch for many animals, and to say that nonterritorial predators are normally prudent, reducing their own fitness to keep prey populations alive for the future, has no known basis in fact. This problem too has a literature which the book completely ignores.

There are a number of interesting cases in the book, which is a useful review for them. However, the theory, which is the book's stated aim, is best treated as a target for students to aim at. This is a shame; group selection has shown signs of coming of age, and an attempt to revive naive group-selectionism can only slow the acceptance of more adequate formulations. -LMV

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Somatic Selection in the Brain

Neural Darwinism: The Theory of Neuronal Group Selection.
Gerald M. Edelman. 1987. Basic Books. xxii + 371 pp. + 1 folded color plate.
ISBN 0-465-04934-6. Hardbound. $29.95.

Somatic selection is, among other things, a mechanism for developing more complex details than can be coded for in a hard-wired but entropy-prone genome. If a neuron's connection to a muscle is used it gets nutrient feedback from the muscle and persists; otherwise the connection degenerates. The propensity to make such a dual response is undoubtedly hard-wired at some level, but which connections have which response isn't. This phenomenon has only recently been adequately demonstrated, and Edelman doesn't deal with it. He goes much further, though: to him the minute-scale structure of the brain, and therefore of thought, is a result of a similar process.

How do we perceive the world? Categorize it from its multiply overlapping and fuzzily bounded phenomena? How do we remenber, generalize, learn? Edelman has an answer in outline. Groups of hundreds or thousands of strongly interconnected neurons form as a result of sorting the impulses from stimuli and strengthening the connections used. This partly surprising claim is supported by an apparently suitable simulation, which is one of the most striking features of the book. Development isn't repeatable in detail among individuals because of chance stimulation of different neurons and groups; different connection patterns result randomly but each may function adequately. A group can even capture neurons from another group with changed usage. Categorization is basic; memory is recategorization, and "true" learning requires surprise. Some structure is fixed early, while external stimuli can organize "neotenous" tissue. The usual idea of information is inapplicable in detail to a system whose very structure changes with stimuli. The feedback of signals is important in the structuring; Edelman calls this "reentry" and it took a while before I figured out what he meant. The writing is moderately turgid, and he doesn't, of course, use "group selection" with its normal meaning.

Is he right? The theory is rich, with much input from both psychology and neurobiology, but it certainly isn't a necessary consequence of what we know. I would like it, or something like it, to be right, as it fits my own way of looking at the subject, but that's no evidence. I think it does work, though. Whether appropriate neuronal groups exist and behave properly for the theory is something we don't know yet. The theory does have some apparent major consequences which Edelman doesn't mention. One is our overriding tendency to impose sharp boundaries on a fuzzy world (or was that merely part of the motivation for the theory?) Another is a mechanistic explanation for behavioral and mental variation among people; studies of identical twins (or armadillo quadruplets, etc.) may be interesting from Edelman's perspective. His use of cell-adhesion molecules as a primary explanation for most aspects of development doesn't, though, seem eclectic enough. The theory is vertebrate-centered; its generality and the evolution of its mechanisms, if they are real, need future consideration. How does quasi-learning by a ciliate relate to the theory?

There have been other theories of learning and the like based on selection, and Edelman is less than generous in his discussion of these, calling Hebb's basically similar theory unrelated and not mentioning others. He does add a lot, and what he adds is important, but others really were in the ballpark long ago.

The fallacy of supporting science merely for short-term goals is nowhere more evident than in the study of natural selection. Would Darwin, or Wallace, have been supported by organizations interested in how we think, or how we fight disease, or how cancers spread, or the many other ramifications the concept has had (even in engineering)? I mean the question to be a serious one and worthy of some thought.

-LMV

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Plant Strategies and the Dynamics and Structure of Plant Communities.
David Tilman. 1988. Princeton Univ. Press. xi + 360 pp. ISBN 0-691-08488-2 hardbound, $45.00. ISBN 0-691-08489-0 softbound, $15.95.

Tilman has written an illuminating book, one of the most so in recent memory. His approach is reductionistic, the reduction of plant competition to its underlying mechanisms. This is done with due regard for environmental differences, and the implications for communities are a central focus. Thus the reductionism is in method rather than in explanandum, a physiological approach to real ecology rather than the common "physiological ecology" that deserves its more recent sobriquet of environmental physiology.

The basic idea is that plants should optimally use resources at levels proportional to those in which they limit the growth of the individual plant. (Tilman does note that evidence is inconclusive as to the factual question of the extent to which this apparent optimum is approximated. He does not discuss conditions under which it may be violated; I don't know whether there are such or whether apparent violations are caused by different time scales between natural selection and human measurement.) Obviously we have here a selective vector for plasticity, and there are others not discussed, but Tilman emphasizes that the scope for plasticity is severely limited by conflicting adaptations.

The same unit of a resource can't be used in different parts of a plant simultaneously. In particular, growth for the acquisition of light competes with growth for the acquisition of soil resources. Recall that the unit considered is an individual plant; a plant in an unfavorable site will be poorer overall, but this is irrelevant to the argument. Within light-gathering growth, photosynthetic structures compete with supportive ones, and so on. (In the latter case the resolution often depends on the time scale of the energetic payoff; a tree seedling needs to be woody only as preparation for a later and taller life, not for its immediate advantage.) Therefore different strategies are possible, and a main aim of the book is to show how they are selectively useful.

Tilman emphasizes two sets of environmental parameters, which he regards as the most important influences on resource availability in terrestrial habitats. These are the rate of release into the soil of the limiting resource(s), and the rate of loss of plant material, including the effects of herbivory and of disturbance and other causes of mortality at all stages. He ignores the effect of deciduous plant parts, of which roots contribute the most, but the omission may not materially affect his argument. Other simplifications are deliberate and appropriate for a manageable theory. An assumption that all causes of loss are density-independent is meant as realistic and therefore is startling even for plants, but again its effect is probably negligible for the most part in this context.

These two parameters vary widely among habitats (also within very local patches, but the maintenance of diversity is only a subsidiary theme). Tilman argues that low loss should be (and is) associated with low soil-resource levels and low light at ground level, while a low supply rate should be (and is) correlated with low nutrients and high light penetration. The parameters obviously form gradients in phase space and sometimes even over continuous stretches of land. Different morphological, life-historical, and physiological traits are inferred to be optimal for different parts of these gradients, although despite consederation of interaction of the gradients themselves the effect of the imperfect correlations of the components of the gradients isn't adequately dealt with. Nevertheless several classes of such gradients in nature, e.g. of altitude or nitrogen supply, support the theory well. This is the case at scales from demes (which Tilman doesn't discuss except for plasticity but for which there are some nice examples) to wide latitudinal comparisons.

That short-term responses often differ from long-term ones should be obvious from a glance at any succession, but apparently the point is often ignored and

Tilman refutes it at length. He does make the interesting inference that secondary succession should be from species which are equilibrally better under a high loss rate, in the same quality soil, to those which dominate low-loss environments. Low but rapidly growing plants are competitively optimal (within a generation, excluding effects of longer-term competition by differential dispersal) when much light is available. An extensive discussion of Tilman's own experimental work on a Minnesota sandplain confirms various parts of the theory. However, it is sometimes relevant that the time scale of succession can overlap those of climatic change and of evolution, and even of soil maturation (the latter being 10^3-10^4 years). I don't know the effect of these variables, but an adequate consideration of levels of competition (e.g. within a patch and regionally) demands an explicit consideration of the time scales involved.

Tilman actually says that questions not addressed by his approach or its apparent extensions are thereby "of trivial importance". I hope that such intellectual chauvinism doesn't repel readers from the theory itself, on the one hand, or make them as blind as the author to the usefulness and validity, in their own context, of other approaches and questions in ecology.

There are multicellular somatogens other than plants. The theory doesn't apply to them, nor is it apparent that modifications would help. A reef coral or a fungus needs more than one resource, but the resources don't interact in the same way as in a plant. Thus if there is to be a theory generally applicable even to Tilman's questions and somatogens it may have to be hierarchically structured rather than generalized. Or perhaps there can be no such theory below the level of energy control.

-LMV

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On Optimization

Physiological Ecology of Animals, An Evolutionary Approach.
R.M. Sibly and **P. Calow**. 1986. Blackwell Scientific. ix + 179 pp. ISBN 0-632-01494-6 hardbound, $58.50; ISBN 0-632-01495-4 softbound, $29.50.

Ignore the title; the book (a good one) is really about an optimality approach to whole-animal physiology. This approach has had its share of attacks recently, but the attacks seem to me largely ill-directed. After all, natural selection at at any particular level and time-scale does give a hill-climbing mechanism for fitness under constraints (including those of demography and the gene pool as well as others more commonly cited) applicable at the time. A suitable analysis has to be sophisticated enough to include interactions such as trade-offs, but that is not a criticism of most real work, including this book. A difficulty which the approach has not yet considered concerns the nature of the individuals involved. The existing theory is for gonogens, but many animals are somatogens. Their incorporation won't require a basically new theory, but their strategies do differ in many ways from those of gonogens. And optimizers would do well to take a much closer and harder look at exactly what it is that is being optimized or maximized. Calling it fitness and defining it as the rate of population increase doesn't always work. Treasure your exceptions; they show other paths which may even permit generality.

-LMV

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Organization of Communities Past and Present.
Edited by **J.H.R. Gee** and **P.S. Giller**. 1987. Blackwell Scientific. xii + 576 pp.
ISBN 0-632-01783-X hardbound, $125.00. ISBN 0-632-02143-8 softbound, $49.50.

How often, how much, and under what circumstances do communities have empty niches (in the Grinnell-Hutchinson sense of parts of ecospace)? Why aren't they filled? Such questions have recently become prominent and are a major focus of this book. Some causes are the following: Available species may simply not be adapted to occupy the vacancies. Availability itself may be determined by historical accidents such as nature of preadaptation or occurrence on a drifting island, by catastrophe, or by vagaries in recruitment. Recovery from disturbance is not instantaneous, and disturbances may be frequent enough to make recovery (succession) the norm. Even predation can be such a disturbance. Individuals may be so clumped that part of the niche is effectively vacant.

A seriously misleading result of considering this general problem has been its incorporation into the framework of a dichotomy or continuum between equilibrial and nonequilibrial communities. The latter are supposed to be unorganized, with species presence and densities fluctuating randomly and unaffected by others. I know of no such communities, nor any which approach this state, and the concluding review by the editors agrees despite using the misleading terminology. This paper is probably the best existing discussion of the subject; Southwood has a rather different approach in a characteristically perceptive introductory chapter. (The other papers deal with a suitable balance of topics and range from provocative to superficial; one author even stands amazed at the existence of sib species.) The distinction the editors make is between communities which are "accommodated to the prevailing environmental conditions" and those which "are responding to conditions at some time in the past". Predictability need not be involved, although they think it is and perhaps it usually is, because of multiple stable states, dependence on initial state, variable thresholds, and chaos. They indicate how all communities have an organization.

They emphasize in an appropriate way the great importance of spatiotemporal scale in affecting our perception of equilibrium, but they don't go quite far enough. They regard "communities in patchy, ephemeral resource habitats" as being completely "nonequilibrial". However, such communities are parts of spatiotemporally larger metacommunities, which is why the biota of one cow patty or bromeliad or streambank slump resembles that of another. It is on this larger scale that regulation and evolution of these communities occur. Two central questions which it is useful to keep in mind with respect to all communities: what regulates species densities, and what regulates community structure? (I indeed assume that regulation occurs, because pattern is rarely random, but it may be only occasional or occur at a level different from what is being examined.) The editors also regard marine phytoplankton as the best example of a community with minimal biotic interactions. Maybe it's the best, but it isn't a good example at all. The classic Redfield ratios (approximately constant proportions of available nitrogen, phosphorus, and carbon) require large-scale competition for their control, as does the concentration of silicon. Recent work on energy flow emphasizes interactions even within the very diverse sorts and spatiotemporal scales of patchiness which help coexistence. Phenomena like the excretion of a large proportion of photosynthate by many phytoplankton have consequences which are only beginning to be explored. That a community is greatly affected by physical changes doesn't mean that its biotic interactions are negligible; in this case they even cause some of those changes.

-LMV

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Cognitive Evolution

Evolution and Learning.
Edited by **Robert C. Bolles** and **Michael D. Beecher**. 1988. Lawrence Erlbaum Associates. x + 263 pp. ISBN 0-89859-542-8. Hardbound. $34.95.

As an outsider, I wonder if anyone has made a psychological study of psychology in relation to the history of the study of learning. The tabula rasa is gone, but it need never have been proposed. As pointed out in this volume, there was more or less adequate evidence against it (provided partly by Darwin and Wallace) well before its advent.

The adaptive history of an organism is related to its current learning ability in several ways: by affecting the subjects of learning programs or objectives of learning, by imposing constraints on ease of learning, and by weighting of stimuli under different circumstances. Some birds learn their songs and others have them preformed (with a suggestion of phylogenetic determination here, not to mention larger phylogenetic differences, as with insects), some parts of some or all brains are specialized for particular subjects, white rats are more variable in maze-running when the reward is food than when it is water, and different natural responses can be easier to learn under situations of reward vs. punishment. There are various kinds of errors; the existence of mimicry depends on one kind. And, although not noted here, in a sufficiently unpredictable environment (relative to the organism) learning itself can be disadvantageous. There are costs not merely in making a wrong choice but in growing and maintaining, over a life and over evolutionary time, the apparatus needed for learning.

However, such adaptive or sometimes nonadaptive differences in learning don't imply that the topological structure of learning is similarly variable. This is a quasi-independent subject mostly outside the scope of the book. The book does include presentation of some points well known to evolutionary biologists but presumably not to psychologists. And some real biology still needs assimilation. Bolles, e.g., thinks that the ease of conditioned food-aversion learning by cattle (a word which, incidentally, has no general singular in English) is anomalous because cattle graze indiscriminantly. Maybe they do, but their wild relatives are choosy enough to segregate their diets when food is scarce, at least in East Africa; a white rat or a steer is a pretty artificial animal and we learn from its learning at our own risk. -LMV

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Recurrence and History

Time's Arrow, Time's Cycle: Myth and Metaphor in the Discovery of Geological Time.
Stephen Jay Gould. 1987. Harvard Univ. Press. xiii + 222 pp. ISBN 0-674-89198-8. Hardbound. $17.50.

A new book by Gould is an event (I almost said a punctuation). We see the world a bit differently afterward.

Blood cycles in our body, lives cycle, weeks cycle, lemmings cycle, water cycles in the Earth, all in different ways but with a predominant regularity. Gould follows a tradition I hadn't known of in this context and incorporates with these another sort of regularity, i.e. natural laws and the like, which similarly act recurrently if not regularly. The resulting edifice of recurrence, misleadingly called cycles, is what is contrasted with directionality or history.

Arrow and cycle are the metaphors of the subtitle; the main myth is the belief in purely rational progress of knowledge. (It is possible and common to err in the other direction. Gould doesn't do that here, but I note one ploy which I haven't seen criticized: A and B actually represent largely independent traditions deriving from C, with A earlier than B; the claim then is that there is a sequence from C to

A to B and that the underivability of B from A represents a paradigm shift or is caused by cultural change.)

The book itself is mostly an exegesis (and the Bible is part of the context) of the main books of Thomas Burnet, James Hutton, and Charles Lyell, reinterpreted in terms of the partial dichotomy of history and recurrence. They were all major actors in the discovery of deep time, but none viewed it as we do now. Each had as a basic underpinning a view of the world as, in one way or another. a steady state, now and for long ages. Such a view is compatible with directionality (picture a multiple sine wave with a nonhorizontal axis). However, "if moments have no distinction, then they have no interest" (p. 80). Gould interprets Hutton as being completely an advocate of recurrence, but to me Hutton's own evidence and causal arguments for this entail historicity of details, a second-order directionality superposed on the primary recurrence. As Gould does emphasize, Lyell also had this viewpoint, although from a different perspective.

Gould is one of the main advocates of the importance of natural selection at and above the species level in causing changes in biotas over geological time. I therefore find it puzzling that he misses Lyell's clear and extended statement of this view, before Darwin and probably making Darwin's development of his own theory easier. (I hope to expand on Lyell's views and their significance elsewhere.) Metaphors grade too easily into the sterility of symbolism, but it is interesting that the photograph of a remarkable sculpture on p. 180, shown mainly for its symmetry, is cut off so that the figure itself is asymmetrical. -LMV

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Mayr, Conceptual Analysis, and Intellectual Pollution

Toward a New Philosophy of Biology: Observations of an Evolutionist. **Ernst Mayr**. 1988. Harvard Univ. Press. [Pagination unknown; review copy ends with p. 524, before the last essay.] ISBN 0-674-89665-3. $35.00.

Mayr does philosophy too, competently. All but about four of the 28 essays in this rather heterogeneous collection had been published previously, all after 1960. I had seen the large majority before and must say that I found nothing new in the book except some Mayrology. The essays are reprinted as originally published (some are condensed) but some have brief notes relating them to later work. Each of the nine groups of essays has an introductory and mostly summary discussion.

Should you look at the book? Yes, if you don't know Mayr's conceptual work rather well. The topics extend well into biology, including essays on such subjects as species flocks of fishes and macroevolution, the latter (four overlapping essays) being confined to aspects of punctuation. All the essays have a conceptual orientation, which I suppose is what Mayr means by "philosophy", although there is an initial set of essays under that specific label, most of which deal with officially philosophical topics.

Mayr does often have interesting things to say, if by some chance you haven't run into them before. He has a strong tendency to clutter up the literature by saying the same thing again and again. That's proselytizing and advertising, not science, and editors of journals and books should keep it out more than they do. It is appropriate when there is a really different audience, as with a book like this, or when previous work needs to be reviewed. Otherwise it is a form of intellectual pollution. There is nevertheless less repetition in this book than I expected.

What we have here, for the most part, is a rather disjointed set of analyses of many of the major current problems of the evolutionary half of biology. Mayr's center is at the level of species, as it has been for decades, and almost half the book focuses on a variety of conceptual and empirical problems related to species. The rest deals with adaptation, natural selection, systematics, the value of diversity, extraterrestrial life, philosophy <u>sensu stricto</u>, and history (especially

Darwin). Mayr presents and defends his views well, but often, perhaps usually, he doesn't understand differing views well enough to attack them adequately. This is a shame, because with Mayr's intellectual power a genuinely informed criticism would be of some general interest.

There is a wide and deep understanding in the book, much meat for a feast and some fat for the birds. -LMV

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Shelled Cephalopods Come Alive

The Natural History of Nautilus.
Peter Douglas Ward. 1987. Allen & Unwin. xiv + 267 pp. ISBN 0-04-500036-0. Hardbound. $35.00.

Nautilus is an animal, not an art object, although its shell is deservedly admired. It has its own problems. Its growth rate depends on the rate at which it can remove water from its latest chamber, the water being needed until the septum is strong enough to withstand the pressure from outside, and this is a slow process. The removal even reverses below about 300 m, the chambers slowly flooding, so although much feeding occurs at these depths growth requires much more time closer to the surface. Not too close, though, because of dangers such as high temperature and more predators. Nautilus therefore frequents the edge of coral reefs, which have steep slopes as well as high productivity in the marine desert. The water in a chamber is hypotonic to sea water and therefore to the blood; it is removed merely by osmotic diffusion into the siphuncle, partly via a hygroscopic lining of the chamber. A balance normally exists between this osmotic pressure and the external hydrostatic pressure, with regulation of the amount of water (and therefore of the animal's total density and buoyancy) being largely by control of the rate of secretion into the chambers. Is this in turn determined by the amount of blood permitted into the siphuncle, or a variable permeability, and regulated by fine sensing of the air pressure in the chambers? The air itself occupies most of each completed chamber under all conditions; it is perhaps always at less than atmospheric pressure and seems to form, oddly, by passive diffusion from the siphuncular tissue.

Unlike the rapid-growing and semelparous naked cephalopods, Nautilus is a classic K-selected animal. Most ammonoids probably were less so. They had much smaller, and therefore presumably many more, eggs. Their more complex sutures and septa permitted more water in the chamber lining and more rapid removal of it, and thus both faster growth (because thinner septa could occur) and more rapid control of buoyancy. Except for two groups partly convergent on nautiloids, ammonoids probably lived in shallower water with all its vicissitudes. Ward is a paleontologist but barely discusses the remarkable diversity of early Paleozoic nautiloids; it would perhaps be enlightening to interpret their radiation in this framework. -LMV

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The Beginning of the Age of Dinosaurs: Faunal Change Across the Triassic-Jurassic Boundary.
Edited by **Kevan Padian**. 1987 (stated 1986 in book). Cambridge Univ. Press. xii + 378 pp. ISBN 0-521-30328-1. Hardbound. $75.00.

Near the end of the Triassic, terrestrial vertebrate assemblages dominated by several groups of herbivores were replaced by assemblages progressively dominated by dinosaurs and their thecodont ancestors. The temporal correlation of the relevant strata presents difficulties which remain partly unresolved, but there is general agreement that the early Jurassic is better represented than used to be thought. The emphasis of this book is on the southwest United States, but other areas are also represented. Most of the papers are rather straightforward and useful treatments of specific groups, including three on footprints, which provide more evidence than one might think. Bonaparte gives a nice and original account of the evolution, phyletic and adaptive, of prosauropods into sauropods, a relationship which had been disputed. The best faunal treatment is by Olsen and Sues, who critically evaluate the stratigraphy as well as the animals. They find a rather large extinction at the very end of the Triassic, correlative with a large marine extinction and also with one of plants. An apparently comparable extinction a few million years earlier may be an artifact of relative knowledge of strata deposited at different times. It is also later than the closest plant extinction and, unlike the terminal-Triassic extinction, is accompanied by an increase in origination. Others conclude from poorer data that this (Carnian) extinction was noncompetitive, but all that is excluded is a prolonged competition. What causally happened at the end of the Triassic also remains to be determined, but there is now better evidence that something rather unusual did then. -LMV

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Forschungsergebnisse zu Grabungen in der Grube Messel bei Darmstadt.
Edited by **Stephan Schaal**. 1987. Forschungsinstitut Senckenberg, Senckenberganlage 25, D-6000 Frankfurt/M. 1, W. Germany (their Courier 91). 213 pp. + 2 folded charts. ISBN 3-924500-29-0. Softbound. DM 40.00 (about $21).

The middle-Eocene oil-shale basin of Messel is one of the premier paleontological sites of the world, and most results of its study are published in this series. Fifteen species of mammals have their gut contents analyzed here; an extraordinary result is the finding of only plant material in all five specimens of a pangolin, which morphologically is as anteater-like as its extant relatives. (Or, facilely, did they die because they couldn't find their social insects?) Another paper reviews all Paleogene bats for which there is good skeletal material, with new specimens and conclusions on relationships and flight. The first report on the insects notes the absence of any aquatic insects in this lake deposit but then figures a tube of a larval caddisfly. Other papers deal with a perch and its phylogeny, a new insectivoran, a new lemuroid primate, and the microstratigraphy of the basin. The latest word I have is that the entire basin is again set to be used as a giant garbage dump. -LMV

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The Evolution of Life.
Edited by **Linda Gamlin** and **Gail Vines**. 1987. London: Collins. 0 + 256 pp. ISBN 0-00-219837-1. Hardbound. L14.95 (about $27).

