VIEWS AND REVIEWS[1]

Another culture

Catastrophes and Evolution: Astronomical Foundations.
Edited by **S.V.M. Clube.** 1990 (September; stated 1989 in book).
Cambridge University Press. xii + 239 pp. Apparently acid paper.
ISBN 0-521-37420-0. Hardbound. $44.50.

Evidence "that the cosmos may be playing a dominant rôle in the evolution of life on Earth...knocks at one of the most established precepts of modern science, namely the essential truth of Darwinian theory, which in turn presupposes a uniform terrestrial framework unaffected by its astronomical environment" (p. ix).

This sort of misinterpretation of modern evolutionary biology is surprisingly prevalent, even infecting some biologists. The question of the distribution of the scale of environmental stresses has no bearing whatever on the nature of evolutionary processes, although it does of course affect their relative importance and scale and thus can have major results.

The book results from a symposium of the Royal Astronomical Society, where the cultural biases differ from those of biology and geology. Such diversity is good, in letting a diversity of views be more easily available than when a single world-view decides what is reasonable and thus publishable. That the book is itself unbalanced would be a defect only if others were unbalanced in the same way. However, most are, which is understandable because catastrophes are exciting. It is also unfortunate, in not reflecting the professional literature in an overall evenhanded manner.

And the book? It is a collection of largely competent, semitechnical papers on, mostly, astronomical aspects of real and assumed earthly catastrophes. Biology comes in by means of passing references to an older book called, three times, *Origin of the Species*.

Catastrophes do occur. They occur at many scales, and there is no known generally applicable separation along this apparent continuum of scales. There are thresholds, but the strengths at which they occur differ for different phenomena. Impacts also occur, again at many scales. We would like to know the extent to which impacts really are catastrophic, the degree of coincidence of the set of impacts with the set of catastrophes, and roughly what proportion of perturbations, at different scales, are sudden enough to be called catastrophic.

The virtual absence of detectable biological effects associated with known large impact craters in the Phanerozoic record is surprising and is an underappreciated problem; the obvious solutions have serious difficulties. Such questions come from a culture different from that of the book. —L.M. Van Valen

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[1]Contribution 113, Lothlorien Laboratory of Evolutionary Biology.
Evolutionary Theory 10: 163-181 (December, 1992).

Danger! Canned phylogenies

Systematics of *Laccaria* (Agaricales) in the Continental United States and Canada, with Discussions on Extralimital Taxa and Descriptions of Extant Types.

Gregory M. Mueller. 1992 (30 June). Fieldiana: Botany (New Series), No. 30, Field Museum of Natural History, Chicago. viii + 158 pp. Acid-free paper. No ISBN. Softbound. $32.00.

This is a thorough revision, within the stated limits, of a major and well-defined genus of mushrooms. The genus is also a major former of ectomycorrhizae in both conifer and dicot forests. The revision is well done in the standard aspects, and has habitat information and lots of nice figures, including figures showing variation.

Mueller recognizes that interbreeding is important somehow, but he opts for a purely cladistic concept of species. This forces sib species, of which there seems to be at least one pair here, to be treated as conspecific. It also forces both polymorphic and polytypic species, like *Homo sapiens*, to be divided up morphologically; I couldn't tell if any of that was done here.

He tries valiantly to estimate phylogenies, both within the genus and for its putative relatives, but fails. Partly this isn't his fault, as many relevant data are still unavailable and there seems to be a fair degree of homoplasy. Partly, though, it is a result of feeding undigested characters into PAUP and believing what is excreted. Thus the highly correlated characters length and width of basidiospore echinulae are treated as independent; the same is true for color of lamella and pileus; and the color of natural basal mycelium and of cultured mats (the latter an innovative character) have completely identical distributions but again are treated as independent. I am unfamiliar with possible functional interrelations of the characters used and so I just mention this as a possibility. Pairs of interdependent characters can have each member weighted by a factor of $(2 - r^2)/2$, as I showed, with a multivariate generalization, in 1974 (Jour. Theor. Biol. 45: 235-247).

