VIEWS AND REVIEWS

Contemporaneity and Precursors

Factors of Evolution: The Theory of Stabilizing Selection.
I.I. Schmalhausen. 1987 reprint (the book itself says 1986) of 1949 translation of 1947 book in Russian. Univ. Chicago Press. xxii + 327 pp. ISBN 0-226-73874-4. Softbound. $14.95.

Much of this remarkable book is valuable for current work. It should have been one of the seminal works of the Synthesis; instead it was largely ignored in the West. I don't know why. The translation eliminated some matters of interest as well as redundancies, including part of Schmalhausen's proposal of the r-K contrast in natural selection (in 1947!). Stabilizing selection is in the subtitle and indeed is probably the unifying theme, but there is a lot more. Norm of reaction, constraints, hierarchical evolution, phenotypic variation, evolutionary rates, and other matters which are or should be emphasized today have still-useful treatments, although they are inevitably mixed with issues of the 1940s. A posthumously published second edition has not been translated. Schmalhausen was one of the victims of Lysenko, although he managed to remain active in evolutionary biology, and a perceptive introduction by David Wake discusses this and the book itself.

Historians of science emphasize influence rather than creativity. While these are presumably correlated, the correlation is obviously far from perfect. Schmalhausen's greatness as a scientist is not diminished by the narrow-mindedness of others. Science is public discovery, not public relations. When we celebrate our intellectual antecedents we should not forget those who were wrong for good reasons (this is why a narrow whiggery is bad), but we should build a great wall between science and its sociology. -LMV

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The exception probes the rule

Beyond the Gene: Cytoplasmic Inheritance and the Struggle for Authority in Genetics.
Jan Sapp. 1987. Oxford Univ. Press. xvi + 266 pp. ISBN 0-19-504206-9. Hardbound. $35.00.

Cytoplasmic genetics has had an odd history. That it is based on real phenomena is now taken for granted, although aspects such as dauermodifications (and other phenomena not associated with specific organelles or symbionts) still have a residual taint, apparently because theory has ignored them. Initially advocated by embryologists, to whom differentiation was most plausibly explained by cytoplasmic inheritance at the cell level and who overgeneralized from this, the participation of the cytoplasm in heredity was awkward to establish by then-standard methods. There were various false starts, which did not help the reception of what has proved to be genuine evidence.

More importantly, cytoplasmic genetics didn't fit comfortably into the predominant way of looking at heredity. Sapp's emphasis is on how this situation affected progress in the subject; in fact the central theme is intellectual politics. He handles the matter rather dispassionately and, so far as I can tell, accurately. The book even covers (if less authoritatively) the current situation.

How much else are we missing, and not merely in genetics, which is swept under our intellectual rugs because it blurs our pretty pictures or is merely confusing? New concepts are necessarily uncomfortable. As Bridges said, "Treasure your exceptions." -LMV

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Evolutionary Theory 8: 109-123 (September, 1987)

On the Structure of Biological Structure

Evolution, Thermodynamics, and Information: Extending the Darwinian Paradigm.
Jeffrey S. Wicken. 1987. Oxford Univ. Press. x + 243 pp. ISBN 0-19-504318-9.
Hardbound. $32.50.

This book isn't as bad as the recent one by Brooks and Wiley on a broadly overlapping subject. In fact parts of it are even worth reading, and it is better written than an average scientific paper.

Wicken is at his best in the first half of the book, where he begins by discussing relevant thermodynamics, generalized information theory, and the nature of life in a sometimes illuminating way. He sees life, for instance, as an autocatalytic, homeostatic system controlled by a much smaller body of information. One may dissect this view and find it wanting, but it is an interesting approach.

He then criticizes the purely informational approach to the origin of life, particularly as embodied in the hypercycle theory. There are serious problems both in how to get there and in how to leave it to become an organism. Primordial microspheres seem to have fewer difficulties here and can be regarded as a byproduct of entropic dissipation producing an autocatalytic structure. He may well be right; microspheres are already semi-isolated systems, as he notes, and therefore are subject to selection and further integration.

The book then gets mushier. Wicken recognizes, as most biologists still fail to do, the generality of natural selection for expansion of energy flow. But he treats ecosystems and communities as obviously adapted entities, with their own strategies. David Wilson's patch selection can help here, but that isn't Wicken's approach. He would probably adopt the Gaia hypothesis of the world as an adapted superorganism if he knew of it; his viewpoint is similar. Gaia, like Wicken's ecosystems, is supposed to evolve adaptation without there being selection among differently adapted units of comparable scope. This is magic, not natural causation. Maybe magic is what we don't understand, but since William of Ockham we should know better than to invoke it in explanation.

A discussion of progress in evolution is also mostly metaphorical rather than causal, although an implicit invocation of selection at different time scales does help.

Information, organization, and energy are central to evolution, and some of their consequences have become known. But the topics are conceptually difficult; it is understandable that progress is slow. - LMV

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Paleontology and Progress

Life Pulse: Episodes from the Story of the Fossil Record
Niles Eldredge. 1987. New York: Facts on File. x + 246 pp. ISBN 0-8160-1151-6.
Hardbound. $19.95.

Eldredge hasn't yet become the Asimov of paleontology, but he's on the way. This book is a popularized version of what looks intellectually interesting in an animal-oriented approach to the fossil record. It's one of the better treatments, and the only one now reasonably up to date. Most of it is also rather superficial, perhaps even more so than is necessary. However, I don't have any major criticisms, unlike with his previous books. Not surprisingly his treatment of vertebrates is less sure than that for invertebrates, and plants and protists get hardly a murmur. His biases are less obtrusive than in his other books. But I wish the American Museum would stop furnishing as accurate, to unsuspecting purveyers, their eye-catching photograph of a too-imaginatively enlarged reconstruction of the giant shark <u>Carcharodon</u>.