Making real biology palatable to someone with no background in science isn't easy. I think this book does it pretty well. What immediately catches the eye are large numbers of gorgeous colored photographs, suitably chosen for their biological as well as artistic interest. There are about 100 pages on the kinds of organisms and their evolution, with the rest on integrative topics like genetics or movement. What is included is mostly done well, with respect to accuracy, clarity, and skimming the cream of interesting tidbits. There aren't many phylogenies; those given are defensible except for the one for the mammals, which is perfectly awful both for relationships and for divergence times. But that's the only serious problem with the book; even the cladist who discusses classification is almost objective. -LMV

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Population Biology: The Evolution and Ecology of Populations.
Philip W. Hedrick. 1984. Jones and Bartlett. xi + 445 pp. ISBN 0-86720-043-X. Hardbound. $27.50.

Population biology is still a mixture, not a subject. Hedrick's book treats the genetic aspects in a fully appropriate way, with theory leading and many examples in boxes. In ecology, though, we know that populations really don't behave like the equations say they should, and we know more or less why they don't. Presenting the subject via the semi-quantitative, semi-deductive approach suitable for population genetics gives a severely misleading picture of the natural world. We need equations, but not as crutches or foundations here. The book isn't unusual in following this approach, and it is as realistic as could be expected while doing so, but it could be much better. -LMV

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Modern Aspects of Species.
Edited by **Kunio Iwatsuki, Peter H. Raven**, and **Walter J. Bock**. 1987 (copyright 1986). Univ. Tokyo Press (distributed in USA by Columbia Univ. Press). xvii + 240 pp. ISBN 4-13-068124-9 and 0-86008-414-0. Hardbound. $52.50.

A good title, but there isn't really much of interest in the book. Most of it consists of potboilers and considerations of single genera and species, the categories not being mutually exclusive. There is an original review of <u>Clarkia</u>, for which quite a bit is known, and Imai extends the prevalence of karyotypic fissioning from mammals to ants. Ehrendorfer gives an original review of chromosomal evolution among angiosperms, probably the most interesting paper here and not emphasizing the usual aspects. He even discusses a well-documented case of progressive phyletic decrease in DNA. -LMV

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The Evolution of Sex and Its Consequences.
Edited by **S.C. Stearns**. 1987. Birkhauser Verlag, 380 Green St., Cambridge,, Mass. 02139. 0 + 403 pp. ISBN 0-8176-1807-4. Hardbound. $59.50.

The study of the evolution of sex has itself evolved in the past decade or two from playing with a peripheral puzzle to a relatively prolific field of research. And deservedly so; sex is perhaps the only well-defined evolutionary problem where we are still groping to find the right questions. Even the nature of the problem is a result of relatively recent work. This book gives an advanced and sophisticated treatment of much of the field, considered broadly. Sex determination, sexual selection, mating types, anisogamy, allocation of resources between the sexes, and other matters receive due consideration as well as the usually central topic of why sex doesn't disappear. Much of the emphasis is on comparative and even experimental evidence which has been gathered on the several topics, although theory isn't neglected. (There is, e.g., a superlative chapter by Lewis on diverse kinds of costs and how they vary among organisms.) Stearns gives a provocative closing essay on the overproduction of zygotes in some organisms, which lets selection occur among them. (He does, though, in the introduction argue against group selection as a factor in the maintenance of sex shortly before he notes the greater probability of extinction of asexual clades, and Arnold seems to think that sexual selection is the only component of individual selection which can oppose others. But mostly the papers are well argued.) The book is a valuable state-of-the-art review which, one may hope, will speed its own obsolescence. Two questions, from that perspective, not asked: Is it because of environmental unpredictability that it is so rare for the mother to determine the sex of her offspring? Why does isogamy persist in the face of the apparent advantages for anisogamy? -LMV

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Genetics, Development, and Evolution.
Edited by **J.Perry Gustafson**, **G.Ledyard Stebbins**, and **Francisco J. Ayala**. 1986. Plenum Press. xii + 361 pp. ISBN 0-306-42268-9. Hardbound. $49.50.

Unlike various books with similar titles, this one makes some attempt to integrate the usually separately flowing streams of investigation rather than just letting them coexist. There are a number of useful reviews, from cell biology to paleontology, on disparate topics (so that the book itself, unlike many of its papers, is unintegrated), but the contribution by Stebbins stands out. A review of work on plants leads him to suggest that the components of cell membranes and the cytoskeleton, and therefore the genes controlling them, mostly determine morphogenesis. Other organisms seem to follow the same rule. He does add growth regulators, but this category is clearly important for pattern and needs more emphasis and dissection. Normal enzymes, cell housekeepers, though, are excluded to the (large) extent that they don't contribute to the categories specified. Thus the large amount of work on isozymes would be rather peripheral to real evolutionary processes. Perhaps, as he suggests, their evolution is more often neutral.

Not all papers reproduced well by the printing method used. -LMV

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Rates of Evolution.
Edited by **K.S.W. Campbell** and **M.F Day**. 1987. Allen & Unwin (8 Winchester Pl., Winchester, Mass. 01890). xx + 314 pp. ISBN 0-04-575030-0. Hardbound. $45.00.

There is excitement about in the study of evolutionary rates, but you won't find much of it here. About half the book is paleontological and half genetical (both terms used broadly). Baverstock and Adams find that the rate of chromosomal evolution in some groups of Australian mammals is unrelated to the rate of molecular evolution, and morphological evolution is decoupled from both, using other data. They think that molecular evolution is the most constant, but this common claim has never been adequately tested, as far as I know. Their data do suggest appreciable variation in rate of chromosomal evolution. Templeton finds that mitochondrial DNA is, as expected, more sensitive than nuclear DNA to drift. Otherwise the genetical papers are potboilers or trivia, except perhaps for Reanney's repeating his suggestions on mutation defenses. Can this really be a function of introns?

A couple of the paleontological papers are of more interest. Runnegar reviews patterns of evolution in the Mollusca, with a particularly good discussion of the significance of the bivalved gastropods. Campbell and Marshall do even better with early echinoderms. They use this group to evaluate hypotheses on causes of adaptive radiation. Several are rejected with the summary conclusion that "large morphological change and high rates of cladogenesis are not coupled in any sense." They conclude, faute de mieux, that "some unusual feature of the primitive genome" must be involved. However, a hypothesis they ignored (and which is difficult to evaluate from these data, but which is supported by mammals and other groups) is that crossing an initial threshold or other transition permits but does not require an immediate diversification; if the group diversifies at all it may be only after further evolution (or environmental change) which gives an appropriate context. Genomic constraints in this context have an odor of magic. The authors also conclude from an explicit character analysis (but I can't evaluate how biased it may be) that almost all major morphological innovations occurred before the end of the Ordovician. They further disagree with Paul by concluding that the classes do not detectably converge toward each other near their earliest known records. Possibly the disagreement comes from evaluating the characters adaptively, which an orthodox cladist like Paul finds irrelevant; I await the sequel with interest. -LMV

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Oxford Surveys in Evolutionary Biology.
Oxford Univ. Press. Hardbound.
Volume 3 (edited by R. Dawkins and M. Ridley). 1987 (stated 1986 in book). (vi) + 254 pp. ISBN 0-19-854199-6. $60.00.
Volume 4 (edited by Paul H. Harvey and Linda Partridge). 1988 (stated 1987 in book). (vi) + 271 pp. ISBN 0-19-854230-5. $60.00.

The function of this series seems to be to provide insightful reviews which would not otherwise appear, because they are presumably solicited. I am not persuaded that a series like this is the best way to proceed, except for the financial condition of the publisher. Some framework, though, is desirable; the series does fill a genuine gap.

Despite the title of the series its scope, at least in the volumes under review, is narrow, The volumes include nothing outside population biology except perhaps for a pedestrian review of the biogeography of an interspecific association and a good but too reductionist (and occasionally obfuscatory) defense of neoDarwinism against recent challenges by some paleontologists. There really is a bit more to the evolutionary half of biology, including entire ways of looking at it; even in population biology the current orthodoxies are given exclusive benefit. Not all advocates of heresies are incompetent, and I would think that their views might sometimes be given a place.

That isn't to say that the reviews aren't worth looking at. Almost all of them are, and almost all are largely original discussions, even Maynard Smith's analytical appreciation of Haldane. Molecular approaches receive due coverage; that the focus is exclusively on animals may possibly be a sampling artifact, but again one that could be remedied. Possibly the most original idea is buried in a convoluted discussion mostly of the obvious by Cloak. He concludes that human altruism can be toward a system which I would generalize a bit as an ideology, which has an existence of its own and which can motivate self-denying behavior by as many as all groups affected. The ideology is, in Cloak's approach, itself real and parasitic. Whether his view is correct is a matter on which I have no opinion. Other papers have probably more insights per <u>n</u> pages than one could reasonably expect.

The last paper in Vol. 3 was partly garbled by the publisher. -LMV

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The Evolution of Sex: An Examination of Current Ideas.
Edited by **Richard E. Michod** and **Bruce R. Levin**. 1988. Sinauer. viii + 342 pp. ISBN 0-87893-458-8 hardbound, $55.00. ISBN 0-87893-459-6 softbound, $29.95.

What we usually mean these days by the evolution of sex is the problem of why sexual reproduction predominates in the face of a usually large advantage for cloning. There are of course other aspects to the subject, but this one remains one of the major mysteries of biology. It has stimulated several excellent books as well as much thought and recently even experimentation. The latter aspect is pretty well ignored here, which is a shame; even data surveys get little emphasis. We need more ugly facts, and some people are getting them. That isn't to say that theory itself is in fine shape — there are several theories available, not all mutually exclusive although probably not all important or even correct, but most aren't generally applicable. Quasi-truncation selection (by a disproportionate decrease in fitness with the addition of deleterious mutations) is and may prove to be a unifying theory for some other phenomena also, as the late Jack Lester King advocated in an unpublished lecture, but we don't yet know how universal its assumptions are in the real world. It is entirely possible that everybody is barking in the wrong forest. The book nevertheless gives excellent treatments of most current theories, including critiques of some, and is now the place to start if one wants to look at the subject seriously. -LMV

The Ecology of Sex.
Paul J. Greenwood and **Jonathan Adams**. 1987. Edward Arnold (3 E. Read St., Baltimore, Md. 21202). vi + 74 pp. ISBN 0-7131-2934-4. Softbound. $11.95.

The closest thing to pornography here is an inaccurate drawing of the alga Spirogyra mating. The book is about evolutionary strategies for eukaryotes: why sex, how sex is determined, what sex to be and how many to produce of each, sexual dimorphism, and mating systems. A rather unified bag of topics which are, however, very mixed in their difficulty. The most difficult, the first, oddly receives the least coverage. Overall the treatment is competent and clear, with an emphasis on natural history enough to give artistic verisimilitude to otherwise bald and sometimes unconvincing theory. But the latter problem isn't the fault of the authors. -LMV

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Genetics, Paleontology, and Macroevolution.
Jeffrey Levinton. 1988. Cambridge Univ. Press. xiv + 637 pp. ISBN 0-521-24933-3. Hardbound. $37.50.

It's been 35 years since Simpson's **Major Features of Evolution** and a bit has happened since then. There are now two later books on the general subject, Stanley's **Macroevolution** and Levinton's. Stanley's book is incisive, selective, lively, dense with examples, and much too often dead wrong even apart from matters of legitimate controversy. Levinton's book is fuzzy, comprehensive, rather boring, but not often clearly wrong. It's a reasonable place to go for an introduction to the literature, and it contains enough background material of diverse kinds to make itself rather generally accessible. The discussion of development is particularly good. But the book is superficial. I don't mean just that there is very little here that is new — syntheses don't need to be innovative. A good synthesis, though, needs to be based on a deep understanding of its subject, and that is what is lacking here for a large proportion of the subjects discussed. I happen to agree with Levinton's positions in most controversies, but I could usually rebut the arguments he gives for his views. And the book is poorly written, even defining macroevolution in a way that seems to mean all evolutionary processes. I'll still have to use **Major Features** (supplemented, of course) in teaching.

The print is so light that it somewhat distracts from the reading, at least in the review copy. -LMV

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Evolutionary Biology. Edition 2.
Douglas J. Futuyma. 1986. Sunderland, Mass: Sinauer Associates. xii + 600 pp. ISBN 0-87893-188-0. Hardbound. $32.50.

Futuyma or Grant? These books are the contenders for a serious course in the mechanisms of evolution. Both have advantages, but I'm reviewing only Futuyma's book here. He knows what he's writing about, usually, and presents the material clearly. The coverage is inclusive; anyone can fault emphases but I found no serious lack except possibly in the perfunctory coverage of adaptive strategies. A major emphasis on genetics reflects current opinion (not mine), and the topics covered as well as examples are strongly oriented toward animals. Creationism comes in appropriately under history; nevertheless some explicit consideration of why we came to regard evolution as true could well replace some of the conventional who-did-what-when treatment.

There is an explicit and largely successful effort to present real controversies fairly. Not that Futuyma doesn't give his opinion here and there, but at least some indication of the nature of the opposition is usually present. The same attitude seems to be involved in the choice of topics themselves. People study evolution in very different ways, and most have a hearing here. The result bears some resemblance to an integrated encyclopedia, but we do need books like that.

Major additions from the first edition are chapters on (mostly) the estimation of phylogenies and on molecular evolution. These are, as usual, model treatments. Saying things like this doesn't mean that I normally agree with what Futuyma says; in fact I don't, more often than I expected. This is most notable in the glossary; the definition of the central concept of natural selection is especially weak as well as convoluted. But I don't think I could have written a better book on the subject, at least without building on what he and others have done. -LMV

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Cladistic Theory and Methodology.
Edited by **Thomas Duncan** and **Tod F. Stuessy**. 1985. Van Nostrand Reinhold. xiii + 399 pp. ISBN 0-442-21845-1. $47.50.

The great achievement of cladistics has been to make the estimation of phylogenies intellectually respectable, accompanied by continued methodological advances. These methods are the main focus of this Benchmark collection. A cladogram is a phylogeny without ancestors, and the methods given here unfortunately ignore ancestors. Otherwise the selection is a good one and can be useful to the uninitiated. The main methods are presented, including statistical ones, and there are both foundational papers and critiques of the classificatory excesses usually practiced by cladists; the views of the latter are here too. There isn't much of the common dogmatic and even anti-intellectual fervor which has characterized much of the original literature; I like the way the editors put the book together. -LMV

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Biology and Management of the Cervidae.
Edited by **Christen M. Wemmer**. 1987. Smithsonian Institution Press. xiii + 577 pp. ISBN 0-87474-980-8 hardbound, $40.00. ISBN 0-87474-981-6 softbound, $29.95.

A long-delayed symposium volume that was worth waiting for, for once. It's superb, mostly. About half is a series of critical reviews, most with an appreciable amount of original material or interpretation, of diverse aspects of natural history and evolution. The title really should have substituted "deer" for "Cervidae", not only for (English-reading) librarians and other non-mammalogists but because the former inclusive family is now, for good phylogenetic and adaptive reasons, broken up into three families of extant species, each with diverse associated fossils which have also been reinterpreted. (I note that the authors of the chapter on ruminant phylogeny have, while this book was in process, published a longer and somewhat different version in Bull. Amer. Mus. Nat. Hist.) One of the groupings used in a chapter on brain allometry is artificial, partly affecting the conclusions. Another chapter shows that heterozygosity affects several aspects of reproduction and the authors interpret it in terms of different strategies, but this would seem hard to evolve and the differences seem more likely to be byproducts of degree of inbreeding. Most of the conclusions in this part of the book look well supported, though. If you haven't seen Geist's provocative treatment of adaptive divergence by social signals, look at its application here. The rest of the book consists of reports of field studies on quite a number of mostly poorly known species, and discussions of various aspects of management in zoos and in the wild. Even primates don't get this good a book. It will be the standard for quite a while. -LMV

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The Cactus Primer
Arthur C. Gibson and **Park S. Nobel**. 1986. Harvard Univ. Press. ix + 286 pp. ISBN 0-674-08990-1. Hardbound. $39.95.

Remarkably enough, this is the first book on the general biology of the cacti. It doesn't cover everything (most aspects of ecology are omitted) but what it does emphasize are the ways most cacti differ from most other plants. Chapters deal with, e.g., succulence, gas exchange, areoles and spines, growth habits, and special chemicals. These fully integrate the structural and physiological aspects of the several topics into a picture of the adaptive unity and variation of the group. Cacti are of course varied, and this is duly treated in the chapters on aspects of adaptation. Some cacti can tolerate tissue temperatures of 69°C; why their proteins don't cook isn't known yet. Adaptations to drought are well covered, but in keeping with the non-ecological approach there is no discussion of why cacti don't do well where it's wetter. Phylogeny receives a good emphasis, though, both within the family and in determining its relatives, and there is even most of a chapter on the largely primitive and even partly nonsucculent genus Pereskia. Nevertheless, there is no overall attempt to integrate the phylogeny with the adaptive diversity to produce a picture of the adaptive evolution of the group. There isn't even a clear statement as to how the family differs from its immediate relatives. But I like the book; what it does it does quite well, and its coverage is ample. -LMV

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The Natural History of Whales and Dolphins.
Peter G.H. Evans. 1987. Facts on File. xvi + 343 pp. + 8 colored plates. ISBN 0-8160-1732-8. Hardbound. $21.95.

Quite a good book. It gives a comprehensive coverage of the living members of the order and a brief overview of the fossils. An illustrated survey of the extant species is followed by aspects of distribution, diet, feeding methods, social behavior, life history, and whaling. These are uniformly well done. I was surprised not to find the argument by economists for hunting to extinction, but the case against whaling is put strongly, including an argument from inflicted pain. Anatomy and physiology have a sparser treatment; e.g., although sounds and their social functions (but not their controversial use for stunning prey) have extensive coverage, there is no mention of the remarkable adaptations of the ears of whales to receive sound underwater. But the book is authoritative for what it does cover, which is most aspects of general interest.

The unusually rigid spine of the book caused the back cover of the review copy to pull away from the interior -LMV

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Atlas of Dinoflagellates.
John D, Dodge. 1986 (stated 1985 in book). Blackwell Scientific. vii + 119 pp. ISBN 0-86542-317-2. Hardbound. $35.00.

After a brief introduction to dinoflagellates the book consists of scanning electron micrographs of the skeleton of 110 extant species which have one (most dinoflagellates don't) and of the cysts of a moderate proportion of those figured. Each species is accompanied by a brief description, including size (apparently of the individual figured) and a locality which shouldn't be confused with the species's range. The micrographs are excellent although, as Dodge notes, they shouldn't be used alone for identification because not all species with skeletons are here. -LMV

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Dinosaurs Past and Present.
Edited by **Sylvia J. Czerkas** and **Everett C. Olson**. 1987-1988. Natural History Museum of Los Angeles County and Univ. Washington Press. 2 volumes. Vol. 1 (1987), xvi + 161 pp. ISBN 0-295-96541-X hardbound, $35.00. ISBN 0-938644-24-6 softbound, $22.95. Vol. 2 (1988, stated 1987 in book), xiii + 149 pp. ISBN 0-295-96570-3 hardbound, $35.00. ISBN 0-938644-23-8 softbound, $22.95.

The use of dinosaurs as subjects for genuine art was, as far as I know, initiated by Eleanor Kish, whose magnificent paintings for the National Museum of Canada were published by them in 1977 in the book A Vanished World, with text by Dale Russell. Most are also included with Russell's paper in vol. 1 here. The volumes under review celebrate dinosaur art, as something beyond dinosaur illustration. There are 88 colored figures and 155 uncolored: paintings, drawings, and photographs of sculptures. They are well reproduced except for occasional problems with the transition between facing pages. Not many reach Kish's standard, but some do. Most of the work is recent and (I think) most has not previously been published, although some which I know have been are not referenced as such. There is also some representation of earlier work, especially by Charles Knight and Waterhouse Hawkins. A list of the art, officially 144 items, is keyed only from the figures to the list; not all can be found the other way around despite an index.

It helps to be a scientist too. as shown by Gregory Paul and his instructor Robert Bakker. Both are artists as well as scientists and both have chapters discussing dinosaurs as lively animals and problems caused by not looking closely enough at the evidence. (The artist who is an editor may want to look more closely also at the names of the dinosaurs she paints.) Most of the other chapters also are involved in one way or another with the process of going from specimens (including pterosaurs) to art, although some also present new science, especially Jack Horner's updating and expanding of his work on nest sites. Two chapters have no obvious relevance to art, one rather trivial one on adaptive differences of very small dinosaurs and a better one by J. Keith Rigby, Jr., which gives in considerable detail the nature of the evidence for survival of dinosaurs for a short time into the Paleocene in Montana, with related matters. Stephen Czerkas gives the reasons for his depiction of <u>Stegosaurus</u> (using a species which Bakker refers in passing to <u>Diracodon</u>) as having mostly a single, median row of plates. I'm not convinced. Its close relative <u>Kentrosaurus</u>, at least, has bilaterally asymmetrical plates, and even Czerkas has the caudal spines and the anterior plates bilateral. He does show, though, that there is no reason to insert extra plates, as has been done. -LMV

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Classification, Evolution, and Phylogeny of the Families of Dicotyledons.
Aaron Goldberg. 1986. Smithsonian Institution Press (their Contributions to Botany 58). iii + 314 pp. No ISBN. Softbound. Availability limited; free.

Most of this volume is an exceedingly valuable review of the families of dicots, with quite a bit of information packed into the third or half a page given to the text for each. There are in addition drawings of parts of one to several species for each family. The orders recognized have similar descriptions. In the introduction Goldberg gives a table of about a hundred characters with states segregated as primitive and derived. Many of these judgments are unexceptionable, but the polarities are hardly ever justified. He makes a strong point of wind pollination being primitive, merely because that is the case in most gymnosperms. Most gymnosperms are conifers, though, which have no close bearing on the matter. Also, and more to the point at least with a translation, "The fewer families showing a particular primitive state the more primitive the state." On grounds like these he gives a cladogram of the orders (none is derived from any other), without citing specific characters. Fortunately he says he plans to consider this more fully later. -LMV

The Biology of Terrestrial Isopods. (Symp. Zool. Soc. London 53.)
Edited by **S.L. Sutton** and **D.M. Holdich**. 1985 (stated 1984 in book). Oxford Univ. Press. xxvi + 518 pp. ISBN 0-19-854001-9. Hardbound. $55.00.

The editors made a serious mistake in arbitrarily omitting all papers on "systematics, evolution, and biogeography" from the symposium in favor of their own fields of physiology, anatomy, behavior, and ecology. Some of the resulting published papers are of marginal quality; a better collection could have resulted from culling by quality rather than by field. That's partly why we have editors.