To this problem is added the completely unordered treatment of multistate characters. For some characters this may be appropriate, but no less than six of the nine multistate characters have objective orderings by size or shape. Ordering them doesn't require that one extreme be primitive, or that an intermediate stage may not be skipped, although there is, as there should be unless one state is an easily achieved loss, a penalty in parsimony for such skipping. Ordering does provide additional information, which is particularly useful when only fourteen characters are recognized even with the redundancies. And there are further problems. Thus Mueller provisionally recognizes two subdivisions of the genus, based on mycelial color. His trees separate members of one of these groups, those with violet mycelia, by two nodes. Using his own characters in the way he did, though, the violet group can be unified, with an actual reduction of one [completely redundant] step, by moving the outlier appropriately. One just needs to look at the trees and think, for this sort of resolution.

And the moral of this story is that canned programs don't give magical answers. Sometimes they can help, but one has to be actively critical throughout. Mueller isn't at all alone in the problems mentioned (and there are other problems with such programs which are irrelevant here, as for molecular data); we just need to understand the implications of what we do.

-L.M. Van Valen

Phylogeny and Classification of Birds: A Study in Molecular Evolution.
Charles G. Sibley and **Jon E. Ahlquist.** 1991 (13 March); stated 1990 in book. Yale Univ. Press. xxiii + 976 pp. Acid-free paper. ISBN 0-300-04085-7. Hardbound. $100.00.

Distribution and Taxonomy of Birds of the World.
Charles G. Sibley and **Burt L. Monroe, Jr.** 1991 (13 March); stated 1990 in book. Yale Univ. Press. xxiv + 1111 pp. + errata sheet. Acid-free paper. ISBN 0-300-04969-2. Hardbound. $125.00.

Ornithology will never be the same again. I don't mean this pejoratively; the books are an extraordinary accomplishment and provide a baseline for all future work in their subjects.

Before classification comes phylogeny. The first book gives results, and some data, for the most extensive program of DNA hybridization ever done. The climax is the "tapestry", several meters long if put together, of a phylogenetic hypothesis for effectively all extant birds. This was first published in 1988, and a few changes or alternative hypotheses are given now for several groups.

I use the word "hypothesis" deliberately. Although the word is part of cladistic orthodoxy, cladograms are inferences from data and are therefore more than Popperian guesses. Presumably that is also the case with the trees given here, but there is no way for a reader to tell. Data are subject to error and inferences are subject to error, each in several ways in this work. Yet the authors give the reader no way in which to evaluate these cumulative errors even approximately. Therefore the tree, although not at all worthless, *must not* be used by others as a validly constructed cladogram.

The work is one of advocacy, with respect to the results. The authors are data chauvinists. Morphological results are nice when they agree but are always rejected when they disagree. Sequence data, now beginning to appear and giving results in part different from those here, are dismissed in advance as coming from only a tiny part of the genome. Various criticisms have been made of previously published parts of the work, but they are for the most part ignored: substantive criticism is not answered and most published critiques are not even cited. (A moderate number had appeared by 1988, the year of some cited references.) The authors attack Houde for proposals, from fossil evidence, on the phylogeny of ratites, but the proposals are fully consistent with the authors' own results if not with their preferred interpretation. (Houde had had the temerity to criticize earlier work by the authors.) And so on.

The tapestry is derived by a method in the spirit of the venerable Unweighted Pair-Group Method, Arithmetic, or average linkages. This assumes, in its phylogenetic application, a strictly constant rate of evolution, and in fact has proven to be the method most sensitive to this assumption (e.g., DeBry 1992, Molec. Biol. Evol. 9: 537). The authors find some heterogeneity even by the insensitive relative-rate test and increase the length of an unstated number of branches accordingly, before using UPGMA.

Some ?later-derived branches seem to indicate an effect of generation time, as expected from strict neutralism, but again this is impossible to evaluate. (Could it be related to biases in the analysis? Anyway,the result, if true, is important and would establish the phenomenon better than isolated and partly conflicting results elsewhere, but someone will have to redo the analysis if it is to be shown.) The possibility of other variation in rate, a common phenomenon elsewhere, is simply ignored. Well, not quite: several groups have alternative analyses by the Fitch-Margoliash method, one of a number which don't assume equal rates, but these results are inexplicably not preferred.

Rooting a tree by UPGMA is easy, because the assumption of metronomic constancy just puts it at the point of greatest divergence. Without this assumption, the other trees actually lack roots although they are presented as having them.