The title comes from the evolutionary radiations which occur after mass extinctions. (Eldredge ignores the evidence that some of these radiations started a little before the relevant extinction.) This is a quantitatively rather minor part of the book, though, but it is worth emphasizing. I suppose even most evolutionary biologists still haven't got it into their mental bones that natural selection operates in a major way on very different time scales. Eldredge facilely concludes that this pattern precludes progress in evolution. So it does, more or less, on the most visible level. But this isn't the only level, as Darwin realized. Perhaps Darwin's subtler view of evolutionary progress is wrong, but the existence of selective mass extinctions is easily reconciled with it. -LMV

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Sexy Fossils

Fossils, Teeth, and Sex: New perspectives on Human Evolution.
Charles E. Oxnard. 1987. Univ. Washington Press. xiv + 296 pp. ISBN 0-295-96389-1. Hardbound. $35.00.

To translate the title, the book is about sexual dimorphisms and sex ratios inferred from fossil teeth, not whatever spicier subject you may have had in mind. Oxnard writes with full anthropological prolixity, but he does have something to say.

In one major point I think he is right, and this goes against most recent conclusions. By far the largest sample of late Miocene apes comes from a site in China. Oxnard has collaborated with Wu and others in the study of this sample, and his analysis shows that it was derived from two separable populations each of which was itself dimorphic. He refers the populations, probably correctly, to separate genera, _Sivapithecus_ and _Ramapithecus_. The morphology itself is not treated here but in my opinion the available evidence supports this conclusion, if somewhat ambiguously. _Ramapithecus_, but not _Sivapithecus_, is human-like in its sex ratio as well as some morphology. We may well have here a human ancestor (at least at the generic level) after the divergence of the African apes.

A third abundantly represented Chinese ape, the later _Gigantopithecus_, is unique among primates in combining a uniform sex ratio with the largest dimorphism known. That it is also the largest known primate may be relevant.

In his other major heterodoxy Oxnard is less successful. He has shown morphometrically, as have others in other ways, that the australopithecines differ from both modern humans and modern apes. Here he seems to adopt unconsciously a _scala naturae_ and assume that modern apes, especially the African ones, represent the ancestral condition instead of themselves evolving for their own benefit after our lineage diverged. His preferred scenario, that no australopithecine was ancestral to _Homo_, does not follow from his own evidence, as he does admit. Even his conclusion of apelike sexual dimorphism and sex ratios here is not statistically convincing, quite apart from possible problems in the allocation of specimens to species and pooling of samples from different sites. -LMV

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Phylogenies and Phossils

Phylogeny Reconstruction in Paleontology.
Robert M. Schoch. 1986. Van Nostrand Reinhold. x + 353 pp. ISBN 0-442-27967-1. Hardbound. $52.95.

The estimation of phylogenies is one of the most important aspects of the evolutionary half of biology. People whose own work would benefit from it often disparage it, although not so much now as around the middle of the century. Ignorance generates disdain, and unfortunately many people who should learn thereby prevent themselves from doing so.

Methods of inferring phylogenies have become the subject of explicit study, and the field has benefitted greatly from this. Some new methods have emerged in addition to the codification and improvement of older ones. Oddly, some workers think that fossils are irrelevant to the subject. Obviously Schoch disagrees with them, although he doesn't emphasize the special contributions that fossil evidence can make.

Schoch is a cladist, and in making the ordinary cladistic confusion of classifications with phylogenies he establishes a mind-set which results in misrepresentation of other views in a variety of ways. He is nevertheless as undogmatic a cladist as I have seen; he even recognizes the difficulties cladists have in classifying organisms which are now extinct.

Within the cladistic framework Schoch does a good job. He tells how, critically and in some detail, to infer cladograms using fossil evidence especially. Unfortunately there are no real examples worked through, but even a novice shouldn't be too bothered because the procedures discussed are simple.

The title of the book is a bit misleading, though. A phylogeny is a cladogram with ancestors, and Schoch is an orthodox enough cladist to deny knowledge of ancestors in most cases. (Cladistics seems self-contradictory here. If an organism is indistinguishable from an ancestor, surely the most "parsimonious" treatment of it is to regard it, provisionally, of course, as the ancestor. This saves a branch and is as testable with further knowledge as any other aspect, while denial of its ancestral status without any positive evidence is quite untestable.)

The book doesn't much discuss automated methods for phylogenetic inference. Their widespread use may postdate his writing, but a perusal of the book should at least make the reader suspicious of using them uncritically. Schoch's brief explicit discussion of this approach is worth reading by the cognoscenti. Cladogram estimation from molecular evidence is, reasonably in his context, almost ignored even though it is often relevant to the position of fossils, and so comparative evaluations must be made when results conflict.

A more serious lack, though, is any consideration of the degree of inaccuracy of the final cladogram. Schoch does realize that sometimes a unique tree can't be found. However, even in cases where one cladogram is best given the data now available it still may be wrong, even very wrong. We want to know what sorts of other trees are in some sense also compatible with the data. Treating a cladogram merely as a hypothesis to be tested, the orthodox cladistic approach, rather than as a problem in estimation, lets this sort of question remain even unasked. When it is asked, the usual answer is to count the number of shared derived characters which other possibilities would add. This is better than nothing, although perhaps not by much. It ignores problems such as delimitation of characters, functional and developmental correlations, ease of reversal, and all the rest that make reliance on unweighted characters a slippery road. Just what to do here is an unsolved problem in detail, but it is better to recognize a messy situation than to ignore it and fall in.