The book itself has quite a lot of valuable material, though, from detailed experimental results to surveys of several kinds of adaptation. The group is less well known than amphibians but has more species and rivals the amphibians in degree of terrestrial adaptation; both groups occur even in deserts. Adaptations to a life on land do receive a prominent role in the book, but it includes many other aspects also, too diverse to summarize. I must mention one, though, showing a complex but adaptive relation of diet preference to the nature of chemical defenses by the plants eaten. -LMV

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Current Mammalogy. Volume 1.
Edited by **Hugh H. Genoways**. 1987. Plenum Press. xx + 519 pp. ISBN 0-306-42430-4. Hardbound. $75.00.

We already have Mammal Review. Why another? Well, Current Mammalogy is bigger, for one thing. More importantly, its editorial board solicits the reviews, so that much appears that probably wouldn't have at all if left to the primary initiative of the authors. I don't like the proliferation of journals without a compelling reason (libraries really can't cope with the cost), and I wouldn't call the reason here compelling. It could, after all, have been served as well, for more people, and more cheaply by, say, appropriately expanding the Journal of Mammalogy. A solution like that would have other problems, but they would probably be less severe for the science as a whole.

Given its existence, the annual series is starting off well. It has a wide range of topics, from behavioral ecology to pest control to paleontology to comparative histology (mislabeled as cellular evolution), and the papers range from competent to excellent. Baker et al.'s review of chromosomal evolution at the level of banding patterns is especially noteworthy, as is Negus and Berger's analysis of fixed or plastic responses in reproduction to environmental diversity. (They don't even mention Millar, though.) Barnosky reviews Pleistocene evidence for and against punctuation. Unfortunately he regards diverse criteria as characterizing a unified phenomenon of punctuation rather than emphasizing their frequent disassociation; he even accepts the common but entirely fallacious statement that lineage selection requires punctuation. From the evidence here and elsewhere, it looks somewhat as though the expansive phase of an adaptive radiation tends to be characterized by gradual change and the quasi-equilibrial phase by punctuation; there isn't enough information on the (initial) bloom and (final) declining phases. The existence and causes of such patterns need more consideration.

A surprising short paper by Temme shows that a class of morphological abnormalities in Rattus exulans is higher near the Eniwetak H-bomb site than elsewhere, but he doesn't refer to the extensive and mostly negative British study on a species of the same genus on the radioactive sands of Kerala, India. I hope that future volumes will contain some explicit systematics. This is absent here except for a short review of the evidence for a common origin for the Lagomorpha and Rodentia; this paper is marred by the false and important statement that primitive rodents as well as lagomorphs are characterized by some unilateral hypsodonty. Not Paramys and the like, and that does present something of a difficulty for the otherwise appealing hypothesis. -LMV

Morphogenesis of the Mammalian Skull.
Edited by **Hans-Jürg Kuhn** and **Ulrich Zeller**. 1987. Hamburg: Verlag Paul Parey. (Mammalia Depicta 13.) 0 + 144 pp. ISBN 3-490-17718-5. Softbound. DM88 (about $46).

The subject of this book has been moribund, if not quite at a standstill, for going on 50 years. De Beer published his treatise on it in 1937, but that probably wasn't the main deterrent. Treatises do sometimes stimulate work, and he hoped his would. Anatomy rather came to be in low esteem by people who didn't bother to understand its real nature and problems; the results are manifold, with creative input coming to a considerable extent from workers in peripheral subjects like paleontology and anthropology. Studies like those reported here involve extensive serial sectioning, a time-consuming procedure which requires a belief that the results will be worth the effort. I think they are, if directed properly, and there has recently been somewhat of a revival of the approach from a more integrated viewpoint. Work since de Beer has been mostly in continental Europe, and the literature is inaccessible to the fruits of American parochialism. This small book, though, is in English despite all but one author being German. And it provides an excellent scholarly introduction to the subject (at an advanced level) in addition to new results. The first paper notes, among other things, that there have been (rarely) evolutionary shifts in both directions between endochondral and membrane development of historically homologous bones. A paper by the editors possibly resolves the controversy on the nature of the monotreme alisphenoid, showing large differences between the two extant genera which seem to require the bone of the common ancestor to have been small. They infer that this was the case also for early therians, but a paper by Maier gives evidence for a continuously large lamina of this bone. A definitive answer will probably need more relevant fossils. The problem is related to the phyletic relationships of the monotremes, a matter which other evidence has done much to resolve in a way probably indicating a surprisingly close affinity between the two extant groups of mammals. -LMV

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Studies in Neotropical Mammalogy: Essays in Honor of Philip Hershkovitz.
Edited by **Bruce D. Patterson** and **Robert M. Timm**. 1987. Field Museum of Natural History (their Fieldiana: Zoology, n. ser., 39). vii + 506 pp. No ISBN. $35.00.

All on mammals, but quite a diversity: distribution, saliva, new species, tent-making by bats, parasitic mites, demography, chromosomal evolution, and other things. Hershkovitz is a distinguished systematist, and the most generally useful papers here are systematic. Pascual and Carlini describe a new superfamily, no less, of extinct rodent-like marsupials which lived contemporaneously with rodents. Reig makes a major revision of the akodontine rodents, with suggestive evidence for origination of the group in the South American Miocene instead of being a later immigrant derivative. The group is absent from known Miocene deposits, but it is predominantly an upland group and there are no small mammals known from the upland Miocene. Reig doesn't, though, deal with later North American fossils which have been proposed as ancestral to the whole group. Canids are more diverse in South America than anywhere else, and Berta reviews the phylogeny and other aspects of the evolution of this neglected group. And Patterson and Feigl conclude, surprisingly, that for Chilean mammals the number of specimens in the Field Museum in 1943 correlated reasonably well with the relative abundance of the species in nature. -LMV

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The Ancestry of the Vertebrates.
R.P.S. Jefferies. 1986. Cambridge Univ. Press. viii + 376 pp. ISBN 0-521-34266-X. Hardbound. $75.00.

In case you haven't run into Jefferies's thesis, which he has been expounding since 1967, it is (to simplify) that vertebrates are descendants of echinoderms. Not that the groups are collateral relatives, the usual view, but that our ancestors had a calcite skeleton around themselves like a sea urchin's, later lost and replaced by bone.

The thesis is indeed controversial. It depends critically on the correct identification of homologies of structures, including cranial nerves, in an early Paleozoic group of echinoderms. Jefferies has been in a definite minority among people competent to evaluate such things, which obviously doesn't mean he's wrong but which must make the rest of us a bit leery until matters get sorted out. Even the orientation of specimens isn't agreed on. The proposed sequence of branching is hemichordates-(extant echinoderms)-amphioxus-tunicates-vertebrates, with his carpoids (asymmetrical echinoderms) scattered between hemichordates and vertebrates.

A major consideration is the resulting lack of homology among the bilateral symmetries of the various non-echinoderm groups. Jefferies justifies this mostly by the partly asymmetrical development of amphioxus. However, here and elsewhere the asymmetries are related to bilaterally different developmental functions; this relation obviously needs evaluation, which it has not received. Despite a useful chapter rebutting other viewpoints, Jefferies also doesn't really consider most of the now rather extensive evidence for other sequences than that given above. I thus remain skeptical.

There is a lot in the book which will be of use well beyond consideration of the hypothesis. This is particularly true for the extensive comparative review of structure and development of protochordates and their relatives. The book itself is beautifully produced. -LMV

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On Evolution and Fossil Mammals.
Björn Kurtén. 1988. Columbia Univ. Press. xvii + 301 pp. ISBN 0-231-05868-3. $40.00.

The Synthesis lacked development and ecology. In his doctoral thesis, published in the same year as Simpson's Major Features, Kurtén successfully integrated quantitative aspects of both subjects into mammalian paleontology. Adequate preparation in systematics was then necessary for a paleontologist, although unfortunately that is less true today, and his later work expanded in that direction (mostly in the Pleistocene), with offshoots into areas such as paleobiogeography, evolutionary rates, and anthropology. Eleven of his papers in these several areas (but not systematics per se) are reprinted here, including his seminal thesis. Most are from journals with small circulation, and all remain of current interest. I wish his paper on evolutionary rates, in the 1959 Cold Spring Harbor Symposia, could have been added; recent work by others has come to similar conclusions on the dependence of estimated rate on the time interval sampled, without noting Kurtén's quarter-century priority. His conceptual advances are numerous and he is deservedly eminent. Although a Finn who has published in several languages, he writes English better than most Americans. I recommend also his novel of Neanderthal decline, Dance of the Tiger, for its anthropology as well as being a good read. -LMV

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Mythical and Fabulous Creatures: A Source Book and Research Guide.
Edited by **Malcolm Smith**. 1987. Greenwood Press. xlix + 393 pp. ISBN 0-313-24338-7. Hardbound. $49.95.

Most vampires weren't batty. Twenty categories of beasts of folklore receive extensive and scholarly discussion here, with due attention to literary and other uses of them and to phenotypic variation and natural origin. Some other beasts have cursory treatment in a final chapter, where the horns of a jackalope are confused with antlers. There are a few pictures. This is the book to start with; each chapter has a bibliography giving easy entry to previous literature. "To many persons, of course, devils and angels are in no sense 'imaginary' creatures. . ."
-LMV

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Mammals: Their Reproductive Biology and Population Ecology.
J.R. Flowerdew. 1987. Edward Arnold, 3 E. Read St., Baltimore, Md. 21202. vi + 241 pp. ISBN 0-7131-2896-8. Softbound. $27.50.

One of the subjects that population ecology grades into is reproductive biology, and it is appropriate to have a more or less unified treatment of them. Actually about half the book is devoted to each, but there is more integration than this indicates. The physiology and behavior of reproduction are both considered from an adaptive approach, and environmental influences of several kinds are dealt with. Here and elsewhere the treatment is comparative, although less attention is paid to the manifold effects of body size than I think is warranted: even its relation to density is omitted. This is possibly related to the omission of anything that smacks of communities, although predation and parasitism (<u>sensu lato</u>) do have to be brought in with respect to population regulation. The chapters on population ecology cover the expected topics; as with reproduction, the treatment is balanced and critical, with theory and diverse examples. A major fault which deserves correction: in the chapter on the reproductive tract (but not elsewhere) only placentals are mentioned. -LMV

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Mammals of Arizona.
Donald F. Hoffmeister. 1986. Univ. Arizona Press. xx + 602 pp. ISBN 0-8165-0873-9. Hardbound. $49.95.

As with any regional book on mammals, the centerpiece is the set of accounts of individual species. The treatment is scholarly throughout and is based on the author's own work over many years. For instance, he examined about 2000 adult specimens of the pocket gopher <u>Thomomys</u> <u>bottae</u>, from 606 Arizona localities, to recognize 14 subspecies in the state. This is not a result of oversplitting (Hoffmeister lumps subspecies here and elsewhere, and the species is variable), but in light of recent work such as that on <u>Peromyscus</u> <u>maniculatus</u> in one direction and on <u>T.</u> <u>bottae</u> itself in another we may expect appreciable change in knowledge in a few years if work continues as it has been. So for species like this which can still be studied the book is a benchmark. For each native taxon, where appropriate, there are complete lists of specimens used, maps, photographs (including habitat), occasional drawings, tables of measurements, plots of multivariate analyses, and discussion of taxonomy, characters, habitat, life history, variation, habits, and the like. A section on zoogeography includes an innovative (I think) table which gives the proportion of each species' localities which fall into each of a set of habitats. The book can serve as a model for similar accounts but will not be equalled easily..
-LMV

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Biology of the Reptilia. Volume 16. Ecology B: Defense and Life History.
Edited by **Carl Gans** and **Raymond B. Huey**. 1987 (stated 1988 in book). Alan R. Liss. xi + 659 pp. ISBN 0-8451-4402-2. Hardbound. $74.50.

This volume meets the sterling expectations that one has for the series. All the reviews are thorough, explicitly comparative, and well done. Not much new, even for viewpoints, but that's not a detriment. A chapter on methods for studying reptile populations may decrease the too-high frequency of work which doesn't really show what the author thinks it shows. It would have been useful to have expanded the account slightly to include criteria for recognizing diet selectivity, which is the topic which has perhaps the worst record. The authors also omit discussion of the frequently useful composite life tables. For the rest, Greene gives a book-length treatment of defenses, from which mimicry, tail autotomy, and parental care are excluded and given in other chapters. Pough's chapter on mimicry even includes mimicry of snakes by insects. (Incidentally, the problem of the mimicry of lethal coral snakes has been solved by empathic learning and a demonstrated inborn avoidance reaction by birds.) There are chapters on comparative life histories of turtles and squamates, and one on the environmental physiology of eggs and embryos which unfortunately doesn't extend its expertise to what little is known of dinosaur eggs. The squamate chapter is built around some multivariate analyses which are inherently confounded by phylogenetic groupings. The authors recognize the importance of phylogeny, and the traits they use probably do evolve rather rapidly (and thereby approximate a decoupling from phylogeny) in many or most cases, but one can't really take the results as more than suggestive. It may nevertheless be the best way to handle the complex data available until there are adequate phylogenies on which to map the evolution of life-history traits separately and in adaptive groups. -LMV

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Animals Wihout Backbones. Edition 3.
Ralph Buchsbaum, Mildred Buchsbaum, John Pearse, and **Vicki Pearse**. 1987. Univ. Chicago Press. x + 572 pp. ISBN 0-226-07873-6 hardbound, $25.00. ISBN 0-226-07874-4 softbound, $17.00.

The classic introduction to the spineless (well, some do have spines of another sort) has a new face and an updated interior. It treats the major living groups; others are touched on in an appendix, and a cursory chapter on fossils isn't quite as trustworthy as the rest. Its main asset is high readability and thus its being accessible to the uninitiated. The figures are good and the topics covered are suitable for the expected audience. A success.

The review copy has several pages at each end stuck together. -LMV

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Lecture Notes on Invertebrate Zoology. Edition 3.
M.S. Laverack and **J. Dando**. 1987. Blackwell Scientific. viii + 203 pp. ISBN 0-632-01461-X. Softbound. $26.95.

This is a handy little book for a once-over of the extant invertebrate phyla and classes, including protozoans. The major characters are given under several headings, with some indication of diversity, and well-chosen figures clarify the text. Phylogeny is almost ignored. What material is given is as up-to-date as could reasonably be expected, and there are mostly adequate references for deeper study. -LMV

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Plant Life in Aquatic and Amphibious Habitats.
Edited by **R.M.M. Crawford**. 1987. Blackwell Scientific. xii + 452 pp. (British Ecol. Soc., Spec. Publ. 5.) ISBN 0-632-01628-0. Hardbound. $110.00.

The title is misleadingly general, as the book is almost restricted to environmental physiology. Although there are the usual sorts of papers on physiology and other aspects of aquatic plants, which don't hang together and could best have been published in journals, the bulk of the book is concerned with the responses of amphibious and flooded plants to anoxia. The sediment and its interstitial water of the habitats of most aquatic plants is effectively anoxic as a result of the metabolism of the plants and of their decomposers. (A surprisingly large proportion of plant photosynthate is lost in the discarding of root parts, but the book simply assumes the anoxia is there rather than discussing its causes.) Amphibious plants, i.e. plants adapted to regular or irregular flooding, have various means of responding to the rapid change in oxygenation which occurs then. Other plants do less well when flooded. The book is frustrating to a nonspecialist in its near absence of comparative and integrative material. Plant x adapts like so, but what does this tell us about plant y? Maybe the authors have the background to know, if such comparisons can in fact be made, but by not having the authors tell the rest of us the editor has unnecessarily restricted the set of people who would benefit from the book. -LMV

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The Natural History of Squirrels.
John Gurnell. 1987. Facts on File. xiii + 201 pp. + 8 plates. ISBN 0-8160-1691-1. Hardbound. $21.95.

More particularly, nongliding tree squirrels of the Holarctic, quite a small subset but a good chunk for a book. Gurnell considers the ecology, behavior, and reproduction of several species in a comparative manner. The organization is by topics such as energetics or parasites, but often information is available for only one species. The main purpose anyway is the presentation of diverse sprts of information about squirrels rather than strict comparisons. And Gurnell does so; it's the book to come to for its subject. He seems a bit more at home with behavioral aspects than with strictly ecological ones, though; e.g., he implicitly assumes that population regulation is either continuous or absent, a fallacy in which he is not alone. -LMV

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Introduction to Phycology.
G. Robin South and **Alan Whittick**. 1987. Blackwell Scientific. viii + 341 pp. ISBN 0-632-01769-4 hardbound, $39.95; ISBN 0-632-01726-0 softbound,

A first-rate treatment. The book's organization is unusual, being primarily by subject rather than by taxon, and this is successful because of a thoroughly comparative treatment of each subject. The blue-green algae are appropriately included, as they have the algal adaptive facies. The book is quite up to date and almost as complete as one could expect. There is even a whole chapter on levels of organization of individuals. Good figures too. The one aspect nearly omitted is development (as distinct from description of life cycles). Interesting work has been done on such topics as the control of heterocyst differentiation in blue-greens, degree of nuclear control in the dasycladacean <u>Acetabularia</u>, and pattern formation in the single cell of the desmid <u>Micrasterias</u>, but it isn't in the book. Maybe next edition. -LMV

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The Evolution and Palaeobiology of Land Plants.
Barry A. Thomas and Robert A. Spicer. 1986. Dioscorides Press, 9999 SW Wilshire, Portland, Ore. 97225. ix + 309 pp. ISBN 0-931146-06-2 hardbound, $51.95; ISBN 0-931146-07-0 softbound, $28.95.

An intellectually interesting book skims the cream of its subject. This is (I think) the first book on paleobotany which tries to do so; the others, good in other ways, take the subject as defined and try to cram it into the reader. And the book does succeed to a reasonable extent. It restricts itself to vascular plants (no bryophytes) and includes a good section on angiosperms, unusual in a paleobotany book. The emphasis is appropriately on phylogeny and adaptation, with relevant evidence, and this is done well. There is room for less detail than in other books, even apart from this emphasis, and serious students will have to find such material elsewhere. (I hope they do so and don't think that the cream is all there is.) A chapter on evolutionary processes seems out of place in a book like this and could have been omitted in favor of such topics as paleoecology, patterns and causes of diversity changes, and the evolutionary history of floras and vegetational patterns. There is a bit of the latter, but only from the late Cretaceous. -LMV

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The Ultrastructure and Phylogeny of Insect Spermatozoa.
Barrie G.M. Jamieson. 1987. Cambridge Univ. Press. xvi + 320 pp. ISBN 0-521-34441-7. Hardbound. $54.50.

Spermatozoa evolve like most anything else, and one can therefore suspect that their structure will prove useful in estimation of phylogenies. Jamieson has collected a great amount of information here, not just on insects but on myriapods, onychophorans, and to some extent other groups. An overall assessment is that the cell is indeed useful but not obviously more so than other similarly complex structures. For instance, a phylogeny of insect orders from spermatozoal evidence reproduces some aspects of one derived from more complete evidence but also has groupings not otherwise supported. One can nevertheless superpose spermatozoan evolution on such more general phylogenies and have a reciprocal illumination, as Jamieson does. He seems to want to accept the Uniramia as holophyletic but the spermatozoa don't particularly agree; his discussion on arthropod monophyly considers diverse sorts of characters and authors and is worth looking at, even if he does think early development is an inviolate touchstone. Too bad we don't have spermatozoa from trilobites, which morphologically rather bridge the gaps among the three major extant groups of arthropods. -LMV

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A Guide to Post-cranial Bones of East African Mammals. Mrs Walker's Bone Book.
Rikki Walker. 1986 (1985 on title page). Norwich, England: Hylochoerus Press; distributed in USA by Blackwell Scientific. xi + 285 pp. ISBN 0-9511105-0-0 hardbound, $34.95; ISBN 0-9511105-1-9 softbound, $24.95.

The book is meant as an identification manual and it is admirably suited to that purpose. It can also function as a quick source of comparative drawings of the relevant bones. There are 71 mammals, mostly larger ones (only 4 rodents and no bats or insectivorans), plus a species each to represent turtles, lizards, crocodiles, and birds. The drawings are good and are actually drawn to be comparative. In a few cases not all related species receive individual drawings, but here and elsewhere each species has an approximate and visually defined measure of bone size given. Quite useful. -LMV

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The Origins of Angiosperms and Their Biological Consequences.
Edited by **Else Marie Friis**, **William G. Chaloner**, and **Peter R. Crane**. 1987.
Cambridge Univ. Press. x + 358 pp. ISBN 0-521-32357-6 hardbound, $59.50. ISBN 0-521-31173-X softbound, $22.50.

Paleobotany has long been on the evolutionary sidelines, but work like that presented in this book is making it be taken seriously.

No, nobody in the book seems to think that angiosperms are polyphyletic. In a chapter summarizing work elsewhere, Doyle and Donoghue do give an analysis of the phyletic interrelations of major groups of seed plants, including angiosperms. They do consider some alternatives to their preferred choice, but their evaluation is based primarily on the number of character changes needed. This of course assumes that the characters are independent and equally valuable; in their analysis all multistate characters are treated as two or even three characters, each of which is then apparently treated like other characters! Apart from bowing to the computters, though, the analysis is good and it will now be difficult to put angiosperms anywhere else.

After a review of relevant geography and climate, several papers give more or less high-level syntheses of floral and vegetational changes into the Cenozoic, with due attention to interactions with animals, dinosaurian and otherwise. There is even half a chapter on flowers themselves, on which surprisingly much has been found recently. A number of provocative suggestions are to be found in these papers, but the most exciting is one by Coe et 4 al. on dinosaur communities. Approximate but reasonable calculations suggest that dinosaurs in the late Jurassic had a biomass per hectare about 20 times that which mammals now have on the African savanna, and estimates of consumption suggest that they were overall not actively endothermic. Here in a book on paleobotany are the best original data on masses and relative abundances of late Jurassic dinosaurs.

Quite an impressive book. -LMV

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A Functional Biology of Marine Gastropods.
Roger N. Hughes. 1986. Johns Hopkins Univ. Press. (ix) + 245 pp. ISBN 0-8018-3339-6. Hardbound. $32.50.

This useful series of books aims to integrate adaptive physiology and ecology into an account of how the organisms concerned make their living, a microeconomic approach. Hughes is a physiologist and gives a nod here and there to ecology, even a whole chapter, but his heart isn't in it. He does, though, take an evolutionary orientation throughout: adaptive evolution, not phylogeny, which isn't used at all. The result is a competent and somewhat dull review of his group. There are lots of pictures and graphs, including a drawing of a remarkable endoparasitic worm, _Enteroxenos_, which shows itself to be a snail by having a normal veliger larva.

Whenever possible Hughes uses a cost-benefit approach, and the topics he considers seem to be partly related to their amenability to it. His currency is energy, as appropriate, but he thinks that the ratio of energy gain to energy cost is the relevant parameter. It doesn't matter for his comparisons, but in numerical (as distinct from ordinal) analyses one should really use the difference between those quantities. It sometimes makes a difference, as with endotherms.