Other criticisms will be, and have been, made by others. (One is incorrect: trees can be estimated from data in the form of distances, as is done here, unless the data are pathological.) Nevertheless, the results are often interesting. I doubt that the break-up of the Pelecaniformes will survive, but the placement of the Cathartidae with (here, even in) the Ciconiidae confirms valid earlier work which had not yet moved the inertia of tradition. Some of the many new proposals will undoubtedly survive, but no one can tell which they may be on the basis of this work.

After phylogeny comes classification. The authors use a stricter cladistic approach than is usual, because they think that they know the times of divergence of each clade. Thus categories are delimited by the inferred ages of branch-points, and we get, e.g., penguins, hawks, albatrosses, pelicans, and grebes allocated to the same order.

Sibley & Ahlquist include an extensive and valuable, if uneven, summary of earlier classifications, often with comments on the reasons for changes and with critiques. Sibley & Monroe's book is, almost entirely, a list of the extant species they accept, with moderately detailed information on geographic range and some information on habitat. There are often brief comments on taxonomic matters; some new proposals are included. There is a set of maps, especially valuable for the more obscure islands. (Here the old names of the Chinese provinces are used, and the two states in Baja California aren't delimited.) There is also a gazetteer. It escapes me why ornithologists want everyone involved with birds to learn two parallel and standardized names for each species, the ordinary latinized name and a pedantic "common" one in the local vernacular, often coined for the purpose and changed whenever the real name changes. "Baltimore oriole" really was a common name; if one wants to refer to a species as such, one can use its real name.

DNA hybridization isn't a magic method, but it does provide very useful information. Unfortunately, interpretation of the data used here will have to be redone before convincing results can be seen. I hope the unpublished data will survive in intelligible form so that this can be done. I had expected better, but the books are major contributions. The second book is the best available worldwide treatment of its subject despite the classification used.

—L.M. Van Valen

Pretentious poppycock

Life Itself: A Comprehensive Inquiry into the Nature, Origin, and Fabrication of Life.
Robert Rosen. 1991. Columbia Univ. Press. xix + 285 pp. Acid-free paper. ISBN 0-231-07564-2. Hardbound. $45.00.

The culmination of this book is "the answer" (p. 244): "*A material system is an organism if, and only if, it is closed to efficient causation*. That is, if *f* is any component of such a system, the question "why *f*?" has an answer within the system, which corresponds to the category of efficient cause of *f*" (italics in original). "This is indeed biology from a new and different perspective." Yes, indeedy. It is. Or at least it is a new and different perspective; I fail to see the biology. Even apart from ecology and such details as biparental reproduction, development and physiology themselves are affected by environmental conditions. The author is from Dalhousie, but even at Dalhousie there are people who know this. (Sorry – Dalhousie isn't really the trailing edge.)

Hogwash like this perpetuates stereotypes by the yahoos and gives theoretical biology a bad name. The book is so bad that it smudges the reputation of the publisher. -L.M. Van Valen

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The Terrestrial Invasion: An Ecophysiological Approach to the Origins of Land Animals.
Colin Little. 1990. Cambridge Univ. Press. ix + 304 pp. Apparently acid paper. ISBN 0-521-33447-0 hardbound, $69.50; –33669–4 softbound, $30.00 (minus 5 cents).

Don't confuse this book with one seven years earlier by the same author and publisher, called The Colonisation of Land, which has bigger pages and a higher price and looks at physiological adaptations of specific groups of land-invading animals.

The present book is self-contained and takes a different perspective. It looks at the evidence bearing on the habitats through which the various groups of land animals got ashore. The major contenders are mudflats, interstices between sand grains, coastal marshes and swamps, rocky and paved shores, and fresh water. Each habitat has advantages and difficulties, often different for different groups, and each may have been used by one group or another. For the land has been invaded many times, and some groups, like snails, have done it repeatedly.

Osmotic and other physiology, fossils, and other evidence are appropriately integrated. For instance, relevant marine groups tend to be tolerant of appreciable osmotic changes, while freshwater groups regulate their ionic balance. The latter are then more in need of a relatively impermeable covering on land. It isn't surprising to be told that, yes, amphibians likely came from freshwater fishes (although there are amphibious marine fishes today), but Little makes a reasonable argument for the ancestors of myriapods and insects coming through the interstitial route.