-LMV

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Interactions and You

Biophilosophy: Analytic and Holistic Perspectives.
Rolf Sattler. 1986. New York: Springer-Verlag. xvi + 284 pp. ISBN 0-387-16418-9. Softbound. $36.50.

It is an interesting phenomenon that the manifold interrelations and intergradations which characterize organismic form seem to have motivated various morphologists, especially plant morphologists, to a more or less mystical outlook. At least there is such a correlation, and one of Sattler's views is that we can't really know causes, just correlations. But complexity is of course easily compatible with normal scientific outlooks, and the similar characteristics of much of geology have not obviously led to such results. (Whether the advantage-to-ecosystem principle of much of systems ecology is quasi-mystical, wrongheaded, or merely not explained within systems ecology, is a fundamental question which deserves more attention than it has received.)

Sattler presents usually brief discussions of a wide variety of philosophical topics relevant to biology and, interestingly, vice versa. Most of the topics are standard ones which are more authoritatively discussed in the several books now available written by philosophers. Sattler's discussion of teleology and function is particularly idiosyncratic. (Because I use this word I obviously think he is wrong, but here and elsewhere he defends original viewpoints which may appeal to others.) He has a remarkably narrow view of the synthetic theory of evolution, thinking that it requires linear rather than interacting causes. And he really is a mystic, believing that one mind is immanent in the universe and denying that we can get objective knowledge of the real world. This outlook surfaces frequently but doesn't seem to affect most of his discussion. For people who can't adequately separate their views on the nature of the world (ontology) from their values it is even an outlook I would recommend.

Two specific criticisms: Sattler thinks that free will is, in principle, immune from scientific study because it is epistemically prior to our inferences about the external world. But even if the latter were true it is possible (and I imagine will happen in the dueness of the course) that computers with free will can be constructed and understood. He also thinks that homology is quantitative, not qualitative, and that degree of homology is degree of resemblance. Shades of redneck biochemists! This is in Sattler's area of expertise, though, and is derived from such problems as a structure which partly resembles a leaf and partly resembles a stem. From the viewpoint of continuity of information the qualitative nature of homology can nevertheless be maintained: some aspects of the structure are homologous to those of a leaf and some (perhaps an overlapping set) to those of a stem. At least this is true if that is how the developmental program operates, and the question is then an empirical one. It doesn't reach the nature of homology itself.

-LMV

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Evolution: Selected Papers.
Sewall Wright. Edited by William B. Provine. 1986. Univ. Chicago Press. xiii + 649 pp. ISBN 0-226-91053-9 hardbound, $70.00. ISBN 0-226-91054-7 softbound, $25.00.

Wow! They're all here. Well, not really. What we have is what Provine thinks are all of Wright's papers on evolution through 1951, plus three later ones, minus Wright's joint papers with Dobzhansky (which have recently appeared in a collection of Dobzhansky's papers). Provine gives brief and perceptive introductions to each paper or short group of papers. There is also a bibliography of Wright's publications, from 1912 through 1984.

My view of evolution seems to be broader than Provine's. Although a book like this does have limits on size, its coverage presents a biased view of Wright's evolutionary interests. Several classic papers are omitted, as is everything else outside a narrow view of population genetics. Size factors, path coefficients, guinea-pig color-gene interactions and digit number, and other topics are completely omitted, and there is only one paper on inbreeding per se. Even a paper on evolutionary cytogenetics is missing. Did you know that Wright discussed developmental noise in 1920 (in an omitted paper)? But be grateful for what is here.

-LMV

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Theories of Human Evolution: A Century of Debate, 1844-1944.
Peter J. Bowler. 1986. Johns Hopkins Univ. Press. xiii + 318 pp. ISBN 0-8018-3258-6. Hardbound, alkaline paper. $32.50.

Bowler has given us a thoughtful, scholarly, and well-written treatment of his topic. He considers both phylogeny and causation, at about equal length. The social environment of scientists is sometimes relevant to the theories they create, and Bowler gives a diversity of plausible if inconclusive examples here, this of course has no bearing on the approach of these theories to the true patterns or causes. The independent discovery of the importance of natural selection for evolution by at least three Englishmen was presumably of this nature, although it is outside Bowler's subject, and relativistic social historians (and even a very few biologists!) have used this pattern to argue that competitive natural selection is unreal. But one just needs to note the distinction of discovery from justification, despite the contingent occurrence of overlap in some cases. Bowler notes the problem and avoids it, but he does discuss relevant aspects of the social environment.

The book deals with a major subject rather than the trivia that some historians feel compelled to expound at interminable length, and it has no real competitors. (Yes, we all do necessary spadework in our own fields, and such studies provide data for syntheses like this book.) Bowler is able to avoid the flames of whiggery by treating the various theories as based on what was then known, but he also avoids the ice of priggery both by evaluating the theories on the basis of that evidence (although he could have done more of this) and by presenting current views. His own knowledge of the biology is fully adequate for his purpose, an unusual comment to be able to make about a historian.

To quibble on a detail: Bowler explicitly considers the perennial problem of what surname to use for people usually called Elliot Smith, Le Gros Clark, and others. What he does is to investigate what other people called them. But it is also possible to see what they called themselves when citing earlier works in later ones. In law as well as in practice, one's name is what one consistently calls oneself, and this can even change during one's life. -LMV

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Alfred Wegener: The Father of Continental Drift.
Martin Schwarzbach. 1986. Translated from German of 1980. Madison, Wisconsin: Science Tech. xx + 241 pp. ISBN 0-910239-03-7. Hardbound. $35.00.