Some copy-idiot thought the first sentence of each figure legend was a title and capitalized it accordingly. -LMV

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Evolutionary Biology of the Fungi.
Edited by **A.D.M. Rayner, C.M. Brasier**, and **David Moore**. 1987. Cambridge Univ. Press. xii + 465 pp. ISBN 0-521-33050-5. Hardbound. $80.00.

How polyphyletic are the fungi? Excluding the slime molds, which still receive a little coverage in this book, they may represent as few as two clades, and it seems adequately shown that the dominant fungi (to which the taxonomic name is restricted) are part of a single clade. The Fungi remain a large and diverse group if not trophically unique. If the book is a guide, phyletic aspects are more mature than most others of direct evolutionary relevance. Nevertheless, not all the authors dealing with it here make the phyletically critical distinction between primitive and derived character states. (Unrooted topologies don't necessarily need it, but no one here is making them.) The subject is, so to speak, yeasty, and yeasts are a focus of controversy here. Rather than representing simplification from many different lineages, there is now some support for the view that yeasts are the primitive form of "true" fungi. This is not, however, well argued; the multiple convergences required by each hypothesis (and the hypotheses are not mutually exclusive) have not been evaluated in a critical way.

Evolution is more than phylogeny. Brasier gives a fascinating account of the heterogeneity and rapid evolutionary flexibility of fungal species; it should, but probably won't, catch the imagination of workers on other groups also. Other authors pursue this general topic as well as reproduction and modes of life, and there are even a few brave attempts at interpreting inadequate data on the genome from an evolutionary perspective. This perspective is said to be unusual in mycology as a whole; perhaps the book will help to remedy that pathological state. One relatively mature area, mutualism, receives a masterly discussion from Lewis. Parts of the book really are for more people than mycologists. -LMV

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Five Kingdoms: An Illustrated Guide to the Phyla of Life on Earth. Edition 2.
Lynn Margulis and **Karlene V. Schwartz**. 1988. W.H. Freeman. xvi + 375 pp. ISBN 0-7167-1885-5 hardbound, $35.95. ISBN 0-7167-1912-6 softbound, $24.95.

This marvelous little book is the place to go to get an overview of the diversity of life on Earth now. The less-known phyla receive full coverage; Placozoa, with one species, has its two-page spread just as Mollusca has. Each of the 92 phyla recognized (and most knowledgable biologists will disagree somewhere) has one or more photographs, a stylized habitat sketch, a brief discussion of characters, adaptations, relationships, and fossils, and usually an anatomical diagram. There is a phylogeny for each kingdom, The phylogenetic treatment is less sure than the rest but sometimes has potentially fruitful suggestions. Extinct groups are ignored. I'm not sure there really needed to be a second edition yet, but recent advances are indeed incorporated. The great strength of the book is on the commonly unappreciated diversity of microorganisms, with 17 phyla given for prokaryotes and 27 for protists (called "protoctists", which is not only more unwieldy but sounds rather like it should refer to primitive anuses). Our multicellular chauvinism leads us to minimize the dimensions in which they exploit their world; this book is a good antidote for that insidious infection. -LMV

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On the Track of Ice-Age Mammals.
Anthony J. Sutcliffe. 1985 (1986 in USA). Harvard Univ. Press. 0 + 224 pp. ISBN 0-674-63777-1. Hardbound. $25.00.

This is rather a potted pourri of almost-essays on topics which happen to have caught the author's fancy. A few actual mammals are discussed here and there, but they seem almost incidental to the environment, localities, geology, art, folklore, and the like which are the real subjects of the book. One could say in folklore jargon that the mammals are a motif. Accepting this approach, and entertainment is a valid way to popularize science, the book comes off reasonably well. The topics are mostly, I suppose, of fairly general interest, and Sutcliffe writes adequately well and knows his material. Lots of pictures, including the usual posed-looking paintings crowded with every animal that will fit in. (One of a mammoth family is nice, though, if you can block out a wolverine and some horses, and the details are scientifically as well as artistically good.) The author knows better than to say the "last" glaciation rather than the "latest"; in fact the book begins with a quotation about the next one. Or will the greenhouse get us first? Tell me after the next nuclear war. -LMV

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Fungal Biology.
Harry J. Hudson. 1986. Edward Arnold (3 E. Read St., Baltimore, Md. 21202). vi + 298 pp. ISBN 0-7131-2895-X. Softbound. $23.95.

"Fungi are certainly not plants." The book's first sentence is appropriate, but what then should we do with them? The usual geography these days is to enthrone them in their own kingdom, which may be best in a modified way. The problem is that the organisms we call fungi now seem to be polyphyletic, a problem which won't go away by the usual solution of removing the various slime molds. Because the largest groups of fungi, the Ascomycetes and Basidiomycetes, may well form a single monophyletic group, the kingdom should shed its outer provinces and be renamed. The name Eumycota is available and appropriate. Calling this group Fungi would create an unnecessary ambiguity with the adaptive facies we call fungi; the latter word is necessary in ecology and similar fields, just as the word "algae" is.

The book itself ignores such matters; "biology" here is restricted mostly to trophic ecology. Not just what the food is, but the adaptations and ecology involved in getting it. Parasitism gets only a short (but interesting) chapter and a couple of pages elsewhere, but decomposers and mutualists receive extended treatment, as do fungi of aquatic and extreme environments. Decomposers of animal bodies are ignored, though. There is even a chapter on mutualism between fungi and some insects. The treatment overall is fresh and informative. -LMV

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The Natural History of Antelopes.
C.A. Spinage. 1986. Facts on File. xiii + 203 pp. + 8 colored plates. ISBN 0-8160-1581-3. Hardbound. $19.95.

Antelopes are any bovids which aren't goats, sheep, cattle, buffaloes, or the like. Despite the title, the book restricts itself to African species, of which there still are a goodly number. Its strength is in descriptive behavioral ecology and life history; there are also useful sections on habitats and a survey of the living species. The chapter on paleontology and classification is a minor disaster. The author doesn't mention tourism but isn't optimistic about farming and the like as conservation methods; apparently this has actually been proposed. -LMV

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A Functional Biology of Nematodes.
David A. Wharton. 1986. Johns Hopkins Univ. Press. (x) + 192 pp. ISBN 0-8018-3359-0. Hardbound. $30.00.

Don't judge a nematode by its looks. They look pretty much alike, nondescript wiggly spindles usually smaller than pins. They are among the most remarkable of animals, though, and live everywhere except in many marine organisms; it seems that marine parasites are in this case derived from nonmarine ones, polyphyletically. Some nematodes swim in fixative for hours; the eggshell of a species tested protected it for a day in concentrated sulfuric acid. Their excretion is apparently not via what is still called their excretory system, which may be involved in osmoregulation. Parasitism has evolved several times, although nematode phylogeny is poorly understood, and parasites now represent more than half of the known species. I do mean known; free-living forms have been much less studied. The female of one parasite feeds by absorption through an enormous everted uterus; Wharton doesn't say how the male manages, if there is one. The book unfortunately doesn't consider the effects of body size on nematodes. Its emphasis is on physiology and functional anatomy, both considered broadly, with a bit of ecology and development. Altogether well done. -LMV

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The Origin of Birds and the Evolution of Flight.
Edited by **Kevan Padian**. 1986. California Academy of Sciences, Golden Gate Park, San Francisco, Cal. 94118 (their Memoir 8). viii + 98 pp. ISBN 0-940228-14-9. Softbound. $12.95.

What was the ancestral sequence for birds and how did they come to fly? These questions are still open but we may be coming close to an approximate resolution. More than half the volume is taken by an important paper by Gauthier, who gives evidence for the origin of birds from deinonychosaur theropods. He doesn't put it this way, and indeed his dogmatic cladism prevents a realistic consideration of some other views. Bock gives a good summary of the argument for an arboreal origin of bird flight, while Ostrom cursorily advocates a cursorial origin. Both agree that <u>Archaeopteryx</u> didn't have powered flight. Ostrom's argument is that <u>Archaeopteryx</u> wasn't adapted for trees. Neither, I think, are kangaroos or hoofed hyraces, but some live there. Pennycuick discusses adaptive origins of vertebrate flight more generally and assumes rather than argues for a gliding (arboreal) ancestry. The volume is essential for those with any serious interest in its subjects. -LMV

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Parasitic Protozoa.
J.P. Kreier and **J.R. Baker**. 1987. Allen and Unwin. xiv + 241 pp. ISBN 0-04-591021-9 hardbound, $50.00. ISBN 0-04-591022-7 softbound, $24.95,

This is a humdrum, competent introduction to its subject, useful but not worth looking at unless you need to. It emphasizes parasites of mammals, especially humans, although by no means exclusively so, and focuses mostly on life histories with some attention to fine structure and diseases produced. Occasional non-parasitic symbionts are discussed. The arrangement is taxonomic, with occasional explicitly evolutionary passages. The authors haven't heard of the early Cretaceous flea, 10^8 years before they have insects attacking vertebrates. In a book like this <u>E</u>. <u>coli</u> is, appropriately, <u>Entamoeba</u> <u>coli</u>. -LMV

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A Functional Biology of Echinoderms.
John Lawrence. 1987. Johns Hopkins Univ. Press. (xi) + 340 pp. ISBN 0-8018-3547-X. Hardbound. $56.50.

For "functional biology" in this case read "functional morphology". There is almost no sign of ecology, physiology, or development — not that a book need cover these, but the title does mislead. In its real domain the treatment is pretty good. It is organized around feeding, oxygen uptake, circulation, locomotion, maintenance of position, protection, and reproduction, For most of these topics each extant class is treated separately. There is a brief overview of some extinct groups, which are then forgotten despite their uniquely great diversity. The evolution of function within extant classes has some consideration here and there, but most of the presentation is by example and by functional rather than phyletic comparison. I was nevertheless surprised to see no comparisons by allometry, surely one of the central pillars of functional morphology. In fact the author thinks that a large range of body sizes implies that individual selection is subordinate to group selection on body size (or so I interpret a rather muddled passage). I was astonished to learn that a moderate proportion of nutrient uptake by some, perhaps most, echinoderms is by active transport of dissolved material through the body wall. There is a lot here of interest if one can wade through the writing. -LMV

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The Biology and Evolution of Lungfishes.
Edited by **William E. Bemis, Warren W. Burggren**, and **Norman E. Kemp**. 1987. Liss. viii + 383 pp. (Reprinted from Journal of Morphology, Supplement 1, 1986.) ISBN 0-8451-4225-9. $49.50.

Lungfishes are living fossils in more than their retention of lungs. Each of the three surviving genera is known from the Cretaceous, and one surviving species is reported from the early Cretaceous. This volume is not a summary of all that is known of the group, although there are papers on function and on the natural history of two of the surviving genera, and a large annotated bibliography mostly of the surviving genera is included; rather the emphasis is phylogenetic, both within the group and on its degree of closeness to tetrapods. Much original material is presented. Marshall's phylogeny of the Dipnoi themselves, although based on a computer analysis, is of some general interest for its well-grounded conclusions that (1) which tree is most parsimonious depends on how one defines the characters and (2) function is important in weighting characters. The majority view that coelacanths rather than lungfishes are the closest surviving relatives of tetrapods should not be a surprise, although the contrary is still advocated. -LMV

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Frogs & Toads of the World.
Chris Mattison. 1987. Facts on File. 0 + 191 pp. ISBN 0-8160-1602-X. Hardbound. $22.95.

Despite the title, the emphasis of the book is not systematics but adaptations and lifestyles. Thus the organization is by topics such as mating or physiology, but these are treated in a broadly comparative manner. What is covered seems to reflect mostly popular rather than professional interest; e.g., the completely hybrid nature of the common frog of French frog legs, Rana esculenta, isn't even mentioned, nor is the phylogeny of frogs. The treatment is competent, though, with some lapses such as the page on paleontology. One chapter does review the families as such. Probably the highlight of the book is a remarkable set of mostly colored photographs of diverse frogs in natural settings. -LMV

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Trees of North America.
Alan Mitchell. 1987. Facts on File. 0 + 208 pp. ISBN 0-8160-1806-5. Hardbound. $24.95.

Why another tree book? Well, I like this one. Its best feature is the large set of colored drawings by David More, who indeed initiated the book. These show whole trees (bare or leafed), leaves, fruits, flowers, and often bark. They are extraordinary, not your ordinary botanical artwork, and are lifelike as well as beautiful. The text is only partly descriptive, emphasizing also distribution, cultivation, and even noteworthy individuals. One should be careful in using the book for identification because many less common species are omitted; there are no keys. However, the book has the great advantage of including introduced and even cultivated species on an equal footing with natives. Many varieties are treated to some extent individually. There are also distribution maps and advice on cultivation. -LMV

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Palaeoecology and Biostratigraphy of Graptolites.
Edited by **C.P. Hughes** and **R.B. Rickards**. 1986. Blackwell Scientific. ix + 277 pp. ISBN 0-632-01071-1. Hardbound. $106.00.

The title is misleading: the book has a lot of Ordovician and Silurian biostratigraphy but only one paper (of 27) related to paleoecology. There is some low-level phylogeny and two mutually contradictory revisions of higher relationships, but interest of nonspecialists will probably be mostly on several papers dealing with various aspects of development of these extinct colonoid hemichordates. For instance, one genus grew as a logarithmic spiral and evolutionary change in relevant parameters is analyzed. One paper gives a now minority view of secretion of the skeleton, and another presents a beautifully detailed description of skeletal secretion for the living colonoid hemichordate _Rhabdopleura_, with emphasis on its very early development. -LMV

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Das System der Medusen.
Ernst Haeckel. 1879, reprinted 1986. Jena: Gustav Fischer Verlag; distributed in USA by VCH. 2 volumes; xxvi + 672 + [9] pp.; 80 pp. + 40 colored plates. ISBN 3-527-26445-0. DM 396 (about $220).

A classic in each sense. We have learned more since 1879, but Haeckel's monograph remains a basic foundation. The orders and often families receive discursive and comparative treatment, while the lower taxa are mostly described without specific reference to others. Haeckel once seriously considered becoming a painter, and his plates are deservedly famous.

The study is of medusae _per se_, excluding the polyps of those taxa which have them. This represented a deliberate departure from earlier emphasis on polyps by others. It is nevertheless noteworthy in light of Haeckel's already-matured interest in the evolution of development. His methods of estimation of phylogeny would provide an interesting study. In this monograph they are topological (used, rather than explicitly stated), relying on intermediate groups, and somewhat recall pattern cladism with appropriate paraphyly and explicit phylogeny. -LMV

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The Evolution of Vertebrate Design.
Leonard B. Radinsky. 1987. Univ. Chicago Press. ISBN 0-226-70235-9 hardbound, $35.00. ISBN 0-226-70236-7 softbound, $12.95.

It has become fashionable in some quarters to minimize the importance of adaptation. It doesn't take much thought to see part of the fallacy of this view, which was quite adequately rebutted by the pre-evolutionary tradition of natural theology, culminating in the Bridgewater Treatises of ca. 1835. Radinsky continues this approach, now called functional anatomy and the like, and combines it with vertebrate paleontology in this semipopular account. The book is well written, attractive, and accurate; I noted no mistakes which the late author would have made knowingly. He notes uncertainties and inconclusive arguments as appropriate. There is some phylogeny, although this is subordinate; the real theme is the history of vertebrate adaptations "from fish to man", as Gregory used to say. As usual with books by people who are both creative and intelligent, even professionals will find some items of interest here. -LMV

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Current Ornithology. Volume 4.
Edited by **Richard F. Johnston**. 1986. Plenum Press. xiii + 324 pp. ISBN 0-306-42352-9. Hardbound. $45.00.

The series is a potted pourri of reviews; this volume is, like the others, a useful collection. Some chapters (geographic variation, heritability in wild birds, history of the Australian avifauna, and a bibliography of translations) have largely predictable contents. One on clutch size shows that Lack's theory and related adaptive strategies may well explain the sizes laid, although there remain problems. Another chapter looks at competition from the viewpoint of individuals, which can experience it at even low densities. People besides ornithologists should look at this, although they probably won't because of the series it's in; it is a good complement to Łomnicki's recent book, reviewed in this issue. I was surprised to find my greatest interest in a review of abundance changes in the dickcissel. The species has a sexually dimorphic biology which, in a changing food environment, has led to a great excess of males (even an all-male population) and greater susceptibility to encroaching cowbirds. It may even become extinct as a result, at least in the northern part of its range. -LMV

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Living Invertebrates.
Vicki Pearse, John Pearse, Mildred Buchsbaum, and **Ralph Buchsbaum**. 1987. Blackwell Scientific and Boxwood Press. xiv + 848 pp. ISBN 0-86542-312-1. Hardbound. $49.95.

There aren't many introductory books on invertebrate zoology, and each is best in some ways. This one is first among serious treatments in readability, despite some feeling of being written down to, and probably in its figures. These are informative, mostly accurate, and very numerous; in fact they and their often extensive legends are the main vehicle for conveying the diversity of the groups. There are even 36 colored plates, officially to illustrate effects of pigments. Unfortunately the figures sometimes don't agree with the text, most notably for one showing 14 origins of animals from protists. The book is also good with respect to structure and adaptations, but its treatment of phylogeny, while competent, is less sure. It is up to date; there are even 3 pages on the Concentricycloidea, which were first described only a few months before the book appeared. -LMV

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A Field Guide to Coastal Wetland Plants of the Northeastern United States.
Ralph W. Tiner, Jr. 1987. Univ. Massachusetts Press. (v) + 285 pp. ISBN 0-87023-530-7 hardbound, $25.00. ISBN 0-87023-538-9 softbound, $12.95.

A byproduct of our historical aversion to marshes (which resemble flooded prairies) and swamps (which resemble flooded woods) is a lack of identification manuals for their biotas. I like this one. It has extensive keys aimed at the ignorant, i.e. at the prospective real users, and information and attractive drawings for more than 150 species. There are also discussions of kinds of habitats and even maps of the salt marshes from Maine to Maryland. -LMV

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Carnivorous Plants of the World.
James Pietropaulo and **Patricia Pietropaulo**. 1986. Timber Press, 9999 SW Wilshire, Portland, Ore. 97225. (iv) + 206 pp. + 16 colored plates. ISBN 0-88192-066-5. Hardbound. $27.95.

Mostly this book is for gardeners. It has a large amount of information, from carnivorous societies of plants (a hyphen was, I trust, omitted) to lists of hybrids to what to do about pests. There is some information of interest to biologists <u>qua</u> biologists, and some of the photographs on the plates are outstanding, but other books give more on the biology. -LMV

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Anatomy of Flowering Plants: An Introduction to Structure and Development.
Paula Rudall. 1987. Edward Arnold, 3 E. Read St., Baltimore, Md. 21202. iv + 80 pp. ISBN 0-7131-2950-6. Softbound. $13.95.

Anatomy, to botanists, means tissue structure. It is not at all a dead subject and the book refers to many recent contributions. There isn't room here for much in the way of comparative treatment, and I don't know whether the near lack of analytical discussion of function reflects the subject or the author. The book is a good brief introduction. -LMV

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The Natural History of Badgers.
Ernest Neal. 1986. Facts on File. xviii + 238 pp. + 8 colored plates. ISBN 0-8160-1409-4. Hardbound. $19.95.

There was once a paper with the title, duly listed by the Journal of Insignificant Research, of "Badger-watching for beginners". Well, this is a book about the results of badger-watching and I wouldn't call it insignificant. Badgers are nocturnal, but Neal found that they didn't mind red light. The result is a good treatment of the usual aspects of natural history, almost entirely for Britain. The American badger is a different genus, and this and other genera get brief accounts also. Clearly the standard treatment. -LMV

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Comparative Primate Biology. 4 volumes in 5.
Edited by **J. Erwin.** 1986-1987. Alan R. Liss. Hardbound.
Volume 1 (1986), Systematics, Evolution, and Anatomy (Daris R. Swindler and J. Erwin, ed.), xvi + 820pp. + 1 phonograph record of gibbon calls. ISBN 0-8451-4000-0, $190.00.
Volume 2A (1986), Behavior, Conservation, and Ecology (G. Mitchell and J. Erwin, ed.), xi + 633 pp., ISBN 0-8451-4001-9, $150.00.
Volume 2B (1987), Behavior, Cognition, and Motivation (G. Mitchell and J. Erwin, ed.), xi + 296 pp., ISBN 0-8451-4002-7, $100.00.
Volume 3 (1986), Reproduction and Development (W. Richard Dukelow and J. Erwin, ed.), xi + 497 pp., ISBN 0-8451-4003-5, $120.00.
Volume 4 (1987, copyright 1988), Neurosciences (Horst D. Steklis and J. Erwin, ed.), xiii + 769 pp., ISBN 0-8451-4004-3, $190.00.

Wow. 16 pounds (7 kg) it hefts overall. The idea is to provide an accessible and comparative research-level overview of most aspects of primate biology that are now active. The neuro-behavioral emphasis reflects the interests of the series' editor.

And it succeeds moderately well. It will certainly be the standard single source for some years, although some areas like social behavior and paleontology have better general volumes available elsewhere and some areas aren't covered at all. There are unusually good indices. Not all the contributors know what comparative biology is, though, and this problem is especially severe in fields where typology and the scala naturae are still strong, like the study of learning, the nervous system, and blood groups. Rather than leading their fields away from the early Nineteenth Century, too many of these reviews just follow the local crowd.

Be warned that the initial paper in Vol. 1, purporting to be an overview of primate systematics, is simply incompetent. This is true at each level from data through synthesis. Fortunately many groups (but not, e.g., cercopithecids, hominids, lorisids, or Oligocene genera) have a more detailed survey by others; these range from good to superb and some give much new information. In these books there is no easy way to locate what is new, as most papers lack summaries. The anatomical coverage is spotty; e.g., the forelimb is discussed only for the colobines (but at more than 100 pages), and there is nothing on teeth or various other aspects. It is nevertheless probably for anatomy that the greatest density of new work is presented. There is only one paper on molecular evolution, and it is sketchy if thoughtful. Chromosomes and other microscopic aspects are absent except for enamel structure. Extinct and extant primates are in most papers treated together. A major lack is a synthetic treatment of the adaptive evolution of the order. Most of the papers are individually worthwhile to major treatments, but readers are going to pick out specific papers, and even jointly there is no sense of unity. Modern work here goes well beyond rejection of a scala naturae, and even the reason for that rejection is not made clear for those who need it.