Phyletic evidence isn't used enough (e.g., it would be important in the discussion of isopods), and the relative lack of grazing of aquatic macrophytes is attributed to prudence by grazers. But I quibble. The book is original, on a major subject, and is well and clearly argued. I like it. -L.M. Van Valen

What's an edentate, mommy?

The Ear Region in Xenarthrans (= Edentata: Mammalia).
Fieldiana: Geology, New Series. Field Museum of Natural History, Chicago. Acid-free paper. No ISBN. Softbound.
Part I. **Cingulates.**
Bryan Patterson, Walter Segall, and **William D. Turnbull.** 1989 (30 November). No. 18. v + 46 pp. $13.00.
Part II. **Pilosa (Sloths, Anteaters), Palaeanodonts, and a Miscellany.**
Bryan Patterson, Walter Segall, William D. Turnbull, and **Timothy J. Gaudin.** 1992 (30 September). No. 24. v + 79 pp. $20.00.

The xenarthrans not only have [well, most of them do] an extra articulation on some vertebrae (whence their name) but have also been, as a group, awkward to articulate into mammalian phylogeny. A recent tendency has been to put them as the sister group of all other extant placentals. This has been based on several morphological traits regarded as more primitive than in other placentals, and on the diversification and apparent origin of xenarthrans in South America, where the early record of mammals has been poor.

Most of the evidence has already proven to be illusory, such as the supposed presence of a septomaxilla bone, and newly discovered early faunas in South America have lacked xenarthrans and plausible relatives, despite an early misallocation to this group. The work reviewed here removes the last pieces of evidence: an imperforate stapes (a dubiously primitive state anyway) is clearly derived within the group, and the foramen ovale is in its usual position for the Placentalia. Nevertheless, Czelusiak et al. (1990, Cur. Mammal. 2: 545-572) give some molecular evidence for an early divergence.

Palaeanodonts are a Paleogene group variously associated with the Xenarthra, Pholidota (pangolins), both, or neither. Patterson et al. remove one argument for special pholidotan affinity and provide some new evidence for special xenarthran affinity, including a newly identified structure. This doesn't affect the (weak) evidence for mutual affinity of all three groups, and in fact a little more evidence for such mutual affinity is provided. A derivation of this whole order Edentata from the pentacodontid insectivorans of the North American Paleocene remains a plausible possibility.

Within the Xenarthra there is also some clarification of phylogeny. The two genera of extant tree sloths are firmly placed well within different families of "ground" sloths, thus demolishing the family Bradypodidae traditionally used for them. One of these genera, *Choloepus*, appears to be part of a radiation in the Greater Antilles, and so a pregnant female may have floated back to the mainland on a fortuitous tree. (Or maybe a branch in the phylogeny is slightly off.) Is the supposed lack of evolutionary origins from islands mostly or entirely a result of fewer species being there and of lack of information? More evidence is given that anteaters are specially related to sloths, and there are other conclusions at lower levels.

The work done is rather less comprehensive than the title suggests. Most was done decades ago by the first two authors, later deceased, and the meticulous revision has been restricted to specimens, or at least genera, which they studied. This even applies, mostly, to comparisons with other work; critical genera described more recently from the Eocene of Europe and the Paleocene of China, of all places, aren't even mentioned. But it's a good job and creditable to all concerned.

-L.M. Van Valen

Dynamic Biogeography.
R. Hengeveld. 1990. Cambridge Univ. Press. xiv + 249 pp.
Apparently acid paper. ISBN 0-521-38058-8. Hardbound. $54.50.

For "dynamic" read "large-scale": (extant) species' ranges and above. Of course biogeography is also often dynamic (deals with processes and causes) at smaller scales and longer time intervals too, but that's not part of this book. And a quarter of the book is devoted to how best to distinguish and classify biogeographic regions. Species richness receives a chapter but the other component of diversity, equitability, isn't even mentioned. There is, though, consideration of the geography of a few biological traits, such as polyploidy.

The largest section of the book covers the ranges of species as such, and this is a good and moderately original treatment. So is a final chapter on geologically recent (from the latest glaciation) evolution of biogeographic regions. Climate is regarded as the major determinant of ranges. The author is critical of received views throughout, and despite his/her being an entomologist by trade the book has rather a botanical flavor, in topics as well as examples. The sea and the microbial world are ignored; each would give appreciably different perspectives and even conclusions, as would a longer time scale.