Wegener wasn't the first to publish the hypothesis of continental drift (although he said he discovered it independently), and of course he didn't find its mechanism. Even part of his evidence was wrong. What he did do was to pursue the implications of the hypothesis and thereby make it more widely known. It can reasonably be said that a developed theory of drift belongs to Wegener, but that isn't the only part of creative science.

Wegener was a meteorologist, not a geologist, and this led to <u>ad hominem</u> arguments against the theory. He wrote books on atmospheric thermodynamics, lunar craters (with his own experimental evidence, apparently unknown to later workers), and paleoclimatology in addition to his well-known book on drift. He was a major scientific explorer of the Greenland ice cap, where he died on a rescue mission; a large peninsula in east Greenland is called Wegeners Halvø.

The book discusses Wegener's work more than his life, although that is summarized. It is done well if sometimes skimping analysis for description. A relatively extensive history of plate tectonics by I. Bernard Cohen is included, as is a chapter of memories of Wegener by Johannes Georgi, a meteorologist (the disceverer of the jet stream) who was in the tiny camp in the center of Greenland which Wegener died trying to rescue. There is also a bibliography of Wegener's writings. -LMV

Biological Metaphor and Cladistic Classification: An Interdisciplinary Perspective. Henry M. Hoenigswald and Linda F. Wiener (editors). 1987. Univ. Pennsylvania Press. xiii + 286 pp. ISBN 0-8122-8014-8. Hardbound. $25.00.

Metadisciplinary analysis, the comparative study of different intellectual disciplines, is a prospectively exciting but still poorly developed field. After some historical material, the meat of this book deals with the construction of trees of descent in biology, linguistics, and stemmatics. The last subject deals with what manuscript was copied from what, before printing.

All the biologists are cladists, which produced some confusion in the other camps. (The book comes from two symposia, where interaction was possible.) Cladists don't distinguish phylogenies from classifications, and this has led here to methods applicable to one of these topics being used interchangeably for both and even to criticism of others who make the distinction, just because distinct topics require at least partly distinct methods. The problem was easily avoidable and, although not all authors are affected, the bias resulting from its presence diminishes the book.

Several of the papers make explicit comparisons between the fields. While I can't summarize those here, it is interesting that the basic methodology is similar although several sorts of differences exist in the finer structure. -LMV

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System und Phylogenie der Lebewesen. Band 1: Physikalische, chemische und biologische Evolution. Prokaryonta. Eukaryonta (bis Ctenophora). [Classification and Phylogeny of Organisms. Volume 1. Physical, Chemical, and Biological Evolution. Prokaryonta. Eukaryonta (through Ctenophora).] Edwin Möhn. 1984. Stuttgart: E. Schweizerbart'sche Verlagsbuchhandlung. xxxv + 884 pp. ISBN 3510651170. Hardbound. DM330 (about $181.)

A masterpiece. Imagine the Traite de Zoologie being redone, competently and up-to-date, by one person: that is almost what Möhn is giving us. His book isn't as detailed as the Traite, especially the later-written volumes, but for most purposes his book is where to look first. It complements the finished Synopsis and Classification of Living Organisms (S.P. Parker, ed., 1982).

Möhn's emphases are on phylogeny and characteristic morphology. This he does quite well, with a critical and original attitude. He tries to classify all organisms cladistically, but the impracticality of such an attempt does intrude. Thus the Animalia are a superkingdom rather than a subgenus or something similar. He also does not usually justify his phylogenies explicitly, and sometimes the justification is not clear at all. However, he has a long and valuable discussion of more hypotheses on the origin of the Animalia than I knew existed.

Some groups are minimized, notably the fungi, which have only 8 pages. Viruses are even deliberately excluded on the odd grounds that they do not form a monophyletic group. See Parker's treatise on these. Möhn gives only cursory attention to extinct organisms and ignores them in his phylogenies. Denial of an endosymbiotic nature for the eukaryotic cell, and deriving it solely from blue-green algae, was probably still just a viable hypothesis when he wrote, but his denial is partly (?mainly) based on his interpretation of cladistic dogma. Mostly he has more sense than to let such preconceptions influence his empirical conclusions as to what has actually occurred.

Prokaryotes have only 36 pages, algae and other protists have 277, Parazoa (including Placozoa) have 118, and Coelenterata (s.l.) have 208. The first half of the subtitle refers to only a small part of the book. The figures are numerous and well-chosen, and there is an excellent bibliography, segregated by taxon. -LMV

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Handbook of Paleozoology
Emil Kuhn-Schnyder and Hans Rieber. 1987 (copyright 1986). Translated from German of 1984. Johns Hopkins Univ. Press. xi + 394 pp. ISBN 0-8018-2837-6. Hardbound, alkaline paper. $32.50.

More than half the book covers vertebrates, and almost half that goes to mammals. Invertebrates and "protozoans" receive short rations; there is more on artiodactyls alone than on brachiopods, corals, or most other invertebrate groups. Half the book is given to figures, but with small pages the coverage is cursory. The information seems mostly as of about 1975 to 1980. The book is OK for its scope, but I don't see why it was translated. -LMV

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The Stages of Human Evolution: Human and Cultural Origins.
C. Loring Brace. Edition 3. 1987 (copyright 1988). Prentice-Hall. vi + 154 pp. ISBN 0-13-840166-7. Softbound. $14.00.

A nice little treatment. Emphasis is about equally on the fossils, adaptive evolution, and history of the subject. It is pretty much up to date, given its scope, except that Brace still emphasizes the "probable mutation effect." The quasi-degenerative phenomena which this theory was developed to explain are probably better accounted for by pleiotropic effects of selection on other features. -LMV

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Guide to Fossil Man.
Michael H. Day. 1986. Edition 4. Univ. Chicago Press. xvi + 432 pp. ISBN 0-226-13889-5. Hardbound. $37.50.