Volume 2A is surprisingly one-sided. Despite its title there is no real ecology at all, and conservation, on which the rest will depend, has only one paper. This long paper is first-rate, though, showing with description, data, and many pictures the problems other primates face from the uncontrolled expansion of one. Its proposed solutions are low-level ones which, to the extent that they are implemented, will buy a little time for the real causes to be dealt with. Primates are quintessentially social animals, but apart from a short and interesting theoretical paper on how to study social relationships the book has only one paper on the subject itself, on individual spacing. Two of the better papers deal in a genuinely comparative and reflective manner on how individuals communicate vocally and chemically. There is also a theoretical discussion ot time and energy budgets. The rest of the book is pretty much a discursive compilation of data on several aspects of individual behavior, with greatest emphasis on its development (or at least on behavior at different ages). I did not find most of these papers readable, but I suppose they will be useful for the information they bring together even if this is often not very comparable among species or ages.

Most of the papers in Volume 2B are presumably those submitted too late for 2A. The section entitled "motivation" consists entirely of four papers reviewing social grooming, animal foods, reproductive seasonality, and zoo breeding! They are each competent surveys but are foundations rather than towers from which one can scan the horizons. The same is rather less true for two of the three other papers, which really are on cognition, dealing with tool use and learning. Perhaps, to extend their arguments, our dependence on tools is related to a neotenously greater retention of exploratory behavior and curiosity as well as to our neotenously large brain. The first paper in the volume, on self-awareness, is a short sharp shock: we can function but are not aware when sleep-walking and in other cases; self-awareness thus may be an important element or at least marker in our mental evolution. It is amenable to experiment in extant species.

There is a reasonably comprehensive survey of a subject in Vol. 3, reproductive physiology from gametogenesis to implantation with a major emphasis on endocrinology, including that of senescence. (Nonreproductive endocrinology, and indeed other aspects of physiology, are omitted from the series.) There are also short treatments of teratology and placental anatomy and an even shorter one of developmental staging. The reviews are good ones and most emphasize what we don't know. Development per se is restricted to a paper on growth rates and one on using early skeletal development for estimating ages. Even though the former paper assembles its data well, there really is a lot more to both individual and comparative development than this.

Combined with papers on endocranial casts and the cerebellum in Vol. 1, Vol. 4 covers most comparative aspects of the brain (including its developmental extension the retina) but not of other parts of the nervous system. The papers do try to be comparative, but here especially there is often little or nothing known for most groups. There is, though, a great deal of information here ready to be mined. The emphasis is anatomical, including a functional approach as appropriate; the series' overall omission of physiology could perhaps have been modified here, if there really is enough comparative information on transmitters and the like, but that would have made an even bigger book. An initial chapter on comparative sizes of brains and some of their components uses an appropriate developmental standard for comparison (although the authors don't call it that), something which is usually not done.

There is a policy at some editorial level to omit periods after normal abbreviations; the authors shouldn't be blamed for infelicities like "ie" or "eg".

So. Quite a useful series, overall. Was it worth everybody's effort? That's a harder question which I won't try to answer. -LMV

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Current Perspectives in Primate Biology.
Edited by **David M. Taub** and **Frederick A. King**. 1986. Van Nostrand Reinhold. xx + 315 pp. ISBN 0-442-28311-3. Hardbound. $44.50.

This proceedings of a congress is one of the worst of its kind. There is another volume on social behavior, reviewed in the last issue, but together the two would make one rather small book, as the page size is quite small. An unknown proportion of the papers at the congress were submitted to journals instead of here; I hope that their average quality was better and that most of what is here came from decisions by the authors that their work wouldn't be published otherwise. Not every paper here is like that, but so many are that the useful ones get lost in the dump. I'm sorry for the authors of the better papers. One of these, incidentally, concludes that the Lagomorpha is the order closest to the Primates; it would be interesting to know the details of such a conclusion, even though it has been superseded by later work. The editors make quite a point that everything was reviewed, but reviews don't matter if there is no quality control when they come in.-LMV

Catalogue of Primates in the British Museum (Natural History) and Elsewhere in the British Isles. Part IV: Suborder Strepsirrhini, Including the Subfossil Madagascan Lemurs and Family Tarsiidae.
Paulina D. Jenkins. 1987. British Museum (Natural History). x + 189 pp. ISBN 0-565-01008-5. L29.50 (about $54).

The BM catalogs are much more than catalogs. In addition to the listing of specimens, with collection data, there is more or less a revision of the group involved. In this case revisionary work is on the less side, but there is a great deal of many kinds of information, compactly presented, on all the taxa included. This includes statistical summaries of new measurements as well as checking everything that could be checked. The result is as reliable a summary as one could want. (Tarsius is included for convenience only.) Some names are in a different form than in their conventional use as a result of this care; I suppose we will have to follow the rules and agree. The author does err, though, in assuming that noncladistic classifications are cladistic. In fact hers isn't either, as a phylogeny can't be reconstructed from it.

The statement that "It is now generally accepted that the Madagascan prosimians are polyphyletic" may possibly be true, although I've not accepted it. The problem comes with special derived similarities between adapids and lemurs, among the lemur groups, and between cheirogaleine lemurs and galagos (in more ways than given here). They could all be nonconvergent if the galagos (and lorises) had a Madagascan ancestry. We don't know enough yet to say, but this may be the easiest solution.
-LMV

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Eggs: Nature's Perfect Package.
Robert Burton. 1987. Facts on File. 0 + 158 pp. ISBN 0-8160-1384-5. Hardbound. $22.95.

I expected a potboiler but I was wrong. This is a beautiful and semi-scholarly book which is written well enough to be accessible to a nonscientist. It is actually about more than eggs; aspects of reproductive strategies and development are also included, but seeds aren't. The entire approach is by natural history rather than physiology; even the chapter on fertilization is almost entirely behavioral. The discussion is comparative but by example rather than anything approaching a survey; invertebrates have a moderate emphasis. Almost any biologist will find something new and of interest; I didn't notice any inaccuracies, although sometimes work of the last decade or so is ignored. The many colored photographs are superb. -LMV

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Seventy-five Years in Ecology: The British Ecological Society.
John Sheail. 1987. Blackwell Scientific. xiv + 301 pp. + 16 plates. ISBN 0-632-01911-5 clothbound, $65.95; ISBN 0-632-01917-4 softbound, $28.00.

Why write a book like this? Why publish it? Who but an antiquarian or a reviewer or someone wanting to embellish memories will read it? There are two recent histories of ecology as a subject; this book has somewhat the flavor of a boardroom-commissioned history of a corporation. If one accepts the genre as valid, the book is competently done, although the most remarkable feature of the society, its continuing accumulation of an immense financial surplus in the past ten years or so, is only superficially treated. But I suppose that pleases the boardroom. -LMV

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Darwin's Insects: Charles Darwin's Entomological Notes.
Edited by **Kenneth G.V. Smith**. 1987. British Museum (Natural History), their Bulletin, Historical Series, 14 (1). 143 pp. ISBN 0-565-09003-8. Softbound. £25.00 (about $45).

Darwin was first an entomologist. Many of the specimens he collected have been lost, but the records of the surviving ones are summarized here. Most of the booklet is given to the notes Darwin made of his specimens from the Beagle, with annotations by the editor for almost all, giving relevant information such as current name or disposition. Some of the annotations are of more general interest; one gives information favoring Chagas's disease as Darwin's malady, information (although not the diagnosis) new to me and others who don't read medical journals. There is also an annotated list of insects named after Darwin. "I have dedicated this species to Charles Darwin Esq., M.A., V.P.R.S., whose enquiries into the obscurer phenomena of geographic zoology have contributed more than those of any other man living to our knowledge, in the general questions of animal distribution."
-LMV

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Charles Darwin's Notebooks, 1836-1844.
Edited by **Paul H. Barrett, Peter J. Gautrey, Sandra Herbert, David Kohn**, and **Sydney Smith**. 1987. Cornell Univ. Press. viii + 747 pp. ISBN 0-8014-1660-4. Hardbound. $75.00.

Darwin's notebooks record his thoughts as he first had them, throughout his scientific life, and thus are an almost uniquely valuable resource for historical inquiry. Most of the items included here have been published before, but not as well. In addition to doing everything possible to ensure accuracy from the sometimes almost illegible original, the editors provide copious annotations on context and references. Later annotations to the notebooks are distinguished to the extent possible, and the result can fairly be called definitive.

In view of doubts sometimes expressed on Darwin's views on the actual processes of speciation, a quotation from 1837 (Notebook B, p. 82) is of interest: "Species formed by subsidence. Java & Sumatra. Rhinoceros. Elevate & join keep distinct. two species made."
-LMV

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The Correspondence of Charles Darwin. Volume 3: 1844-1846.
Edited by **Frederick Burkhardt** and **Sydney Smith**. 1988 (stated 1987 in book). Cambridge Univ. Press. xxxii + 523 pp. + 13 plates. ISBN 0-521-25589-9. Hardbound. $37.50.

The magnificent series continues; it couldn't be better done. Many letters had not previously been published, and the accuracy of earlier collections was suspect. The care taken with this one is noteworthy. In addition, almost every letter, however short, is accompanied by notes explaining people, events, or the like referred to, or other aspects of context. A long appendix gives brief information on all people referred to, and translations are given as well as the originals for foreign letters. The index is (of course?) excellent for the science as well as the history, and the editors' introduction gives a brief biography for the interval, supplemented by Darwin's own journal in its original version. After three volumes about a thirteenth of the letters have been covered. "There is one expression which you Botanists often use (though I think not you individually often) which puts me in a passion, viz calling polleniferous flowers "sterile", as non-seed-bearing." (17 November 1845)
-LMV

Charles Darwin's Natural Selection. Being the Second Part of His Big Species Book Written from 1856 to 1858.
Edited by **R.C. Stauffer.** 1987 reprint of 1975 book. Cambridge Univ. Press. (xvi) + 692 pp. ISBN 0-521-34807-2. Softbound. $29.95.

Read this book if you haven't. The Origin was its abstract; more precisely about half the Origin was. (The first part was expanded into Animals and Plants Under Domestication and only one page survives; and the longer version was never finished.) A lot that Darwin actually wrote for publication is first given here, and I think that it is overall a better book to the extent that it was completed; occasional sections even in it remain as outlines. There are more examples, fuller argument, even references. Stauffer edited it cleanly and provided a collation with the Origin and a good index. A longer and richer version of the wedge analogy isn't indexed; it is on p. 208. The text itself ends with the following prolix sentence: "Hence I conclude that it has not as yet been absolutely proved that the same species has ever appeared, independently of migration, on two separate points of the earth's surface: if this were proved or rendered highly probable, the whole of this volume would be useless, & we should be compelled to admit the truth of the common view of actual creation; & that organic beings are not exclusively produced by ordinary generation, with or without modification." This passage was made more positive and unPopperian in the Origin, apparently as a result of an annotation by Hooker reproduced here. -LMV

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Nature in the New World, from Christopher Columbus to Gonzalo Fernández de Oviedo.
Antonello Gerbi. 1986 (copyright 1985). Univ. Pittsburgh Press. xvii + 462 pp. (Translated from Italian of 1975 by Jeremy Moyle.) ISBN 0-8229-3516-3. Hardbound. $39.95.

Gerbi was a historian, apparently a rather eminent one, who thought he knew enough natural history to write a treatment of European views to 1540 on the American biota. He didn't (e.g., he thought it was an open question ln 1972 whether lizards are closer to snakes or to crocodiles), and he also didn't have the interest to do so, which I suppose is fortunate. This book is mislabeled. It is really about diverse sorts of views of the early voyagers and commentators, especially Oviedo. The natural world is mentioned here and there but not in a coherent way, not even in relation to the prevalent world view that all geographical variation was latitudinal. There does seem to be material for a useful study, but someone else will have to write it. -LMV

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Modern Science and the Book of Genesis.
James W, Skehan. 1986. National Science Teachers Association, 1742 Connecticut Ave. NW, Washington, D.C. 20009. 0 + 30 pp. ISBN 0-87355-046-3. Softbound. $4.00.

The author of this booklet is a Jesuit theologian and a geologist. It gives an unusually clear discussion of modern interpretations of Genesis, a history of other interpretations and their problems, and a conclusion on the broader implications of antirationalism. A position statement by the publishing organization concludes the text and, despite its value, is an unintentional foil for the rest in both style and philosophical acuity. What the author wrote is the best I've seen. -LMV

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Science and Earth History. The Evolution/Creation Controversy.
Arthur N. Strahler. 1987. Prometheus Books (700 E. Amherst St., Buffalo, N.Y. 14215). xiv + 552 pp. ISBN 0-87975-414-1. Hardbound. $39.95.

Another one yet? Actually this may be the most useful book on the subject for serious inquirers. Strahler is a geomorphologist by trade and the book is especially good with respect to physical geology, but he has compiled a great deal of material on all aspects. I did note some outdated information on phylogeny construction and mammalian paleontology, but nobody knows everything and most of the material I could evaluate is both current and as comprehensive as could be expected. Strahler presents views (and evidence used) by both creationists and scientists, partly by many direct quotations, and he evaluates everything discussed, in a running commentary. There is a large amount of information here and probably anyone will find new material, even apart from the diverse details of intellectual dishonesty which creationists use as ploys. A computter glitch garbled p. 437. -LMV

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Evolution and Creation.
Edited by **Ernan McMullin**. 1986 (copyright 1985). Univ. Notre Dame Press. xv + 307 pp. ISBN 0-268-00917-1 hardbound, $24.96. ISBN 0-268-00918-X softbound, $12.95.

The authors of this book are philosophers or theologians, although Ayala has done a few other things too. His chapter is the only straightforward account in the book, an elementary treatment of the nature of evolution emphasizing genes. The other chapters are what used to be called apologetics, and they strike me as uniformly overdrawn. It must be difficult being a Catholic in an age of real science, with dualistic and supernatural beliefs which cannot be challenged and into which the outside world has to be interpreted. Doing so would seem to require a deliberate or unexamined partitioning of one's rationality rather like the Sunday ethics vs. weekday ethics of our larger culture. The writers are intelligent people and some of their arguments are ingenious, but they do not suggest an internal consistency of mind. -LMV

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[**Animal Asymmetry**: A Population-Phenogenetic Approach.] Asimmetriya Zhivotnykh: Populyatsionno-Fenogeneticheskii Podkhod. (In Russian.)
V(ladimir) **M. Zakharov**. 1987. Moscow: Nauka. 0 + 216 pp. No ISBN. Softbound. [Russian books are inexpensive but go out of print rapidly.]

Fluctuating asymmetry is the usual measure of sensitivity to developmental noise; its degree is an entirely different phenomenon from canalization, which is part of the program itself. This book is the first one on the subject. After a general discussion it reviews a large amount of work, much of it in Russian. Zakharov concludes that stress increases fluctuating asymmetry. That this is not a specific response to an environment per se is indicated by different responses of species more or less adapted to the environment. He even proposes, as have others, that fluctuating asymmetry be used as a measure of environmental stress. Unfortunately there are too many exceptions for this to be workable; another relationship, emphasized by others but also less than perfect, is with degree of heterozygosity. The phenomenon is nevertheless worth pursuing; it relates to an important aspect of development not obviously approachable from below, but any reductionist who reads this may take that as a challenge. -LMV

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The Growth and Form of Modular Organisms.
Edited by **J.L. Harper, B.R. Rosen**, and **J. White**. 1986. London: Royal Society (reprinted in 1988 from their Philosophical Transactions; distributed in USA by Cambridge Univ. Press). (ii) + 250 pp. ISBN 0-85403-281-9. Hardbound. $75.00.

"Module" is used here in a narrow sense as a complex and indefinitely repeated unit like a coral polyp or a rootlet. Limbs, segments, and the like are excluded, and the editors may have forgotten organisms like tapeworms, many polychaetes, aphids, and colonoid radiolarians. However, as they note, "It may be that the distinction between the biology of modular and unitary organisms is more profound than the classic distinction between animals and plants."

Our theories emphasize organisms like us, with others commonly treated as exceptions. This is as true for systematics as it is for population ecology. The volume attempts to shift our theoretical focus with respect to adaptive and formal aspects of growth and form. Most emphasis is on degrees of integration of modules, theory and comparison of the architecture of integrated units, functional anatomy, and hierarchies of modules. A point worth emphasizing, which has caused confusion elsewhere, is that metabolic allometry is eliminated by fully modular growth. There are also excellent synthetic and critical chapters. Together with a companion volume in the society's Proceedings, not reprinted, the book is a first-rate treatment of its subject. I wonder when the different focus will catch the attention of official developmental biologists — some of them are still interested in real organisms.

The review copy lacked a group of 16 pages. -LMV

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Metabolic Arrest and the Control of Biological Time.
Peter W. Hochacka and **Michael Guppy**. 1987. Harvard Univ. Press. xiii + 227 pp. ISBN 0-674-56976-8. Hardbound. $27.50.

Chemical (thermodynamic) equilibrium is death, or rather is approached only long after death. Organisms use energy merely to stay alive, quite apart from their active functioning. But some use more than others, and some organisms can reduce their level of basal maintenance to quite low levels from time to time. The book focuses on the mechanisms of the latter phenomenon.

As usual, "biological time" refers to the rate of living rather than to time itself. It is interesting and important that all measures and even definitions of time rely on rates; rate rather than time is conceptually if perhaps not causally fundamental, even in physics. This priority of rate has consequences which I hope to explore elsewhere.

Many organisms can dry out almost completely and recover on rehydration. The only water retained is bound and highly structured, enveloping macromolecules and apparently organized partly by an unusually high concentration of glycerol and other polyols. During such anhydrobiosis there is no detectable metabolism whatever; an upper limit is 10^{-7} of the usual rate. How long such a metastable state can be maintained with potential recovery is apparently not well documented but is at least decades. The rate and nature of deterioration of the metastable state may not be known; at least the authors don't address the matter.

Other organisms, such as the insects and frogs discussed here, can survive freezing for a while. Not just the use of antifreeze, but full freezing. Oxidative metabolism continues here at about 10^{-2} its usual rate. The mechanism involves sequestration of water within the cell and has some similarity to that in anhydrobiosis. Hibernation, aestivation, and tolerance of oxygen deficit are related phenomena which the authors discuss more or less intelligibly from the viewpoint of adaptive physiology. -LMV

Molecular Evolution of Life.
Edited by **Herrick Baltscheffsky, Hans Jörnvall**, and **Rudolf Rigler**. 1986. Cambridge Univ. Press. (iv) + 375 pp. (Reprinted from Chemica Scripta, vol. 26B.) ISBN 0-521-33642-2. Hardbound. $69.50.

For once it is actually useful to have a journal issue reprinted as a book. There are papers here that biologists, including evolutionary biologists, shouldn't miss, as well as the usual routine advances, descriptive reviews, and potboilers. Not many of the latter, and the book is a better collection overall than might be expected. The papers are research-level and are of 2 to 12 large pages.

The contributions are by chemists or chemically oriented biologists and physicists. Most of them do, though, bear on evolution. I noted only one case of reinvention of a wheel (Eigen and collaborators on sequence space, but they have their own perspective on it). Another paper by al. et Eigen proposes a way to reconstruct ancestral sequences which is not only known but known to be incorrect generally.

Some highlights: Tata reviews the evolution of hormones and their receptors, making some points new at least to me. For instance, a molecule already involved in "molecular linguistics" becomes a hormone mostly by addition of receptors in a proper place (he doesn't consider the internal response of the cell and its adaptive nature, though), and the specificity of hormone (or other signal-molecule) receptors may often evolve by removal of a piece of protein already structurally coadapted to a physically adjacent piece. Neurath has a nice treatment of the evolution of exons and protein domains. Shimke et al. provide a mechanism, involving over-replication of DNA and cell selection, for multiple (genome) mutation as a way to account for some observed genomic phenomena. Wizzell et al. note that immune memory lasting past the lifetime of lymphocytes is a problem, and then propose that it occurs by the recurrent production and destruction of lymphocytes with antibodies like the relevant antigen, thus stimulating proliferation of the original antibody. (But doesn't this explain too much — shouldn't it occur even without initial stimulation and thus lead to an antibody titer before challenge?)

Edelman accepts the view that morphological evolution occurs mostly by heterochrony and proposes a developmental-genetic theory, involving regulation of cell-adhesion molecules, to account for it. However, although his theory may have merit it is difficult for me to see how heterochrony explains most evolution of pattern formation. Mechanisms for the latter will have to be integrated. Perhaps the most significant paper is one by Baltscheffsky et al., who find that Rhodospirillum can produce inorganic pyrophosphate as an energy source under some conditions instead of ATP, and propose that this less complex path may antedate the use of ATP. -LMV

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The Origin of Eukaryotic Cells.
Edited by **Betsey Dexter Dyer** and **Robert Obar**. 1986 (copyright 1985). Van Nostrand Reinhold. xv + 347 pp. ISBN 0-442-21952-0. $44.95.

This book, by two former students of Lynn Margulis, presents evidence for her version of the origin of the eukaryotic cell. (They say that her seminal 1967 paper almost couldn't be published. How many papers like that actually couldn't, and can't?) I happen to agree with her on most things, but a book of readings really should give other views also. Given this bias, the book makes a pretty good selection, although at least one paper (on gene transfer from a fish to a bacterium) has had its main result falsified after being chosen. However, because of the bias I can't recommend the book as an overview; I hope that the mind-set indicated for the editors won't affect the future publication of papers which differ from their own outlook. -LMV

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The Eukaryote Genome in Development and Evolution.
Bernard John and **George L. Gabor Miklos**. 1987 (stated 1988 in book). Allen and Unwin, 8 Winchester Pl., Winchester, Mass. 01890. xviii + 416 pp. ISBN 0-04-575032-7 hardbound, $60.00. ISBN 0-04-575033-5 softbound, $29.95.

This is a strange book. The authors think that they are being radical reductionists, and for a large part of the book I thought so too. They seemed to advocate an explanation of all development as resulting from diverging cell lineages with binary choices from specific genes. They seemed to want to explain the major aspects of evolution by processes internal to the genome. Most of the book, meanwhile, is a straightforward account of the phenomena known to occur in the genome, and patterns immediately resulting therefrom. But it finally turns out that they do recognize the importance of cell interactions and perhaps other regulation in development. And their antipathy to evolutionary theory comes, aside from having read statements of its death by others, merely from being interested in different questions. They don't care about phylogeny (and they don't think anyone else should either); they don't care about population processes (and they don't think anyone else should either). What they want to know, evolutionarily, is how morphological innovations come to occur, in a developmental context. So. I suppose, do the rest of us, among other things. But one can't really castigate a whole research program just for having an interest other than one's own, so they try to make it look outdated. They could, though, try to understand levels of selection and drift a little better than they do; the processes operate in the genome too, I'm afraid that in the end the book doesn't really say anything at all, although it does provide a convenient review of genome phenomena. -LMV

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Development of Hormone Receptors.
Edited by **György Csaba**. 1987. Birkhäuser Verlag, 0 + 196 pp. ISBN 0-8176-1858-9 (Boston), 3-7643-1858-9 (Basel). (Also published as Supplement vol. 53 to Experientia.) Hardbound. $65.50.