Geography is in decline, with departments being eliminated at major universities. (Our once-eminent department, at Chicago, was destroyed by a margin of one faculty vote in the Division of Social Sciences, where it resided administratively; neither biologists nor geologists were consulted.) Books like this show that the subject itself isn't dead. -L.M. Van Valen

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Animal Physiology: Adaptation and Environment. Edition 4.
Knut Schmidt-Nielsen. 1990. Cambridge Univ. Press. xii + 602 pp.
Apparently acid paper. ISBN 0-521-38196-7. Hardbound. $39.50.

Adaptationism is alive and well in physiology, and the subject would be dead without it. It's just that the adaptive significance of, say, respiration is obvious, or at least can be investigated readily, more so than for something like a zebra's stripes. One doesn't need to do esoteric things like find a significant effect on fitness in a natural population. (One doesn't ordinarily need to do that for nonphysiological traits either, but that's another story.)

If you don't know Schmidt-Nielsen's book, you should. Maybe this will be the last edition by the master, but it still is considerably modified from the third, seven years earlier. The emphasis is still on whole-animal and integrative aspects; thus the chapter on respiration deals with gas exchange rather than cellular phenomena, while the functioning of neurons has almost a chapter. He missed the advantage of hindgut fermentation (low-quality forage being OK), but I quibble. There is an emphasis on comparison among different groups, but this is done from a functional perspective rather than a phylogenetic one. The latter would be a different book, and both perspectives are valuable. -L.M. Van Valen

Zähne und Gebiss der Säugetiere [Teeth and Dentition of Mammals]. **Erich Thenius.** 1989. [Handbuch der Zoologie. Band 8 (Mammalia). Teilband 56.] Berlin and New York: Walter de Gruyter. [xi] + 513 pp. Apparently acid paper. ISBN 3-11-010993-X. Hardbound. DM 825 (about $503).

A remarkable book. So you don't read German? (Schäme Sie!) You lose the text and still have the figures. There are 881 of these, most with two or more parts. Most of the figures are original in this book. (I checked a couple that I thought might be unattributed copies; they weren't.) The figures are drawings or sometimes photographs, most of the new ones superbly done. Fossil and recent genera are included, with no apparent bias toward either. Entire skulls are frequently shown, and the book is actually about as good as the French *Traités* as a source for figures of whole skulls. For teeth it's better.

The text is as valuable as the figures, though. It is thoroughly comparative, but oddly with almost no attention to phylogenetic change. (Thenius did publish a large volume on mammalian phylogeny in 1969.) Almost every family is covered, with varying emphasis. The anterior dentition receives as much emphasis as the cheek teeth; there is even an appendix giving tooth formulas for hundreds of genera.

An introductory section treats general aspects such as terminology, the parts of the dentition, and occlusion. Most of the book, however, is on the specific groups of mammals. Here the morphology and to some extent occlusal relations of the permanent dentition are covered in some detail. Except for a short section in the introduction, there is almost no mention of the deciduous dentition, even in cases like the modernized bats where it is highly modified. The morphogenesis of teeth is entirely omitted, and there is also no treatment of microstructure. I mean these comments to be descriptive of the book rather than adverse criticism; the book is extremely valuable as it is and any book must have a finite scope. This isn't to say that I agree with everything that is said, and as in any general work there are some factual errors. This merely means that for serious work one should, as always, go to original sources; there is a large bibliography included. —L.M. Van Valen

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Bibliography of Herpetological Bibliographies. **Helga Brasseler.** Courier Forschungsinstitut Senckenberg, Senckenberganlage 25, 6000 Frankfurt/M., Germany. Apparently acid paper. Softbound.

[Part] **I.** 1989 (15 December). Vol. 116. 214 pp. ISBN 3–924500–53–3 DM 38 (about $24).

[Part] **II.** 1991 (15 December). Vol. 140. 129 pp. ISBN 3–924500–78–9 DM 30 (about $20).