If you don't know this classic work, you should. It is the first place to look for basic information on fossil hominids. This edition is quite up to date and is larger than its predecessors. For each major site it gives one or more photographs of specimens, details on the site and its age, a morphological description (with measurements when available), comments on affinities and significance, information on location of the specimens and availability of casts, and a bibliography. There are also brief summaries of three sets of controversies, in an unusually unbiased manner. This book is a thoroughly professional treatment which is nevertheless useful even for people who know nothing whatever of the subject. -LMV

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Spinnenfauna Gestern und Heute. Band 1. Fossile Spinnen in Bernstein und ihre Heute Lebenden Verwandten. (The Spider Fauna of Yesterday and Today. Volume 1. Fossil Spiders in Amber and their Living Relatives Today.)
Jörg Wunderlich. 1986. Wiesbaden, W. Germany: Erich Bauer Verlag bei Quelle & Meier (received from Aula Verlag). 0 + 283 pp. ISBN 3-88988-104-1. Hardbound. DM 235 (about $129). DM 185 if all 3 volumes subscribed for.

Despite having a large number of pictures, some even in color, this is an original monograph. Each of the five chapters has an English summary. The treatise is based on the Dominican and Baltic ambers, respectively about 20 and 40 Ma old. No species from these faunas now survive, but about 85% and 25% respectively of the genera do. Most of the monograph is systematic, including a number of new taxa. A cladogram of the araneoids is given, including recent forms, some of which are figured in addition to the fossils. In another group two sets of subspecies are described without evidence for geographic or temporal separation, but Wunderlich sees the problem. A number of cases of phenomena such as parasitoidy, mimicry, copulation, and feeding are described from amber specimens, and the biogeographic affinities of the faunas are discussed. The faunas differ appreciably in several aspects of their overall composition. -LMV

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Lizards of Western Australia. I. Skinks.
G.M. Storr, L.A. Smith, and R.E. Johnstone. 1981. Univ. Western Australia Press (and Portland, Oregon: International Specialized Book Services). xii + 200 pp. ISBN 0-85564-195-9. Softbound. US$29.50.

The 130 known species of skinks in Western Australia constitute the largest fauna of this lizard family in any similar-sized region. The book is intended as an identification guide but has broader value in documenting the nature of this diversity. Each species has a page with a summary of its characters, the derivation of the name, and habitat and distribution, with a map of its own. Most species have colored photographs of living individuals on one of the twenty plates; drawings of the head show characters for many species. Mercifully the authors have not coined new "common" names for the 129 species which lack them; everybody is thereby saved from learning twice as many names as necessary. -LMV

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The Freshwater Fishes of Europe. Vol. 1, Part 1. Petromyzontiformes.
Juraj Holcik (editor). 1986. Wiesbaden, W. Germany: Aula-Verlag. 0 + 315 pp. ISBN 3-89104-040-7. Hardbound. DM 236 (about $129). DM 198 if all 9 volumes are subscribed for.

This is a rather remarkable work. There are only 10 European species of lampreys, including the Caspian Sea, and most of the book is devoted to individual accounts of their biology. The following categories are used for each species, although for some species there are no daa for some: Figure of oral disc, synonyms, holotype, etymology, description, morphology (of both adult and ammocete), karyotype, protein specificity, sexual dimorphism (with data tables), geographic variation and abnormalities, age and size variability, subspecies, hybrids, distribution (with map), introductions, habitat, migrations, hardiness, feeding habits, longevity, growth (with data), population dynamics (sometimes with data), age at maturity, gonads, egg size and description, spawning period, spawning sites, mating habits, spawning habits, early ontogeny and metamorphosis, parasites and other diseases, economic importance, and bibliography. The treatments are well done and provide the best source for comparative information on single lamprey species. There is also a short treatment of each genus represented, with a whole-body figure, synonymy, description, habitat, comparison with other genera, and keys to European species for adults and ammocetes. My only criticism is that the authors did not take this opportunity to designate holotypes, as required by the Code, for those species which lack them.

There is also a 65-page general introduction to recent lampreys by Hardisty, authoritative as usual. This discussion gives probably the best overall treatment of lampreys for the nonspecialist, although more detailed reviews exist. -LMV

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Shrubs of the Great Basin: A Natural History.
Hugh N. Mozingo. 1987. Univ. Nevada Press. xxi + 342 pp. + 24 plates. ISBN 0-87417-111-3 hardbound; $27.95. ISBN 0-87417-112-1 softbound; $16.95.

About 65 shrubs are covered, with several pages and a nice drawing for each in addition to the plates. The writing is clear and informative and discusses a diversity of topics as appropriate for each plant. -LMV

Tuberous, Cormous and Bulbous Plants. Biology of an Adaptive Strategy in Western Australia.
John Pate and Kingsley Dixon. 1982. Univ. Western Australia Press (and Portland, Oregon: International Specialized Book Services). xi + 268 pp. ISBN 0-85564-201-7. Hardbound. US$59.95.

These are plants which survive their annual droughts, often in poor soil, by means of underground storage organs. About four percent of the vascular plants in Western Australia do this, as do often a greater proportion elsewhere. Many unrelated plants have evolved this strategy, and they have very diverse above-ground forms. This appears to be the first book-length treatment of this adaptation on a comparative basis, and it is successful. The authors deal with the development, phenology, storage materials, and distribution of the plants in addition to providing a specific discussion for each. Much of the work is original. -LMV

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A Field Guide to Dryandra.
R.M. Sainsbury. 1985. Univ. Western Australia Press (and Portland, Oregon: International Specialized Book Services). xvi + 119 pp. ISBN 0-85564-234-3. Softbound. US$27.95.