The theme is more evolution than development. Some, perhaps most, vertebrate hormones occur in protists, and even some in bacteria. Their function usually differs and has thus evolved, although the difference in itself doesn't require that a current microbial state is ancestral even if it is known. The aggregation of cells to form multicellular units has occurred many times, but there seems to be no evidence for most such cases on the nature of changes in signals. (The book itself is quite limited and doesn't even mention slime molds, where something is known.) The intergradations among the several kinds of metazoan cell signals (hormones, neurotransmitters, inducers, etc.) suggests a common sort of ancestry. Signals are no more effective than their receptors, and the receptors are equally widespread. There is suggestive evidence for the origin of receptors for some amino-acid signals from food receptors.

As with behavioral signals, evolution of the structure (as distinct from the cellular distribution) of a signal should usually require concomitant evolution of the receptor interpreting it. That the signal structures evolve at all is therefore a problem in each case. One remarkable possibility, for which there is preliminary evidence, is that a signal and receptor can be coded for by the complementary strands of DNA of the same gene. -LMV

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Molecular Evolution.
Edited by **E.A. Terzaghi, A.S. Wilkins**, and **D. Penny**. 1984. Jones and Bartlett. xiii + 409 pp. ISBN 0-86720-021-9. Hardbound. $40.00.

This is a useful collection of papers on molecular evolution sensu stricto; molecular variation in populations is excluded, but the coverage does include a bit on the origin of life and on phylogeny. The editors provide excellent introductions to the nine groups of papers. The papers chosen are defensible, although I do wonder at the lack of either classic by Zuckerkandl and Pauling. Kimura's name is consistently misspelled in the table of contents (but not elsewhere); the reproduction of the papers very occasionally verges on the illegible. -LMV

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The Túngara Frog: A Study in Sexual Selection and Communication.
Michael J. Ryan. 1985. Univ. Chicago Press. xv + 230 pp. ISBN 0-226-73228-2 hardbound, $33.00. ISBN 0-226-73229-0 softbound, $14.95.

This should become a minor classic. It is a rather extensive and well-documented study of sexual selection in action, the adaptations which have resulted from this selection, the costs in nonsexual aspects of fitness (energy and especially predation), and the behavioral compromises which are the resultant of the opposing selective components. The sexual selection is by female choice, as usual for frogs; the existence of such choice was for a while controversial. Ryan even gives a good discussion of the nature of selection on the female preference itself, usually an adaptively obscure behavioral trait. He does, though, make a mistake elsewhere which is too prevalent in the literature. In estimating selection intensity he uses the entire variance of, e.g., male mating success. Because some part of this is nonselective the estimate is merely of the scope for selection, the load space. He does have data to estimate actual selection intensities but doesn't do so, apparently not realizing the difference. -LMV

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A Primer of Population Genetics. Edition 2.
Daniel L. Hartl. 1988. Sunderland, Mass.: Sinauer Associates. xi + 305 pp. ISBN 0-87893-301-8. Softbound. $15.95.

Good books deserve new editions but don't get them if their market is small; often other books get them if they do have a market. Hartl's is of the first sort. It's perhaps the best short introduction to the subject and views its subject rather broadly. The treatment is theoretical in an elementary way with lots of real examples and realistic diagrams. There is as much on quantitative genetics as on the gene-oriented sort. A major addition is a chapter on molecular evolution. This is a tour de force and gives perhaps the best short current summary available for this area.

Of course the book has to omit a lot, but the only actual error I noted was the equation of normalizing with stabilizing selection. (In case you forgot, the response can be canalizing too.) That's a symptom of a more general feature of the field rather than of the book, a minimization of the phenotype even in quantitative genetics. We need to abstract from the world and simplify in order to gain understanding, but somehow the phenotype, at a causally as well as formally central position, tends to disappear in cracks between our presently existing subjects. -LMV

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Basic Concepts in Population, Quantitative, and Evolutionary Genetics.
James F. Crow. 1986. W.H. Freemen. xii + 273 pp. ISBN 0-7167-1759-X hardbound, $28.95. ISBN 0-7167-1760-3 softbound, $15.95.

This is a superb book. It is primarily theoretical, but with little mathematical preparation needed. Enough evidence is inserted to ensure that the reader isn't walking on a cloud. The writing is unusually clear, with care being taken to concentrate on important aspects. Controversies are presented as such, usually with an evaluation. Molecular evolution as well as variation is included, and the coverage generally is as comprehensive as the length allows. A long treatment of reproductive value could be clarified by noting its relation with the expected number of offspring in a stable population; I've never seen this done. -LMV

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Population Genetics,
Freddy B. Christiansen and **Marcus W. Feldman**. 1985 (stated 1986 in book). Blackwell Scientific. ix + 196 pp. ISBN 0-86542-307-5. Softbound. $19.95.

This low-level text is about human population genetics both in its examples and in its coverage, although some other examples are given. Not even antibiotic resistance gets in. The treatment is accurate and usually up to date, although a human evolutionary tree dates from 1963. I didn't know anybody could write a book on population genetics that omitted both Fisher's Fundamental Theorem and Wright's adaptive landscape; each does have a less-than-universal domain but each is basic. The book is useful for a collection of human examples. -LMV

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Genetics of Populations.
Philip W. Hedrick. 1985. Jones and Bartlett. xvi + 629 pp. ISBN 0-86720-011-1. Hardbound. $37.50.

A solid introduction. The running text gives theory and many examples are segregated in boxes. These can be original, e.g. a calculation showing that much more than the observed bottleneck is needed to explain the lack of observed isozyme variation in the California elephant seal. The coverage is fairly comprehensive, with both quantitative and evolutionary genetics but not the result of their merger. Molecular evolution and genetics receive some coverage but this is restricted to now-classic topics, without, e.g., genome shuffling. Mutation-rate regularities aren't given, and the page on group selection is weak. But these are quibbles; it's a good book. -LMV

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Quantitative Genetics.
Edited by **W.G. Hill**. 1984. Van Nostrand Reinhold. vol 1, Explanation and Analysis of Continuous Variation, xi + 347 pp. Vol. 2, Selection, xiii + 397 pp. Hardbound. ISBN 0-442-23219-5 (vol. 1), $52.95. ISBN 0-442-23218-7 (vol. 2), $52.95. ISBN 0-442-23217-9 (both), $94.95.

As part of the Benchmark series, the volumes are meant to give a representative sample of classic papers. Hill has done a good job at it. The papers range from 1904 to 1977 and thus exclude not only preMendelian work but the first Mendelian treatment, in the last part of Mendel's paper itself. (Mendel's multifactorial result is usually attributed to much later work, such as the 1910 paper of East included here.) Most of the papers are theoretical, on the diverse aspects of the subject, but there is a suitable selection of experiments. Selection in natural populations is omitted. Most of a paper of Weinberg's receives its first English translation. Hill provides an introduction for each group of papers and refers to others which have also been of lasting value. Many of the papers are still of scientific use, and the volumes as a whole, with the introductions, provide a prodromus for a history of the field. -LMV

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Biogeographical Evolution of the Malay Archipelago.
Edited by **T.C. Whitmore**. 1987. Oxford Univ. Press. x + 147 pp. ISBN 0-19-854185-6. Hardbound. $65.00.

Wallace's Line gets better and better. The best part of this book, apart from a handy summary of the plate movements and land history by Audley-Charles, is a set of two chapters by Musser and Holloway on the geographic affinities of some elements of the fauna of Sulawesi (Celebes). They conclude, by analyses including phylogenetic information, that their groups have closest affinities with the Philippines and with southern islands rather than, in particular, with Borneo, which is large and close but across Wallace's Line. (Another chapter, on plants, agrees but the evidence is weak and the author actually notes that it is cribbed from the forthcoming theses of two of his students, who aren't given authorship.) Some other chapters use the "center-of-origin" (i.e., greatest diversity) approach and are less persuasive, particularly since one, advocating angiosperm origin in blocks drifting north from Australia, is more or less contradicted by the relative times of entry of groups of fossil pollen. In doing analyses like this one should always remember the marsupials as a cautionary tale. Their "center of origin" from the modern biota is Australia, and a phylogenetic analysis of the extant fauna would place their origin in South America. However, it is now clear from the spatiotemporal distribution of fossils and their phylogeny that they got to Australia from South America but actually originated in North America, where the only extant species is a recent immigrant. I don't mean to imply that modern distributions contain no information, but it needs a bit of salt. There are some other useful chapters, and in this book they really are chapters. Several authors discuss the work of others and even fight back and forth. George provides a sensible and eclectic integrative conclusion.
-LMV

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Biogeography and Plate Tectonics.
J.C. Briggs. 1987. Elsevier. xi + 204 pp. ISBN 0-444-42743-0. Hardbound. Dfl.155.00 (about $75).

This is the first lengthy treatment of global historical biogeography to be based firmly on plate tectonics. That isn't to say that Briggs accepts the restorations of geophysicists uncritically. He emphasizes that they must also be consistent with biological evidence, and gives a number of cases of disagreement. In most of these I think Briggs is right. One or two may be resolved by geophysists' use of plates and Briggs's use of land and water; the two aren't always the same, as he knows in other contexts. He considers each continental plate separately, and also the oceanic plates of the Pacific. For each he summarizes the evidence on temporal change in geographic relations among plates, using all organisms with adequate data. He gives less emphasis to phylogenies than I would, and too often he uses outdated sources, at least for mammals and plants. Like some others, he sees the effect of barriers on differences which may be more a result of different climates, as between Amerasia and Euramerica in the late Cretaceous. Despite all, though, he does a pretty good job, and the book should be a standard source as well as one to take potshots at, as standard books usually are. It ends with a new set of paleogeographic maps based on land and sea, with justifications for their major features. -LMV

Biological Aspects of Human Migration.
Edited by **C.G.N. Mascie-Taylor** and **G.W. Lasker**. 1988. Cambridge Univ. Press. viii + 263 pp. ISBN 0-521-33109-9. Hardbound. $54.50.

The highlight of this otherwise unimaginative book is a chapter by Weiss which gives a critical review of diverse evidence on the phyletic interrelationships of the world's peoples, and on mechanisms of their diversification. It's the best treatment I've seen, although there is of course more recent evidence not covered, and it deserves a wider audience than it is likely to get here. It is short and lucid enough to be accessible to a cultural anthropologist, but its arguments are partly original and should be considered seriously by specialists. The rest of the book is what one would expect, with an interplay of biological and cultural factors looked at separately and together. -LMV

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6. Beitrag zur Fauna und Flora der Kapverdischen Inseln.
Edited by **Wolfram Lobin**. 1987. Forschungsinstitut Senckenberg, Senckenberganlage 25, D-6000 Frankfurt/M. 1, W. Germany (their Courier 95). 213 pp. ISBN 3-924500-31-2. Softbound. DM 48.50 (about $26).

Almost all the papers are on insects and plants. The former are taxonomic and agronomic, while the latter are prodominantly biogeographical at a more or less descriptive level, including within-island as well as broader aspects of distribution. One plant paper does relate an independent property of the plant (crassulacean acid metabolism) to distribution in a comparative way, and a paper on lichens finds their secondary metabolites to be related to their area occupied. There is some conseration of the Canaries as well as the Cape Verdes. -LMV

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Deep-Sea Sedimentary Environments Around Southern Africa (South-east Atlantic and South-west Indian Oceans).
R.V. Dingle et 23 al. 1987. South African Museum, P.O.Box 61, Cape Town, South Africa [their Annals 98 (1)]. 27 pp. + 2 large maps in tube. ISBN 0-86813-090-7. Softbound. R25 (about $10).

The centerpiece is a colored map, in unusual detail, of the kinds of deposits over much of the ocean around southern Africa; the other map is bathymetric. The text discusses the nature, sources, causes, and Cenozoic history of the sediments and their movements. The history and currents are complex and have resulted in a striking patchwork of deposits and erosion surfaces. -LMV

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Stratigraphy of the Middle Miocene Bopesta Formation, Southern Sierra Nevada, California.
James Patrick Quinn. 1987. Natural History Museum of Los Angeles County (their Contributions in Science 393). 31 pp. + folded map and chart. No ISBN. Softbound. $28.00.

In addition to mapping, Quinn reports a small vertebrate fauna covering three mammal intervals. -LMV

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Theories of the Earth and the Universe: A History of Dogma in the Earth Sciences. **S. Warren Carey**. 1988. Stanford Univ. Press. xviii + 413 pp. ISBN 0-8047-1364-2. Hardbound. $45.00.

The title sounds like it comes from a cracked pot, but it doesn't. Carey is one of the founding fathers (there were no mothers) of plate tectonics, and that wasn't an accident. He is one of the most original thinkers in the history of geology, with an incisive mind and a great respect for facts over fashion. He has nevertheless been outside the mainstream for most of his life, and not because of living in Tasmania, which "is not on the way to anywhere else."

There was an informal symposium on continental drift in 1960 at Columbia, where I was a student. Carey was the main speaker, but some others agreed. He convinced me that there really were major anomalies in the received view and major patterns which demanded explanation. He convinced others too, there and elsewhere, and he was briefly in the mainstream as geology shifted its paradigm (by rational means). What removed him from it again was the general adoption of the idea of subduction of plates. Carey had abandoned this view in 1956, probably before anyone else had thought of it, in favor of an expanding Earth. As radical as it may sound, the latter is emphatically not a view to dismiss out of hand. It explains a great deal, some of it better than plate tectonics does. That I don't believe it comes from the lack of any evidence whatever for expansion on the Moon, where much very early surface is available. Carey dismisses this offhandedly as an early stage, but that won't do without justification. Direct measurement should settle the whole question shortly.

The book itself is a good discussion of his views and their history, including extensions into cosmology which have interested physicists. -LMV

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Sedimentary Environments and Facies. Edition 2. Edited by **H.G. Reading**. 1986. Blackwell Scientific. xi + 615 pp. ISBN 0-632-01572-1 hardbound, $90.00. ISBN 0-632-01223-4 softbound, $47.50.

This has been one of the two standard treatments of the subject and it's good to see it updated. It emphasizes both the causal processes leading to the formation of the various kinds of sedimentary rocks and the inverse problem, criteria for recognizing the depositional environment from the preserved sediments. It is quite well illustrated and is clearly written. A minor difficulty with the organization is the omission of discussion and criteria for recognition of volcanigenic sediments, of which altered ash falls (bentonites) are the most relevant to the book's purpose. That presumably reflects the traditional geological classification of rocks, but such material does often occur in sedimentary rock, is relevant in environmental reconstruction, and is really sedimentary. A chapter on the relation of tectonics to sedimentation is a useful feature that is included. -LMV

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Eocene Molluscan Paleontology of the Whitaker Peak Area, Los Angeles and Ventura Counties, California. **Richard L. Squires**. 1987. Natural History Museum of Los Angeles County (their Contributions in Science 388). 93 pp. No ISBN. Softbound. $23.00.

There are several non-mollusks too. The area is new and the stage has been rather sparsely studied, so there are several new species. The study has critical comparative remarks for individual species as appropriate. -LMV

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Selected Studies in Carboniferous Paleontology and Biostratigraphy.
Edited by **Paul L. Brenckle, H. Richard Lane**, and **Walter L. Manger**. 1987. Forschungsinstitut Senckenberg, Senckenberganlage 25, D-6000 Frankfurt/M. 1, W. Germany (their Courier 98). 206 pp. ISBN 3-924500-35-5. Softbound. DM 48.00 (about $25).

Most of the emphasis here is biostratigraphic, with a number of proposals for zonation and correlation. Other groups have some consideration but conodonts get most attention. Incompatible zonations by corals and conodonts are shown to be due to facies-dependence by the corals; the conodont zonation is by a phylogeny while the corals merely immigrate. Another paper considers the synchroneity of a phyletically controlled first appearance to be doubtful, as it may be in this case, but the author proposes instead reliance on an apparently rather widespread sabkha facies which he thinks he validates by its relationship to that very origination! A third paper reminds us that even some conodonts are related to facies. Some large foraminiferans get a good review, but the foram highlight is the demonstration that some supposed agglutinated forams are actually encysted mating groups of single smaller species. This mating behavior was previously known only in the Cenozoic; it apparently originated in a number of lineages in the Carboniferous. It seems like quite a beneficial arrangement; why isn't it more common, in other groups too? -LMV

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A Trip Through Time: Principles of Historical Geology.
John D. Cooper, Richard H. Miller, and **Jacqueline Patterson**. 1986. Merrill. x + 469 pp. ISBN 0-675-20140-3. Hardbound. $34.95.

Rather, a provincial trip through North America. The rest of the world is barely mentioned. One does have to make compromises in a very elementary book like this, and the restricted scope does allow more consideration of processes and regional history, getting closer to the rocks, than would be possible in a more inclusive book. A third of the book is on general principles of historical geology and aspects of evolutionary biology, followed by the temporal presentation. There is considerable emphasis on sketches in the history of geology. Plant and mammal evolution have separate chapters. The historical physical geology is competently done, but the same can't be said for the paleontology, which occupies a good part of the book. Despite usually good reconstructions of organisms and communities, there are so many major errors in the paleontology that I cannot recommend the book to anyone. -LMV

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Plankton and Fisheries.
John Grahame. 1987. Edward Arnold, 3 E. Read St., Baltimore, Md. 21202. (iv) + 140 pp. ISBN 0-7131-2941-7. Softbound. $16.95.

In order to understand fisheries one needs to understand the food web of which they are a part. (A part, not something superposed on a separate system.) The marine food web itself is the focus of this introductory but thoughtful book. Grahame appropriately emphasizes plankton and their dynamics. The vertical water circulation gets due treatment, even explaining why phytoplankton remain susceptible to an inhibition of photosynthesis by destruction of pigments by near-surface light intensities. The names of the algae aren't always current, though; primary production from small algae isn't given its due; and the discussion of energy is confusing because free energy isn't distinguished. The use of commas for semicolons should have been caught. Fishes and whales are treated, like the rest, in a succinct and incisive way; I hope the book sees wide use. -LMV

Diseases and Plant Population Biology.
Jeremy J. Burdon. 1987. Cambridge Univ. Press. viii + 208 pp. ISBN 0-521-30283-8 hardbound, $49.50. ISBN 0-521-31615-4 softbound, $19.95.

Not all herbivores are macroscopic. A rust differs from an aphid or a rabbit in its mode of attack, and thus in the kinds of defenses possible, but the results can be similar. The widespread neglect of parasites in ecology is odd, and as we see the boundary between a parasite and a herbivore is fuzzy. Burdon is successful in bringing plant pathology into an evolutionary context, although the subject is too new with respect to natural populations for it really to know where to go. He deals with ecological and genetic aspects about equally, with emphasis on both the hosts and the parasites. There is much opportunity for real coevolution, including a common sort which should be better known: an arms race between parasite and host which is based largely on a single gene in each. Costs of resistance and virulence are emphasized appropriately. Theory and examples are nicely balanced. The distinction between parasites and commensals, and its environmental and host-state dependence, isn't clarified, though; the microflora of leaves would have been a good way to deal with this. A brief section on community effects merely whets the appetite. An exciting book in a young subject. -LMV

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Population Ecology: A Unified Study of Animals and Plants. Edition 2.
Michael Begon and **Martin Mortimer**. 1986. Blackwell Scientific. vii + 220 pp. ISBN 0-632-01443-1. Softbound. $19.95.

It is good to have an updated version of this standard text. As the subtitle indicates, it does treat animals and plants together, although microorganisms are ignored. The emphases are conceptual and illustrative; equations are brought in to clarify, not to rule. A short chapter on communities barely touches the subject. The expected topics are treated well, although one can quibble on space alloted. One real criticism, of the subject more than the book: although modular organisms are recognized, they are then forgotten and the book simply assumes that "individuals" of one sort or another are the proper units in population ecology.
-LMV

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Serpentine and Its Vegetation: A Multidisciplinary Approach.
Robert Richard Brooks. 1987. Dioscorides Press, 9999 SW Wilshire, Portland, Ore. 97225. (viii) + 454 pp. + 48 plates. ISBN 0-931146-04-6. Hardbound. $47.50.

Serpentine is loved by more than geologists: Not by plants, which find it hard to grow on, but by the botanists who study the many endemics which do manage to thrive or at least survive there. Geologists then sometimes use these plants (or undervegetated areas) to recognize buried serpentine rock. Because of Brooks's title I have just used "serpentine" in a confusing way, as a synonym of "ultramafic rock". Such rock, high in Mg and Fe, characterizes deep volcanic belts. Soils derived from it have high concentrations of some elements inimical to plants and low concentrations of nutrients and clay. The endemic plants seem merely to be those which can survive under such conditions; perhaps none do best there, being excluded elsewhere by the competitive effects of non-ultramafic plants and by parasitism of fungi, which are sparse in ultramafic soil. The serpentine animals studied seem just quasi-specialists on serpentine plants, and there is no indication that the microbiota has been studied as such. The book itself synthesizes the geological (including geochemical), biological, and geographical aspects in a thorough way. Most of the book is in fact a rather detailed account by region of the ultramafic rocks of the terrestrial world and their vegetation. Not only the first book on the subject but an outstanding achievement. -LMV

Ecology and Tropical Biology.
Ian Desmukh. 1986. Blackwell Scientific. xii + 387 pp. ISBN 0-86542-316-4. Softbound. $29.95.

This book can be briefly characterized as a short, tropical, readable Odum. I mean this positively: Odum books are good if you can read them. Desmukh's version is a general book on ecology in which the examples and focus of interest are tropical, especially African. Aquatic examples and topics are excluded. The treatment is elementary but sound, and it discusses so many examples that occasionally it reads a bit like a review, which it isn't. A quarter of the book is on human ecology: food and agriculture, populations and disease, and conservation. This is treated dispassionately but without abandoning ecological reality, and its message is the stronger for doing so. There is much in the book for the professional, in the way of documented cases, but it is written at a level accessible to a nonbiologist. I hope that many such will indeed access it. -LMV

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The Ecology of Temporary Waters.
D. Dudley Williams. 1987. Timber Press, 9999 SW Wilshire, Portland, Ore. 97225. (ix) + 205 pp. ISBN 0-88192-081-9. Hardbound. $36.95.

Organisms which live in water which then evaporates or flows away obviously have a problem. There are a lot of habitats like this, though, and they have their share of specially adapted indigenes. The strategies are to be active in air (rare), enter dormancy, or disperse. This book describes, at an introductory level, the kinds of habitats involved, their inhabitants (emphasizing arthropods), and relevant adaptations. There has been a moderate amount of experimental work done on communities in temporary waters, although not nearly the proportion they would seem to deserve; this is quite omitted, even in a chapter on suggestions for studies. The book is nevertheless a useful background even though the print is too light for comfortable reading. -LMV

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Populations of Plant Pathogens: Their Dynamics and Genetics.
Edited by **M.S. Wolfe** and **C.E. Caten**. 1987. Blackwell Scientific. viii + 280 pp. ISBN 0-632-01433-4. Hardbound. $90.00.

The book is supposed to integrate ecological and genetic work but, as usual, they remain mostly separate. Almost all the papers deal with agricultural situations, which are now a major set of habitats and really shouldn't be so ignored by ecologists. The subject of population plant pathology itself seems to be groping out of ad hoc and typological approaches into something resembling population biology, but it isn't there yet. The book is nevertheless useful as indicating the state of the subject and also for giving a number of case histories. Theoretical population geneticists (and others) may want to consider features of some fungi which make standard analyses inappropriate. There are also a number of fungicides, apparently less potent ones, to which no fungus has evolved resistance. It is apparently unknown why not. A fascinating review on Dutch elm disease shows that it is caused by two species (called strains or subspecies, one possibly arising diphyletically from the other, although this is my inference). One is more aggressive than the other and has consistently outcompeted it (an example of herbivore competition, but the resource doesn't remain abundant). It is worldwide and affects many species of elms. The author isn't optimistic on reduction of virulence even though he thinks that such a change would be advantageous, which would on present evidence be true only for populations because the mutualism between the fungus and its beetle carrier thrives on dead and moribund trees. -LMV

Invertebrate-Microbial Interactions: Ingested Fungal Enzymes in Arthropod Biology.
Michael M. Martin. 1988 (stated 1987 in book). Cornell Univ. Press. xii + 148 pp. ISBN 0-8014-2055-5 hardbound, $32.50. ISBN 0-8014-9459-1 softbound, $14.95.

Almost no animals produce an enzyme which can digest cellulose, yet this ability is common among bacteria, protists, and fungi. (An odd circumstance. What keeps animals from synthesizing their own? Calling it a constraint just names it; do animals avoid that part of the protein-configuration space for some other reason?) The use of living microorganisms for the purpose in special regions of the gut has evolved quite a number of times, as is well known. But there is another way. Martin discovered that several groups of arthropods are able to use cellulases remaining functional in the residue of digested fungal cells. The book is an account of his work on this remarkable phenomenon, and it is a good read. Most of the arthropods use the enzymes for their own food, but fungus-garden ants egest the enzymes in a way which promotes the growth of the fungus they eat. Thus the fungus uses the ants as a means of extracellular digestion as well as in other ways, although the selection originating the process must have been by the resulting benefit to the ants. -LMV

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Insects and the Plant Surface.
Edited by **Barrie Juniper** and **Richard Southwood**. 1986. Edward Arnold, 3 E. Read St., Baltimore, Md. 21202. viii + 360 pp. ISBN 0-7131-2909-3. Hardbound. $69.95.

Before an insect can feed on a plant it must penetrate the surface, avoiding any surface defenses while doing so. Some plants feed on insects, too, and the interaction is again via the surface. So are mutualistic interactions. The book itself is a collection of examples of such phenomena, useful but rather plodding. There is oddly no discussion at all of root surfaces, which are rather different from others and where much interaction occurs. But roots haven't been studied much from any perspective; there is a lot to learn. One chapter stands out, where Lawton proposes that plant surfaces are fractal (and even self-similar in effective dimensionality at different scales) and that this indirectly causes the greater number of small herbivores. -LMV

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The Biology of Symbiosis.
D.C. Smith and **A.E. Douglas**. 1987. Edward Arnold, 3 E. Read St., Baltimore, Md. 21202. xi + 302 pp. ISBN 0-7131-2939-5. Softbound. $29.95.

Symbiosis is here used in its original sense of the living together of different organisms, thus including parasitism and excluding brief mutualistic interactions like pollination. Despite its title the book itself is restricted to cell-level interactions, mostly mutualistic, with at least one partner a microorganism. There's still a lot to cover. The approach is by separate coverage of different symbiotic partnerships, with emphasis on physiology and function. The experimental viewpoint predominates and there are brief accounts of quite a diversity of experimental work. I hadn't even known of all the partnerships discussed. There are good figures of the expected kinds and there is some attention to what we don't know. A short discussion of principles concludes the book. The authors have the odd view, which actually seems to be common among physiological workers, that the algal partners in lichens normally have their fitness decreased by lichenization. But of course the algae wouldn't be living at all where the lichens do, so their fitness is enhanced. But it's a good book overall and is probably the best introduction to its actual subject. -LMV

Population Ecology of Individuals.
Adam Łomnicki. 1988. Princeton Univ. Press. x + 223 pp. ISBN 0-691-08471-8 hardbound, $45.00. ISBN 0-691-08462-9 softbound, $13.95.

That individuals of a population are not equivalent to each other has usually been ignored in ecology except for parameters like sex and age. They haven't been ignored as much as Łomnicki implies, though, and the book tends in places to reinvent wheels, as with the argument that population regulation can occur by selective dispersal of competitively inferior individuals, or alternatively by maximizing expected lifetime (rather than momentary) fitness, as is believed to be the case for the apparently excessive territories of lions. Some individuals are more fecund than others, some are more resistant to heat, some are smaller, some move more, some win in contests, and so on. It is differences like these that Łomnicki deals with, and the book gives refreshing treatments of a number of problems for which such differences are relevant. He uses models as tools rather than as crutches and is usually appropriately careful about their limitations. He even gives a new derivation for the logistic equation which results in a large conceptual reinterpretation of it. The individual-oriented approach isn't carried very far in this book, but it is good to have it brought forcefully to our attention. For instance, population-level parameters like $\underline{r}$ can be reinterpreted and measured for individuals, as I have done in unpublished work with Drosophila. The boundaries of individuals aren't obvious when our criteria of individuality conflict. Body size is important in more ways than discussed here. The next level, reinterpreting processes in an energetic framework, isn't touched on. And so on. Let's hope the book stimulates much more in the way of explicit consideration of variation in ecology; microcausation can have unexpected effects at higher levels.
-LMV

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Ecology of Microbial Communities. (Soc. Gen. Microbiol., Symp. 41.)
Edited by **M. Fletcher, T.R.G. Gray**, and **J.G. Jones**. 1987. Cambridge Univ. Press. x + 440 pp. ISBN 0-521-33106-4. Hardbound. $74.50.

It may be that microbiology will provide simple models to stimulate ecology as it did molecular biology, but we aren't there yet. I don't know whether the main difficulty is technical (it's even difficult to know what organisms are there) or from lack of imagination. In a feisty introductory chapter Brock characterizes a large proportion of work in the field as effectively useless. Most of the remaining chapters are more or less descriptive reviews of various aspects of the field; they don't cover it completely. Despite my own greater interest in soils I found the richest material in chapters on aquatic environments, e.g. in the response of bacteria to patches such as normally leaky planktonic algae, that the amount and ratios of nutrients are both important for niche separation, and a depiction of the trophic structure of mats of blue-green algae in hot springs. Karl presents the now heretical view that chemoautotrophy in deep-sea vents may be less important there than heterotrophy depending ultimately on photosynthesis — but, if so, why such a concentration of organisms on the normally sparsely settled sea floor? -LMV

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The Ecology of Woodland Rodents. (Symp. Zool. Soc. London 55.)
Edited by **J.R. Flowerdew, J. Gurnell**, and **J.H.W. Gipps**. 1986 (stated 1985 in book). Oxford Univ. Press. ISBN 0-19-854003-5. Hardbound. $79.00.

This book concentrates on two species of mice in western Europe, Apodemus sylvaticus and Clethrionomys glareolus, thereby complementing other recent volumes on similar species. A few other species, especially A. flavicollis, are discussed occasionally. The reviews are well done and provide a good overview; none provoke excitement. They cover genetics, behavior, communication, diet, individual energetics, parasites, predation, home range and dispersal, fluctuation and regulation of populations, and coexistence and community structure. The view advocated here that island races originated predominantly by drift is suspect, partly from later theoretical and experimental work but partly from results then available on the rate of regeneration of quantitative genetic variance. Most papers, by covering more than one species, make explicit comparisons between the rather different animals. Stenseth appropriately advocates intensive work on species like _C. glareolus_, comparing populations from areas where they cycle with those where they don't. Is competition involved in the coexistence? It sometimes occurs, even between the most unlike species discussed, but not necessarily in a way to fortify their niche separation. Mostly this seems to be from the ghost of the Pliocene, but we aren't given data on what happens when one species is alone on an island. Does its realized niche expand at once or in evolutionary time? Some authors remind us that ecological and evolutionary time can overlap. -LMV

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Dynamics of Marine Fish Populations.
Brian J. Rothschild. 1987 (1986 on title page). Harvard Univ. Press. xv + 277 pp. ISBN 0-674-21879-5. Hardbound. $37.50.

As is well known, fish populations sometimes change in ways which don't fit our preconceptions. And _post hoc_ correlations with environmental variables have a distressing habit of coming apart soon after they are made. The problem is bad enough that appeal has been made to that model of last recourse, chaos. Rothschild doesn't mention chaos, but his approach may be a way out. He decomposes the diverse influences on a fish population somewhat in Holling's tradition, with particular emphasis on positive and negative density dependence and ways in which environmental signals can sometimes have an effect different from their usual one. The treatment is mathematical in spirit but mostly in a qualitative way; the few equations don't lend an air of false precision to a subject whose very structure is still being discovered. Whether further development of this approach will need unobtainable data in order to be of real use to fisheries remains to be seen, but I think that it does embody the germ of understanding. -LMV

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Subtidal Ecology.
Elizabeth M. Wood. 1987. Edward Arnold, 3 E. Read St., Baltimore, Md. 21202. iv + 125 pp. ISBN 0-7131-2957-3. Softbound. $11.95.

A valuable introductory treatment, focusing on Britain but not unduly so although restricted to temperate seas. The approach is community-level, including species interactions affecting the community. An unexpected chapter on the pelagic biota complements those on the benthos. The author's touch is less sure for the former organisms, e.g. on phytoplankton productivity.

I had naively accepted the common statement elsewhere that eukaryotic cells don't differ much in size, while noting a few exceptions like eggs, coenocytes, and _Acetabularia_ on the one hand and rotifers on the other. A figure in this book, which I checked, inadvertently shows that cells of protists are often orders of magnitude larger than those of metazoans. A comparative survey would be quite a useful contribution, as would an adequate theory. -LMV

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Methods in Plant Ecology. Edition 2.
Edited by **P.D. Moore** and **S.B. Chapman**. 1986. Blackwell Scientific. xiii + 589 pp. ISBN 0-632-00996-9. Softbound. $55.00.

The title is almost self-explanatory. Subjects for which methods are given range from population ecology to soil and chemical analyses to a cursory look at some statistics. Some chapters review aspects of the biology, but methods always receive first emphasis. Usually the reader will benefit from more detailed descriptions in the original literature, but the book provides a good orientation as to what to look for (and what to look out for) and can be warmly recommended as at least a place to start. It is quite different from the first edition even in subjects included. -LMV

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Community Ecology: Pattern and Process.
Edited by **Jiro Kikkawa** and **Derek J. Anderson**. 1986. Blackwell Scientific. xi + 432 pp. ISBN 0-86793-264-3 hardbound, $56.00. ISBN 0-86793-272-4 softbound, $35.00.

Community ecology is an immature science and this book reflects the fact even more than it needs to. By immature I mean that it too often seems not to know where it is going. Most of the chapters in the book are predictable surveys of predictable topics; two or three are pretty good, though. And the book as a whole is a useful introduction to much of the subject. Conspicuously lacking are considerations of energy (etc.) flow and regulation, food-web structure (peripherally touched on), and aquatic communities except for a few pages on decomposers. The book is largely phenomenological rather than theoretical, which I don't mean as an adverse comment except for the initial overviews. The final chapter is anthropological, classifying ways people live in rain forests. -LMV

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A Reassessment of Reptilian Diversity Across the Cretaceous-Tertiary Boundary.
Robert M. Sullivan. 1987. Natural History Museum of Los Angeles County (their Contributions in Science 391). 26 pp. No ISBN. Softbound. $6.00.

The heart of this paper is a critical, even hypercritical, worldwide survey of the occurrence of reptile taxa around the end of the Cretaceous. This is quite a useful review even though it misses some data (dinosaur records from teeth in channels of the Hell Creek Formation, or the ?latest plesiosaur, from the type Maestrichtian). Stratigraphic data outside North America are often from sometimes inaccurate secondary sources. Superposed on this valuable treatment is an overdrawn argument against a mass extinction on land between the Cretaceous and the Paleogene. Partly this is from a narrow view of mass extinctions: all at once or it wasn't "mass". Partly it is from biased argument: that most species of a taxon occur before the late Maestrichtian implies an appreciable decline in species diversity by then, even though the intervals are very unequal, or a similar diversity of lizards in Maestrichtian and Paleocene implies a lack of the known extinctions, or that the stratigraphically latest discovery is really the last occurrence even for rare taxa. And partly it is from a strict adherence to the more dogmatic type of cladistic systematics; adaptive diversity is of no importance whatever to them, even, it seems, in discussing extinction. -LMV

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The Galaxy and the Solar System
Edited by **Roman Smoluchowski, John N. Bahcall,** and **Mildred S. Matthews**. 1986. Univ. Arizona Press. xii + 483 pp. ISBN 0-8165-0982-4. Hardbound. $29.95.

This book was motivated by the reports of periodicities in extinctions and impact craters. The several proposed astronomical causes are examined in detail by a number of authors, and the evidence for the periodicities themselves also is considered. There is no consensus by these astronomers on anything in the current dispute, including the existence of the periodicities hemselves. The nature of the evidence and the ways in which it can be used are well brought out; the book is accessible to non-astronomers even though it is billed as the first book to examine the influence of our galaxy on the Solar System. Some geological input here and there could have helped; e.g., one author makes an argument for the latest glaciation having been caused by a comet breaking up and spreading dust around. -LMV

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Hawai'i's Terrestrial Ecosystems: Preservation and Management.
Edited by **Charles P. Stone** and **J. Michael Scott**. 1986 (copyright 1985). Univ. Hawaii Press. xxviii + 584 pp. No ISBN. Hardbound. $22.50.

Hawaii has not been noted for conservation. With an economy based on agriculture, military bases, and beach-oriented tourists its remarkable biota hasn't seemed important to enough people. The situation has changed recently, although with over 2000 introduced arthropods, 4600 plants, cats, mongooses, pigs, goats, mice, rats, and the rest already there and mostly a permanent part of the biota. There are still problems with introductions, and indeed the only mention of <u>Bufo marinus</u> (a problem species elsewhere) in the book is as a recommendation for cockroach control. One interesting conclusion from exclosure studies is that alien plants don't have much success unless the native vegetation has been damaged by alien vertebrates. The book gives valuable and extensive evaluations of the native biota and invaders, with extensive and frank discussion among various interested groups on what to do. There is some hope, although no mention is made of human population increase. -LMV

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Forest-Bird Communities of the Hawaiian Islands: Their Dynamics, Ecology, and Conservation.
J. Michael Scott, Stephen Mountainspring, Fred L. Ramsey, and **Cameron B. Kepler**. 1986. Cooper Ornithological Society (their Studies in Avian Biology 9). xii + 431 pp. No ISBN. Softbound. $26.50.

There is community ecology here, but most of it will have to be extracted by diligent readers. Nevertheless the book is a remarkable summary of a remarkable series of censuses in difficult terrain. For all 57 species, native and introduced, there are maps and tables of density in relation to habitat. A conclusion that competition is usually unimportant relies on usually positive covariation in densities, but this sort of analysis necessarily ignores both niche partitioning and effect of response to habitat variables. There is a great amount of useful information, though. Hawaii is noted for its agriculture rather than for conservation, with half or probably much more of its original avifauna eliminated by the Polynesians, half of the rest by later invaders, and an unusually large proportion of the survivors having very few individuals. The survey was for conservation, and this aspect and related topics receive an exemplary treatment. I hope it signals a rather more general change in attitude; preserving birds preserves the habitat they live in, and it is difficult for a layman to see why an endangered beetle or nematode is worth saving, even if we knew it were there. -LMV

The Freshwater Fishes of Europe. Volume 9. **Threatened Fishes of Europe.**
Anton Lelek. 1987. Aula-Verlag, Postfach 1366, D-6200 Wiesbaden, W. Germany. 0 + 343 pp. ISBN 3-89104-048-2. Hardbound. DM236 (about $125).

"Species that occur very rarely and exceptionally. . . are considered as extinct." They therefore are not mentioned unless they are more abundant somewhere. Mostly the book is for managers, not for scientists. There is nevertheless a summary of reasons for population decline whenever known or reasonably inferred. Fishes have a strange environment which includes fish hatcheries and their activities. As a result some species and races are introduced in large numbers in places where they haven't occurred or have declined, with the usual variable effects on the indigines. In addition to short general treatments of the fauna and of recommendations, there are short sections on habits, distribution (often with a map), recognition, and aspects of conservation for each species treated. In many cases too little is known to make a recommendation; sometimes even the taxonomy is unclear. Probably the information is needed now, but if the volume were chronologically as well as numerically the last in the series it would perhaps benefit from the syntheses by others. The author does appropriately emphasize the conservation of habitats rather than merely of species in isolation. -LMV

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Pharmacopées Traditionelles en Guyane: Créoles, Palikur, Wayãpi.
Pierre Grenand, Christian Moretti, and **Henri Jacquemin**. 1987. Paris: Éditions de l'ORSTOM (their Mémoire 108). 0 + 569 pp. + many colored plates. ISBN 2-7099-0803-4. Softbound. 280 F (about $44).

One of the less-emphasized reasons for the conservation of tropical forests and other habitats is the likelihood of discovery of new drugs or the recognition of others used locally. The inhabitants of these forests have used various species for one purpose or another, but (despite some more or less well-known examples of adoption elsewhere) their knowledge has tended to be ignored. The effective extinction of whole cultures and even peoples continues and has undoubtedly resulted in the permanent loss of medically useful information in addition to the more obvious consequences. The French in particular have recently made considerable effort to collect such information; this book records the results from what used to be called French Guyana. After an ethnological introduction, about 500 species are discussed individually with respect to traditional (by culture group) and scientific pharmacologies. Almost all are angiosperms, including some introduced and cultivated species; there are six pteridophytes, two fungi, and no algae or animals. More than a hundred species have colored photographs of living plants or fruits, while there are drawings of others. -LMV

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Bibliographie Ornithologischer Bibliographien II.
Helga Brasseler. 1987. Forschungsinstitut Senckenberg, Senckenbergsanlage 25, D-6000 Frankfurt/M. 1, W. Germany (their Courier 99). 214 pp. ISBN 3-924500-36-3. softbound. DM 37.50 (about $20).

The criterion for inclusion of a reference is a bibliography with at least 100 entries. Most are from longish papers. There are suitable indices but they are very incomplete and even inconsistent from one to another. It is surprising that there is no entry for weights, a subject in dire need of compilation if there ever was one and on which there has been some recent effort in that direction. The previous bibliography[2] was Courier 72 (1984); the present one is longer and includes earlier as well as later references. -LMV

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Classification of Southern African Mammals.
J.A.J. Meester, I.L. Rautenbach, N.J. Dippenaar, and C.M. Baker. 1986. Pretoria, South Africa: Transvaal Museum. (viii) + 359 pp. + folded chart loose at end. ISBN 0-907990-06-1. (Transvaal Museum Monograph 5.) Hardbound. $40.00.

This book is for professionals. Its scope is Namibia and the area south of the Zambezi River, with the adjacent ocean. There are no figures, except for a large chart summarizing the classification. The book expands and updates the 1953 work of Ellerman, Morrison-Scott, and Hayman.

There are keys down to the species level (even to suborders, I assume from habit rather than expected need) but no other morphological information is given, not even diagnoses of taxa. There are, however, complete synonymies at the generic and lower levels, verbal summaries of geographic distributions, and discussions of taxonomic matters where there has been disagreement in the relatively recent literature. It is all quite well done and will be the standard for many years. Most of the world still lacks comparable authoritative summaries.

There is reportedly already a supplement available at $7.00, but this was not sent for review. -LMV

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South American Land Birds: A Photographic Guide to Identification.
John S. Dunning. 1982. Harrowwood Books, 3943 N. Providence Rd., Newtown Square, Pa. 19073. xvi + 364 pp. ISBN 0-915180-21-9 hardbound, $39.50. ISBN 0-915180-22-7 softbound, $39.50 too, it says, but I suppose somebody goofed.

There are more species of birds in South America than in any other continent, and Dunning has given his own photographs here of more than 1100 of them. These species and the rest each have a very brief description and a range map, the latter quite a useful adjunct beyond mere identification. There is also an indication of general habitat. Each photograph is of a live bird on a seminatural perch soon after mistnetting (except, presumably, for the rhea and such). The result is an extraordinary collection and a record of occupants of vanishing habitats. Unphotographed species are in a separate section at the rear. This is not always well referred to from the front section and it has no direct reference back to the front. With one or two other recent books, this one makes the kaleidoscope of South American birds reasonably accessible. -LMV

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Contributions to the Knowledge of the Cichlid Fishes of the Genus Aulonocara of Lake Malawi (East-Africa).
Edited by **Wolfgang Klausewitz.** 1987. Forschungsinstitut Senckenberg, Senckenberganlage 25, D-6000 Frankfurt/M. 1, W. Germany (their Courier 94). 139 pp. ISBN 3-924500-32-0. Softbound. DM 33.00 (about $17).

Most of this volume on an actively speciating fish genus is devoted to tables of measurements and bivariate statistical descriptions. The detail provided is potentially valuable, but almost no use is made of it here. Parts of the bivariate statistics are flawed by an inadequate account of exactly what is being done and by using regression (with one variable taken as independent) rather than coregression. A method proposed for comparing regressions (not for testing differences) uses only confidence intervals and therefore is nearly worthless because it depends on sample size. Real systematics, including a little phylogeny, does manage to fill in the interstices; there are even color photographs showing, among other things, large color differences among males from different parts of the lake. -LMV

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Les Reineckeiidae (Ammonitina, Callovien) de la Téthys Occidentale: Dimorphisme et Évolution. Étude a partir des Gisements du Centre-ouest de la France.
Elie Cariou. 1984. Dept. Sciences de la Terre, Université Claude Bernard, 43 bd. du 11 novembre, 69622 Villeurbanne, France (their Documents des Laboratoires de Géologie, Lyon, hors-série 8). 2 volumes, 0 + 599 pp. total. ISBN 2-85454-044-1. Softbound. 460 F (about$75).

Ammonites are one of the most promising groups for the detailed study of evolution in the fossil record, but they haven't been exploited much for that purpose. Cariou has given one of the better examples. Most of the monograph is a detailed revision of one subfamily as it is represented in the Callovian (part of the middle Jurassic) of France. Variation and other aspects are considered, but there is no Brinckmann-style documentation of evolution within species. There is much well-preserved material, well figured, from the middle 19 of the 23 stratigraphic subdivisions Elie gives for the stage. The entire evolution of the group occurs in this short interval. The phylogeny is worked out in detail, although there is one case where two forms are called subspecies although they overlap in space and time. Elie does not use whatever information specimens and species from elsewhere may contribute. Phyletic branching, but not phyletic change, is concentrated in four short intervals, which Elie says also happens at the same times for other ammonite families. One genus has a sequence of five successive species with no branching and then a rapid radiation. There are examples of various evolutionary trends, continued and reversed and parallel, recapitulation, proterogenesis, and paedomorphosis. The three cases of the latter occur simultaneously. Elie says they happen abruptly, but in each case there is a gap of a stratigraphic interval where it occurs; the change is nevertheless more rapid than others, as Hallam has noted for other cases. Dimorphism, presumably sexual, is well developed and the morphs evolve in the same way whenever this can be determined, again as in other ammonite groups. A good, solid study. -LMV

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A Synopsis of the Avifauna of China.
Cheng Tso-hsin (Zheng Zuo-xin). 1987. Paul Parey. xvi + 1223 pp. ISBN 3-490-12518-3. Hardbound. $163.00.