The genus consists of small woody plants of the Proteaceae and is endemic to Western Australia, where it has undergone rather a radiation. Each species receives two or three attractive color photographs, a distribution map, and notes on characters, habitat, and cultivation. Every genus should have its Sainsbury. -LMV

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Aspects of the Conservation of Butterflies in Europe. Volume 8 of Butterflies of Europe (Otakar Kudrna, editor).
Otakar Kudrna. 1986. Wiesbaden, W. Germany: Aula-Verlag. 0 + 323 pp. ISBN 3-89104-039-3. Hardbound. DM 248 (about $136). DM 216 if all 8 volumes are subscribed for; this price possibly no longer available as this is the last volume.

"Butterflies, sick of the day's long rheum, to die of a worse than the weather foe." Thomas Hardy wrote this in The Dynasts, a much-less-than-epic poem (of sorts) about Napoleon's wars. Butterflies have other problems now. Because they are perhaps the most visible of insects, and are often attractive to our eyes, their conservation can be used as a means to preserve habitats which contain many less noticeable species. Kudrna approaches the matter with a skeptical eye, which makes his positive conclusions the more impressive. In addition to extensive discussions of the relevant aspects of conservation biology in general and as applied to European butterflies, and a comprehensive proposal for their future conservation, Kudrna gives an annotated checklist and a detailed summary survey of the status of each species by country. Eastern Europe is included. There is quite a bit of additional information of various kinds, even including color photographs of a number of relevant habitats. -LMV

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The Origin and Domestication of Cultivated Plants.
G. Barigozzi (editor). 1986. New York and Amsterdam: Elsevier. vi + 218 pp. ISBN 0-444-42703-1. Hardbound. $71.00 or Dfl160.00.

Some symposia don't deserve publication. About half the papers in this volume would be publishable as original research or reviews; the rest are potboilers.

There is nevertheless some useful material here, if one wants to thresh it out. Particularly noteworthy are papers on wheats, host-parasite coevolution, and a wide-ranging one on some less well studied crops which includes a critique of the "domestication syndrome". Several reviews on more specific crops and one on evidence of diffusion from southwest Asia are also worth having together. -LMV

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Origins of Sex: Three Million Years of Genetic Recombination.
Lynn Margulis & Dorion Sagan. 1986. Yale Univ. Press. xiii + 258 pp. ISBN 0-300-03340-0. Hardbound. $38.00.

One origin is of bacterial sex, the other of eukaryotic sex. They regard the first as an adaptation for DNA repair (rather than as, say, a byproduct of adaptive evolution of a plasmid) and the latter as a byproduct of diploidy. At last I think that's what their views are: the book is not clearly written.

Much of the discussion centers around the diversity of protist reproduction, although this itself isn't dealt with adequately. There is indeed a long section on the kinds of protists, and whether they are known to have meiosis, but meiosis (and ordinary mitosis) isn't a unitary phenomenon. There are quite a number of separable components, and they are indeed separated in different protists. A few years ago I tried to make some phylogenetic sense out of the distribution of components, as far as known, but there was too much incompatibility among the distributions of different characters and I gave it up as a bad job. Massive parallel evolution seems inescapable, in one or more directions. Margulis must be aware of this problem but it doesn't show.

The authors give an apparently original argument for the maintenance of eukaryotic sex: meiosis is needed for cell differentiation, because haploidy must occur sometimes to check on whether the semi-autonomous replication of microtubule organizing centers is still synchronized to nuclear replication. (Again, I think that's what they say.) They mention asexual eukaryotes but don't seem to regard them as much of a problem for their theory. Maybe I do misunderstand them, but it really is up to the authors to state their case clearly.

A group of pages fell out of the review copy on first reading. -LMV

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The Blind Watchmaker.
Richard Dawkins. 1986. Norton (in Canada: Penguin). xiii + 332 pp. ISBN 0-393-02216-1. Hardbound. $18.95.

Not a selfish gene in it. What Dawkins has put his formidable powers of semipopular writing to is no less than the undermining of the various objections to modern evolutionary theory made by people who don't understand it or, occasionally, who make them as publicity ploys.

He succeeds. Really. He does.

The title is of course from Paley's version of the argument from design: if even a watch is so intricately constructed that it must have had a purposeful maker, then so much more so ourselves. Dawkins shows in detail how this still-current misunderstanding arises and why its argument fails. He does the same for other supposed objections, mincing no words. I was surprised to find almost nothing to disagree with; I hope it isn't just for this reason that I see his critiques as uncompromisingly rational. In several places I thought he was falling into one trap or another, but he soon turned around and showed why it is a trap.

Luck or cunning? (by Darwin's opponent Mivart), chance and necessity (by the neoDarwinian advocate Monod): such false dichotomies, although not these as such, are collapsed to the point where they diverge from sustainable views.

There isn't another book in its class. -LMV

Ecology and Evolution of Darwin's Finches.
Peter R. Grant. 1986. Princeton Univ. Press. xiv + 458 pp. ISBN 0-691-08427-0 hardbound, $55.00. ISBN 0-691-08428-9 softbound, $22.50.

Is there anyone who would look at an evolutionary journal like this and not already know, at least in outline, what Grant and his collaborators have accomplished in the Galapagos? I suppose one can call it the most successful body of work ever done in evolutionary ecology. Here it is in one place, integrated into a coherent and compelling story.

Grant tells the story well and organizes it from the problems to the solutions, as they are now. Much of it has not appeared in the various papers describing the research, especially the large part of the book devoted to interpretation.

Most aspects of evolutionary ecology find their place in this work, and the chapters go readily from one part of this diversity to another. I can't summarize the book easily and I hope that I shouldn't need to, that anyone in the evolutionary half of biology who knows of the book's existence will look into it and thereby become enchanted. The book as well as the science is beautiful.

I do have one quibble, the reliance on the magnitude and sign of genetic covariances to infer evolutionary distance. There are several problems here, but I will just give a counterexample to illustrate one. Consider two characters with a close positive allometry, genetic as well as phenotypic. Two species may, and often enough do, have the same slope but with the coregression lines separated from each other along the second principal component. In the two-character analysis it is much more difficult to go from one line to the other than along one line. But the position of the lines itself is a character, a developmental one. If it varies the evolutionary shift may be easy. -LMV

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Helping and Communal Breeding in Birds: Ecology and Evolution.
Jerram L. Brown. 1987. Princeton Univ. Press. xi + 354 pp. ISBN 0-691- 08447-5 hardbound, $45.00. ISBN 0-691-08448-3 softbound, $16.00.

I hadn't realized how much there was to say about communal-breeding birds. This book is not a natural history. Instead, it is an excellent analysis of an evolutionary problem that has broader implications for other social systems including our own. The focal problem of the book is an evolutionary paradox-- why should individuals help other raise young that are not their own? For birds this is not an easy question to answer because the social systems in which helpers are found are remarkably diverse and complex. Using a variety of approaches from comparative natural history to sociobiological theory, Brown produces a thoroughly engaging and thoughtful book. It should be of interest to any evolutionary biologist as an example of how one should go about answering an interesting but difficult case of evolutionary adaptation.

Brown comes down strongly on the side of kin selection. However, although he may be right, the matter is controversial. Other partial or complete potential explanations for helping, such as learning, inheritance of territories, or facilitation of group adaptations aren't sympathetically dealt with for the most part. Brown emphasizes predictions of kin selection, but often these aren't unique to his favored theory. Kin selection works automatically and doesn't need to be tested as such, but nevertheless as an explanation for a particular adaptation it may be subsidiary to other causes.

The loose usage of "in" for "by" in the title perhaps isn't a real problem here, but sometimes the error causes ambiguity or hilarity. Rend it literally and see what it means. -VCM

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Primate Societies
B.B. Smuts, D.L. Cheney, R.M. Seyfarth, R.W. Wrangham, and T.T. Struhsaker (editors). 1987. Univ.of Chicago Press. xi + 578 pp. ISBN 0-226-76715-9 hardbound, $70.00. ISBN 0-226-76716-7 softbound, $27.50.

Undoubtedly destined as intended to become the standard text and reference work on primate social biology. It is probably the best synthesis of knowledge on primate sociality ever to be published. It outdoes its predecessor, Primate Behavior: Field Studies of Monkeys and Apes (edited by Irven DeVore and published in 1965) in scope, sheer amount of information, and synthesis. It remains to be seen whether it will be as stimulating to the development of the field as that earlier volume was. The articles emphasize what we don't know and what directions future research should take. The facts are there for anyone who disagrees with the authors' interpretations of them. The multi-authored nature of the book becomes lost on reading, an unusual phenomenon aided by combining cited literature together at the end. The style and quality of the articles are remarkably consistent; most variation can be attributed to topic rather than workmanship. This book is a pleasure to read as well as an invaluable source of information. - VCM

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Foraging Theory.
David W. Stephens and **John R. Krebs.** 1986. Princeton Univ. Press. xiv + 247 pp. ISBN 0-691-08441-6 hardbound, $40.00. ISBN 0-691-08442-4 softbound, $14.50.

The dust jacket is about as close as one gets to real animals in this foray into foraging theory. It is a rather nice natural history of optimization models, starting with a basic model and adding elaborations in subsequent chapters. Although the authors assume the reader to have reasonable knowledge of the math used, the mathemathically incompetent can still derive a great deal from the book. The authors make excellent use of boxes to highlight interesting topics or to isolate technical asides. The author maintains throughout a detachment about their modelling, frequently pausing to question the relevance of an approach. They do not venerate the approach as the mystic source of wisdom to which we turn to understand what animals are actually doing in the wild. The rather abstract level of the treatment of foraging behavior emphasizes the applicability of the models discussed to other problems in evolutionary ecology and behavior. I know of no better summary and synthesis of the topic. - VCM

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Current Perspectives in Primate Social Dynamics.
David M. Taub and **Frederick A. King** (editors). 1986. Van Nostrand Reinhold. xx + 531 pp. ISBN 0-442-28289-3. Hardbound. $54.50.

This is the first of two volumes collecting of some of the papers presented at the IXth Congress of the International Primatological Society which was held in 1982 at the Yerkes Regional Primate Research Center in Georgia. Some of the participants elected to publish their papers elsewhere; the others should have also. The volume is a miscellaneous collection of papers that are not united by any theme. They vary in scope and quality but most are of narrow interest that would have been better placed in specialty journals. - VCM

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The Evolution of Insect Life Cycles.
Fritz Taylor and Richard Karban (editors). 1986. New York: Springer-Verlag. x + 287 pp. ISBN 0-387-96349-9. Hardbound. $58.00.