The function of this book is to give a critical list of the birds of China, to subspecies, with as much detail on distribution as is feasible. The taxonomy is good, although the index omits some questionable forms or former names given in footnotes. Breeding habitat is noted for each species. Each subspecies has a description of breeding, migration, and winter ranges in China, usually with actual localities marked on a map, and an indication of relative overall abundance. A summary table provides data on the distribution of each subspecies in 16 regions. While this book was in press Meyer de Schauensee published a somewhat similar one; they differ in detail but each is a valuable source. -LMV

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Artunterschiede an den Langknochen Grosser Artiodactyla des Jungpleistozäns Mitteleuropas.
Thomas Martin. 1987. Forschungsinstitut Senckenberg, Senckenberganlage 25, D-6000 Frankfurt/M. 1, W. Germany (their Courier 96). 124 pp. ISBN 3-924500-33-9. Softbound. DM 20.00 (about $11).

There are detailed descriptions, measurements, and figures for the long bones of two bovids and three cervids. A good reference. -LMV

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L'Évolution chez les Ammonitina du Lias Moyen (Carixien, Domérien Basal) en Europe Occidentale.
J.L. Dommergues. 1987. Dept. des Sciences de la Terre, Université Claude Bernard, 43 bd. du 11 novembre, 69622 Villeurbanne, France (their Documents des Laboratoires de Géologie, Lyon, 98). 297 pp. ISBN 2-85454-037-9. 250 F (about $45).

This study deals with several groups of early Jurassic ammonites, collected with fine stratigraphic resolution from a number of areas. The stratigraphy itself and paleobiogeography have thereby been improved. The main focus, though, is on the ammonites themselves: their detailed phylogenies and evolutionary patterns. In this connection Dommergues explains for French readers the unnecessary and confusing terminology which Gould and Vrba proposed for adaptation; he advocates its use. The phylogenies themselves seem well founded and indicate, as is common for ammonites, several examples of heterochrony of different sorts and of perhaps sexual dimorphism, which is regarded as niche partitioning. Because attention is restricted to the geographic area studied it is difficult to know what proportion, if any, of the many apparently sudden speciations actually represent speciation rather than immigration. The author tries to distinguish among several kinds of speciation from his data, but partly for this reason I do not think he succeeds. Also, relative scarcity of specimens does not imply a genetically effective bottleneck, despite Stanley's uninformed claim to that effect which Dommergues relies on; genetically large populations can be even invisible paleontologically because of the small proportion fossilized and the very small numbers required for a bottleneck. The near absence of stasis does, however, appear to be well founded. So the monograph is good in parts, and the author is to be commended for trying to fit his material into a general evolutionary framework. -LMV

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Aspecte der Ornithologie.
Edited by **Dieter Stefan Peters** and **Wolfgang Wiltschko**. 1987. Forschungsinstitut Senckenberg, Senckenberganlage 25, D-6000 Frankfurt/M. 1, W. Germany (their Courier 97). 155 pp. ISBN 3-924500-34-7. Softbound. DM 40.00 (about $21).

A symposium, and rather a mixed bag. There is even a paper on the genetics of housemouse behavior in eliciting attacks by a predaceous bat. About half the papers deal with migration, from both physiological and geographic perspectives. One of these gives evidence on single-cell responses to magnetic fields and their relation to pineal function in both rodents and pigeons. An Eocene skeleton from Messel confirms the disputed view that flamingos belong to the Charadriiformes. -LMV

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Arikareean, Hemingfordian, and Barstovian Mammals from the Miocene Colter Formation, Jackson Hole, Teton County, Wyoming.
Anthony D. Barnosky. 1986. Carnegie Museum of Natural History, 4400 Forbes Ave., Pittsburgh, Pa. 15213 (their Bulletin 26). 1986. 69 pp. No ISBN. Softbound. $20.00.

Three small faunas contain both large and small species. Several species of lagomorphs and rodents are new, and for these and some others comparisons are made with relatives elsewhere. The stratigraphy is carefully done and there is a correlation chart for mammaliferous Miocene formations of the western United States and Canada. A competent treatment of mostly scrappy material. -LMV

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Systematics, Functional Morphology and Macroevolution of the Extinct Mammalian Order Taeniodonta.
Robert Milton Schoch. 1986. Peabody Museum, Yale Univ. (their Bulletin 42). xii + 307 pp. ISBN 0-912532-04 (sic). Softbound. $20.00.

Since Patterson's paper of 1949 taeniodonts (an odd group of Paleocene and Eocene mammals of North America) have been regarded as a good example of quantum evolution. They are a good example, in fact a better one than Patterson realized, but Schoch emphasizes other things, mostly phylogeny and function. His systematics is not, I think, the last word; e.g., I doubt the validity of the recently proposed genus Huerfanodon, and Ectoganus lobdelli seems specifically distinct from its descendant E. gliriformis. Although, unlike most cladists, Schoch thus recognizes the reality of ancestors, he is inconsistent in this. There is no apparent reason to deny the classical phylogeny of Onychodectes to Conoryctella to Conoryctes (the three valid genera of the Conoryctinae, which Schoch unfortunately makes a family, or of Conoryctes to its contemporary Wortmania. Known species are available for each. Similarly, there is no apparent reason (despite misleading statements accompanying a cladogram) to deny the sequence Psittacotherium-Ectoganus-Stylinodon. Thus, although at the species level the phylogeny is now a little bushy (although probably not as bushy as Schoch would have it), there is still a single generic lineage in each subfamily without overlap in time. Oddly, Schoch returns one form to subspecific rank despite its co-occurring with its counterpart. I suspect that the two (in Stylinodon) are conspecific morphs, but this can't be demonstrated.

Otherwise I like the treatment, which is the only one available except for an earlier version in the unlamented microfiche version of the Geol. Soc. Amer. Bull. Descriptions and figures are thorough, as is stratigraphic discussion. So is a set of functional analyses, based both on reconstructions and on analogies. He thinks that their inferred habits of digging for food imply an upland habitat and thereby account for the usual rarity of specimens, but why a mammal can't root on a floodplain too isn't made clear. Adaptation does clearly evolve within the order, as is indicated. -LMV

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The Terrestrial Amphipods (Amphipoda: Talitridae) of Tasmania: Systematics and Zoogeography.
J.A. Friend. 1987. Australian Museum (their Records, Supplement 7). 85 pp. ISBN 0-7305-3622-X. Softbound. A$35.00 (about $28).

That amphipods as well as isopods have become fully terrestrial is not widely appreciated, probably because they are absent from Holarctica. They are widely distributed elsewhere, though, and are sometimes important in litter decomposition. This monograph on the Tasmanian species is predominantly systematic, with extensive descriptions, comparisons, and line drawings. Phylogeny is not explicitly derived, but a zoogeographic section says that the more derived genera occur in the less moist habitats. -LMV

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A Revision of the Genera Nanularia Casey and Ampheremus Fall (Coleoptera, Buprestidae, Chalcophorinae).
C.L. Bellamy. 1987. Natural History Museum of Los Angeles County (their Contributions in Science 187). 20 pp. No ISBN. Softbound. $5.00.

A general review of these small genera, including non-taxonomic aspects as known. -LMV

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Les Vertébrés Fossiles de la Formation Pisco (Pérou). Deuxieme Partie: **Les Odontocètes (Cetacea, Mammalia) du Pliocène Inférieur de Sud-Sacaco.**
Christian de Muizon. 1985 (1984 on title page). Éditions Recherche sur les Civilisations, 9 rue Anatole-de-la-Forge, 75017 Paris, France (their Mémoire 50). 0 + 188 pp. + 17 plates. ISBN 2-86538-113-7. Softbound. 206.48FF (about $32.50)

The phylogeny of whales is beginning to be understood, although there are still appreciable disagreements. Well-preserved specimens from the Pliocene of Peru provide the basis for the exemplary treatment here. Whales more or less isolate their inner ear acoustically to improve their hearing, and this is especially pronounced in odontocetes. De Muizon relies especially on progressive stages in this isolation for his phylogeny, although he uses other evidence also. One main conclusion is a splitting of the river dolphins into three families, although as I think that whales are oversplit at that level I return the Pontoporiidae to the Iniidae. De Muizon makes a good case, though, for separating the Platanistidae widely from the others, as the sole survivors of a large early radiation of odontocetes. There are other innovations here also, plausibly argued for and supported by good figures. -LMV

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Osteology of <u>Hypostomus</u> <u>plecostomus</u> (Linnaeus), with a Phylogenetic Analysis of the Loricariid Subfamilies (Pisces: Siluroidea).
Scott A. Schaefer. 1987. Natural History Museum of Los Angeles County (their Contributions in Science 394). 31 pp. No ISBN. $9.00.

Description of the skeleton of this rather central member of its family is followed by a careful, even though computtered, estimation of the broad phylogeny of the family. One subfamily is regarded as paraphyletic merely because it seems to lack shared derived character states; whether any member is phyletically closer than others to its presumed exgroup isn't pursued because of inadequate evidence. Schaefer regards this putatively paraphyletic grouping as useful but doesn't take the next step of looking critically at cladistic classification itself. -LMV

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Early and Middle Silurian Conodonts from Midwestern New South Wales.
Günther C.O. Bischoff. 1986. Forschungsinstitut Senckenberg, Senckenberganlage 25, D-6000 Frankfurt/M. 1, W. Germany (their Courier 89). 337 pp. ISBN 3-924500-27-4. Softbound. DM 65.00 (about $34).

Bischoff groups the conodont elements into apparently natural assemblages by critical use of several criteria. Two genera and quite a number of assemblage species are new, and a refined zonation emerges. A potentially valuable table gives the number of specimens of each element of each species found in each sample. The documentation throughout is extensive, including more than a thousand crisp photographs, some showing imprints of the cells which secreted the elements. Phyletic relationships are discussed as feasible. Intraspecific variation is barely considered, though, except for desription of three "morphotypes" of one putative species, the degree of discreteness of which is not indicated, There are also two sympatric "subspecies" described in another genus; what these really are is unclear, although they cannot of course be subspecies. -LMV

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Trilobites of the Upper Cambrian Sunwaptian Stage, Southern Canadian Rocky Mountains, Alberta.
Stephen R. Westrop. Canadian Soc. Petrol. Geol., #505, 206 7th Ave. SW, Calgary, Alta. T2P 0W7 (their Palaeontographica Canadiana 3). (vii) + 179 pp. No ISBN. Softbound. Can.$25.00.

There are several mass extinctions in the late Cambrian. These bound what are called biomeres and trilobites are particularly affected. The low-diversity fauna of more eurytopic survivors then diversifies until the next crunch. Westrop's exemplary treatment leaves such matters mostly for future work, but he does find that the extinctions are far from the abrupt events they have been thought to be from more southern sections. Not much in the way of obvious environmental changes accompany them, though. The faunas themselves are large and well treated, with critical remarks on taxa and stratigraphic zonation and an overly brief environmental analysis by biofacies. -LMV

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Les Serpents de la Guyane Française.
Jean-Philippe Chippaux. 1986. Éditions de l'ORSTOM, 70 route d'Aulnay, 93140 Bondy, France. 0 + 165 pp. ISBN 2-7099-0829-8. Softbound. 120 F (about $19).

This is primarily a taxonomic review of the poorly known fauna. (The localities, marked on maps for each species, are almost uniformly near the major towns and are clustered near the coast.) The treatment is critical and makes some innovations. Drawings of heads and usually maxillae accompamy the text. A graph of joint sex ratio of captures by month shows a marked peak, and a joint juvenile survivorship curve is given but is inadequately based. Tables list habitat and time of activity for each species. -LMV

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An Early Miocene Pinniped of the Genus <u>Desmatophoca</u> (Mammalia: Otariidae) from Washington.
Lawrence G. Barnes. 1987. Natural History Museum of Los Angeles County (their Contributions in Science 382). 20 pp. No ISBN. Softbound. $7.00.

The eared seals had a large radiation in the middle Cenozoic, part of which is the distinctive subfamily Desmatophocinae. Its second species, based on a fairly complete skull, is described here and merely makes the group more distinctive. -LMV

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Beiträge zur Fauna, Faunengenese und Zoogeographie des Nepal-Himalaya Arthropoda.
Edited by **Jochen Martens**. 1987. Forschungsinstitut Senckenberg, Senckenberganlage 25, D-6000 Frankfurt/M. 1, W. Germany (their Courier 93). 503 pp. ISBN 3-924500-91-6. Softbound. DM 85.00 (about $45).

There isn't really zoogeography here, but there is a large amount of valuable basic information. Except for an account of the expeditions, with localities, the volume is a series of taxonomic treatments of some of the arthropods. There are many new taxa and some of the papers give comparative treatments. Data for zoogeographers are here but need to be extracted and interpreted, of course in conjunction with previous work. -LMV

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Contributions Toward a Revision of the New World Nomadine Bees. A Partitioning of the Genus Nomada (Hymenoptera: Anthophoridae).
Roy R. Snelling. 1986. Natural History Museum of Los Angeles County (their Contributions in Science 376). 32 pp. No ISBN. Softbound. $5.00.

Three major groups, perhaps clades although not so stated or justified, of these kleptoparasites are given generic rank and discussed. One of the previously recognized subgenera is sunk because it was based on only one character, not given here; Snelling says he does this uniformly, although one may also want to know about the likely adaptive shift involved. A revision of major bee classification by Warncke is rejected without analysis. -LMV

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Two New Species of Ophiolepis (Echinodermata: Ophiuroidea) from the Caribbean Sea and Gulf of Mexico: With Notes on Ecology, Reproduction, and Morphology.
Gordon Hendler and **Richard L. Turner**. 1987. Natural History Museum of Los Angeles County (their Contributions in Science 395). 14 pp. No ISBN. Softbound. $4.00.

A title shouldn't be an abstract. Otherwise a competent account; more than a third of the specimens had regenerated arms. -LMV

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Reconstruction of Cranial Morphology and Analysis of Function in the Pleistocene Ground Sloth Nothrotheriops shastense (Mammalia, Megatheriidae).
Virginia L. Naples. 1987. Natural History Museum of Los Angeles County (their Contributions in Science 389). 21pp. No ISBN. Softbound. $5.00.

A nice study based on muscle scars, teeth, and the modern sloths. Naples comments on other topics also, including the derivation of the two tree-sloth genera from different families of ground sloths. -LMV

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Contributions Toward a Revision of the New World Nomadine Bees. 2. The Genus Melanomada (Hymenoptera: Anthophoridae).
Roy R. Snelling and **Jerome G. Rozen, Jr.** 1987. Natural History Museum of Los Angeles County (their Contributions in Science 384). 12 pp. No ISBN. Softbound. $5.00.

Two of several new species of these kleptoparasites described here are intermediate between two genera, which are here united. -LMV

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Propodial Elaboration in Southern African and Indian Ocean Fissurellidae (Mollusca: Prosobranchia) with Descriptions of Two New Genera and One New Species.
James H. McLean and **R.N. Kilburn**. 1986. Natural History Museum of Los Angeles County (their Contributions in Science 379). 12 pp. No ISBN. Softbound. $3.00.

The front of the body of these limpets has various unusual projections which provide the basis for the two new genera. The title shouldn't be an abstract. -LMV

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A New Species of Barisia (Sauria, Anguidae) from Oaxaca, Mexico.
John P. Karges and **John W. Wright**. 1987. Natural History Museum of Los Angeles County (their Contributions in Science 381). 11 pp. No ISBN. Softbound. $5.00.

New species of North American lizards aren't common, and this one was found independently by the two authors in a cloud forest. Description, comparisons, habitat, reproduction. -LMV

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The Type Specimens of Tapiravus validus and ?Tapiravus rarus (Mammalia, Perissodactyla), with a Review of the Genus, and a New Report of Miotapirus (Miotapirus marslandensis Schoch and Prins, new species) from Nebraska.
Robert Milton Schoch. 1984. Peabody Museum, Yale Univ. (their Postilla 195). 12 pp. Unbound. $1.50.

The function of a title is to attract interested readers and repel others, not to serve as an abstract. The paper itself is competent. -LMV

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Literature of the Receptaculitid Algae: 1805-1980.
Matthew H. Nitecki, Kristine L. Bradorf, and **Doris V. Nitecki**. 1987. Field Museum of Natural History (their Fieldiana: Geology, n. ser., 16). iii + 215 pp. No ISBN. Softbound. $24.00.

Receptaculitids are a group of Paleozoic algae of disputed affinities. This bibliography gives a short, deliberately uncritical abstract for each paper included; unpublished theses are unfortunately excluded and there is, remarkably, no index. The Estonian Õpik is rendered as Oepik; I don't know whether the German convention applies there too. -LMV

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Middle Miocene Marine Birds from the Foothills of the Santa Ana Mountains, Orange County, California.
Hildegarde Howard and **Lawrence G. Barnes**. 1987. Natural History Museum of Los Angeles County (their Contributions in Science 383). 9 pp. No ISBN. Softbound. $4.00.

Limb bones of seabirds from a dam site, some run over by construction machinery. Flightless auks may not have gotten much farther north in the Miocene. -LMV

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Three New Luminescent Ostracodes of the Genus Vargula (Myodocopida, Cyprinidae) From the San Blas Region of Panama,
Anne C. Cohen and **James G. Morin**. 1986. Natural History Museum of Los Angeles County (their Contributions in Science 373). 23 pp. No ISBN. Softbound. $4.50.

These are the first described of a number of similar new species which partition their habitat spatially and which differ in their pattern of luminescence. Two of the species here are sib species. All have detailed descriptions and even scanning micrographs. -LMV

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The Taxonomy and Nomenclature of some Australian Paragiine Wasps (Hymenoptera: Masaridae).
Roy R. Snelling. 1986. Natural History Museum of Los Angeles County (their Contributions in Science 378). 19 pp. No ISBN. Softbound. $4.00.

A revision of several taxa, some new, with extensive descriptions. The author unfortunately wants to follow Carpenter's cladistic sinking of the family into the Vespidae, from which it has adequate adaptive and phenotypic difference for separation. -LMV

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Late Miocene and Holocene Mammals, Exclusive of the Notoungulata, of the Río Acre Region, Western Amazonia.
Carl D. Frailey. 1986. Los Angeles County Museum of Natural History (their Contributions in Science 174). 46 pp. No ISBN. Softbound. $5.50.

This is the first reasonably good pre-Pleistocene mammalian fauna from the Amazon Basin. There are several new genera, not all formally described, and two rodent species known elsewhere. The one postglacial specimen, a ground-sloth skeleton, somewhat suggests a drier climate. -LMV

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Preliminary Description of a New Late Paleocene Land-Mammal Fauna from South Carolina, U.S.A.
Robert Milton Schoch. 1985. Peabody Museum, Yale Univ. (their Postilla 196). 13 pp. Unbound. $1.75.

This first Paleocene mammal site from eastern North America has two complete teeth, from a taeniodont and a new genus placed in a new family but which is probably referable to the Pseudictopidae of the otherwise Asian Anagalida. -LMV

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Catalogue of Type Specimens of Recent Fishes in Field Museum of Natural History.
Myriam Ibarra and **Donald J. Stewart**. 1987. Field Museum Nat. Hist. (their Fieldiana: Zoology, new ser. 35). iii + 112 pp. No ISBN. Softbound. $14.00.

The title adequately describes this work, which is an annotated alphabetic list with indices. -LMV

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A Dyrosaurid Crocodile (Crocodylia: Mesosuchia) from the Paleocene of Pakistan.
Glenn W. Storrs. Peabody Museum, Yale Univ. (their Postilla 197). 16 pp. Unbound. $1.75.

A partial vertebral column and some limb fragments of a marine crocodile, not identifiable below family. -LMV

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Moles of the Scapanus latimanus Group (Talpidae, Insectivora) From the Pliocene and Pleistocene of California.
J. Howard Hutchison. 1987. Natural History Museum of Los Angeles County (their Contributions in Science 386). 15 pp. No ISBN. Softbound. $4.00.

A little gem by the Master of the Moles, including two new species (one unnamed) and discussion of hypsodonty and other matters. -LMV

Rodents, Bats, and Insectivores from the Plio-Pleistocene Sediments to the East of Lake Turkana, Kenya.
Craig C. Black and **Leonard Krishtalka**. 1986. Natural History Museum of Los Angeles County (their Contributions in Science 372). 15 pp. No ISBN. Softbound. $3.50.

This material is from the Koobi Fora Formation, better known for its hominids. The scrappy material has good wash drawings and comparative descriptions. -LMV

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The Subfamilies of Eurytomidae and Systematics of the Subfamily Heimbrinae (Hymenoptera: Chalcidoidea).
Gerald R. Stage and **Roy R. Snelling**. 1986. Natural History Museum of Los Angeles County (their Contributions in Science 375). 17 pp. No ISBN. Softbound. $4.50.

The authors sink most of the existing subfamilies; the revision of the Heimbrinae is a detailed one. -LMV

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Les Kavas de Vanuatu. Cultivars de Piper methysticum Forst.
Vincent Lebot and **Pierre Cabalion**. 1986. Éditions de l'ORSTOM, 70 route d'Aulnay, 93140 Bondy, France (their Travaux et Documents 205). 0 + 234 pp. + 1 microfiche. ISBN 2-7099-0828-X. Softbound. 80FF (about $12.50).

Despite the subtitle, there is almost no botany here. The book is concerned with the ethnology, agriculture, and pharmacology of this source of an intoxicating beverage. -LMV

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A Phylogenetic Study of the Horned Lizards, Genus Phrynosoma, Based on Skeletal and External Morphology.
Richard R. Montanucci. 1987. Natural History Museum of Los Angeles County (their Contributions in Science 390). 36 pp. No ISBN. Softbound. $9.00.

A careful character analysis here is followed by a computterized cladogram, but one based on a partly critical analysis of the original output. Whether other topologies (except for one tip) are plausible isn't discussed, nor is the topology re-evaluated in light of the a posteriori unequal values of the characters as found by inferred homoplasy (even if, as here, all characters are initially given equal weight). Such analysis is usually omitted, but that doesn't mean it should be. Somebody will have to do it some time. Anyway, the resulting cladogram is then treated to a long and speculative historical treatment onto which the few fossils are then glued. It may all be right, but it would be more convincing with allowance for what one can think of as the sampling error of the set of characters used. -LMV