Is adaptation the rule rather than the exception? Despite catcalls from the bushes, any naturalist knows that it is. Our not-so-mythical naturalist also knows that adaptive changes commonly occur within species as well as among them. For obvious reasons insects are especially useful in studying evolutionary as well as induced adaptations, and the evolution of fixed and plastic adaptations is the focus of this book. As one might expect from insects, the kinds of adaptations discussed, even in the realm of life cycles, are very diverse. The approach is implicitly or (usually) explicitly comparative, although what is compared is also diverse. Theory comes in from time to time but the adaptations themselves are the focus of most papers. A nice selection of good work. -LMV

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Light and Life Processes.
Jerome J. Wolken. 1986. Van Nostrand Reinhold. xii + 259 pp. ISBN 0-442-29348-8. Hardbound. $39.95.

This is rather a potpourri of a book, with particular emphasis on comparative aspects of vision. Photosynthesis, influence of light on behavior, and bioluminescence are also included, as are the author's views on liquid crystals in the origin of life. There is elementary material on the nature of light and the structure of eukaryotic cells, so one would think a general reader was in mind, but some of the material presented is almost research-grade and there is no attempt even to indicate the existence of other views. The central part of the book is indeed a useful summary of aspects of the evolution and physiology of vision, and some other physiological tidbits were new to me. But presentation of the material as a set of more or less separate essays would have given better focus and not fostered the impression that all of the subject was being covered. -LMV

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Immune Mechanisms in Invertebrate Vectors.
A.M. Lackie (ed.) 1986. Oxford Univ. Press. (Symposia of the Zoological Society of London, 56.) xv + 285 pp. ISBN 0-19-854004-3. Hardbound. $75.00.

Or, how do snails and arthropods (especially Diptera) cope with their parasites? They don't have antibodies, which are restricted to vertebrates. Instead they have a variety of mechanisms to detect and kill invaders. Some of these are broadly or even very narrowly specific. The treatment here is mostly at this level, with little in the way of evolutionary or even explicitly comparative information, although Anderson does have a chapter on the complexities of the interactive population genetics of resistance. As one can expect in this symposium series, the papers are authoritatative and selective in coverage. The responses of parasites to their host's defenses do, however, get some discussion. -LMV

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Vertebrate Reproduction.
Volker Blüm. 1986. Translated from German of 1985. New York and Berlin: Springer-Verlag. ix + 405 pp. ISBN 0-387-16314-X. Softbound. $36.50.

The book is meant as a text but is a useful general overview from the descriptive and physiological approaches. It is fully comparative; although mammals are emphasized more than other groups the latter receive extensive treatment.

Comparative syntheses are explicit rather than being left for the reader to make. Coverage extends from prereproductive behavior to early development and parental care; there are also chapters on anatomy and endocrine control. The translation is well done, like the book itself, and the writing comes out as normal biologese. I will use this book again. -LMV

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Symbiosis: An Introduction to Biological Associations.
Vernon Ahmadjian and **Surindar Paracer.** 1986. Univ. Press of New England. xii + 212 pp. ISBN 0-87451-371-5. Hardbound. $32.50.

This is an excellent and wide-ranging treatment of most aspects of symbiosis in the broad sense. Most of the book is organized taxonomically, but there are separate chapters on behavioral and evolutionary aspects not otherwise covered and on the origin of the eukaryotic cell. Even though the book is written as a text it will be useful for any nonspecialist: there are lots of interesting tidbits and, where I could judge, the treatment is mostly up to the minute. Lots of nice figures and even some historical sketches. Three areas are dealt with superficially, though: phylogeny (including relations between those of host and parasite), coevolution (D.S. Wilson and D. Janzen are not even mentioned), and community ecology. There is enough good natural history that one can treat these gaps as merely outside the scope of the book. -LMV

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Freshwater Ecology.
Alison Leadley Brown. 1987. London (and Portsmouth, New Hampshire): Heinemann Educational Books. vii + 163 pp. ISBN 0-435-60622-0. Softbound. 8.50 pounds (about $14).

This is an introduction to ecology which happens to be focused on fresh waters. Despite the author's definition of ecology as the study of distribution and abundance, the systems approach has as much emphasis as the control of distribution, and abundance (population ecology) is ignored. There is some treatment of physiological and applied ecology, and brief suggestions for field projects are given. A competent little book. -LMV

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Ecology of Protozoa: The Biology of Free-living Phagotrophic Protists.
Tom Fenchel. 1987. Madison, Wisconsin: Science Tech (Outside USA and Canada: Springer-Verlag). x + 197 pp. ISBN 0-910239-06-1. Hardbound. $39.00.

Even Karl Grell would be proud of this book. What he did for protozoan life history Fenchel has done for their ecology: a modern approach informed by an extensive knowledge of the diversity of the subject. The book doesn't cover population or systems ecology, but it does treat diverse aspects of physiological and community ecology. It emphasizes ideas over details but not over diversity; it focuses on the organisms and their problems. Not all problems, but a goodly share and some which are probably unfamiliar to (what does one call a student of Metazoa? Surely not a metazoologist!).

The population and community biology of microorganisms has been remarkably neglected, particularly considering their frequent potential for large-scale laboratory and integrated lab-field studies at reasonable cost and time. Not all of their advantages for molecular biology and such are relevant here, but enough are. Is it just that they aren't traditional? I suspect that if this bottleneck can be broken future conceptual advances will to a considerable extent come from the world of the small. -LMV

Erratum

Leigh M. Van Valen

My essay-review of Yablokov's book Phenetics (Evolutionary Theory 8: 61-64, 1986) contained some errors kindly noted by Alexander Gimelfarb. Chetverikov's exile was to three places in European Russia (first as a consultant to a zoo, eventually as a professor). In the USSR papers are initially submitted not to the local party official but to someone who checks for the inclusion of state secrets. And review of manuscripts submitted to journals is comparable to that in the West.