To be included, a work must have a bibliography of at least 100 titles. There seems to be no time limit; one work listed is from 1842. More than 2000 items are included, and there is an extensive (though realistically incomplete) subject index. —L.M. Van Valen

The South American Gracile Mouse Opossums, Genus *Gracilianus* Gardner and Creighton, 1989 (Marmosidae, Marsupialia): A Taxonomic Review with Notes on General Morphology and Relationships. **Philip Hershkovitz.** 1992 (30 October). Fieldiana: Zoology (New Series) 70. Field Museum of Natural History, Chicago. vi + 56 pp. Acid-free paper. No ISBN. Softbound. $17.00.

More here than meets the eye. The family Marmosidae is itself new, as is another one (Glironiidae), the latter appearing as a *nomen nudum* because undiagnosed, as is also one of three new subfamilies. Half the text is a comparative survey of more or less important anatomical features of American marsupials, well done (except for some strange mislabeling on the composite didelphoid skull) and with some new results, such as confirming the existence of an entotympanic bone [from the photograph, of the rostral variety and thus putatively homologous to the cartilage of the Eustachian tube] in microbiotheriids. Nice figures too, although some photographs are reproduced too small for good examination. Also nice drawings of the heads of four species of *Gracilianus* and four other marmosids. There are four new species in *Gracilianus*, one unnamed.

In the past few years there has been a remarkable inflation of taxonomic categories in the Marsupialia. This started as an overdue attempt to make them comparable to categories in the Placentalia, but it has progressed so far that the disparity has been reversed. This applies to both American and Australian marsupials. I agree that *Gracilianus* is probably appropriately separated at the generic level from *Marmosa*, but dividing the extant Didelphidae (excluding the microbiothere once put there but which is here placed in its own cohort) into four families isn't helpful to the nonspecialist and is unnecessary for the specialist.

This series has a cover so dark that it inhibits reading the print. -L.M. Van Valen

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Monografía de la familia Hydrochoeridae (Mammalia: Rodentia). **Alvaro Mones.** 1991 (2 April). Courier Forschungsinstitut Senckenberg, vol. 134. Senckenberganlage 25, 6000 Frankfurt/M., Germany. v + 235 pp. Apparently acid paper. ISBN 3-924500-63-0. Softbound. DM 50 (about $32).

Capybaras. Big rodents. There actually are two extant species, not just one, as is shown here. The family is one of the most recent to originate, only about 10 million years ago, and had a modest radiation in South America before declining in apparent response to invaders from the north. (But they got as far north as the United States too, and one species still lives in Panama.)

Mones's revision is meticulous. There's a new subfamily, coauthored with Vucetich, a justified phylogeny, and an immense bibliography, but only new information gets into the figures. Therefore most taxa aren't illustrated, which isn't as bad as it sounds because most of the known evolution is in the repetitive plates of the cheek teeth, for which changes are easily visualized from verbal description. Otherwise it's a definitive work. -L.M. Van Valen

The Emergence of Animals: The Cambrian Breakthrough.
Mark A.S. McMenamin and **Dianna L. Schulte McMenamin.** 1990. Columbia Univ. Press. x + 217 pp. Apparently acid paper. ISBN 0–231–06646–5. Softbound. $17.00.

We usually think of the Cambrian breakthrough (or silent explosion) in terms of the animal groups which originated and diversified then. At least as important, though, was its other side, with possibly the greatest reorganization of biotal evolution in the history of life. This was when the modern system of multiple trophic levels appeared, with ramifying consequences. The authors give a good and accessible treatment of the revolution, dealing with geology and environment as well as the organisms and ecosystems. Both sides of the boundary are treated in some depth. They agree with Seilacher's view of a Garden of Ediacara (their name), a world of quilted autotrophs in the Vendian. This may well be correct, although the occurrence of such organisms in deep-dark as well as shallow water is a bit awkward. There are a number of new proposals in the book, among which I find the linear continuum of "soft" (autotrophs) to "hard" (keystone predators) ways of life, with deposit feeders in the middle, unhelpful. There are even some nice new photographs of body and trace fossils. Russians have done important work in this interval (in fact the Vendian Period and the earliest Cambrian stages were named in Russia), and some of their work I hadn't known about before this book. Outsiders and insiders should find interest in the book, and it's well put together. Not physically, though – several groups of pages fell out of the review copy. –L.M. Van Valen

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Evolutionary Genetics and Environmental Stress.
Ary A. Hoffmann and **Peter A. Parsons.** 1991 (7 February). Oxford Univ. Press. ix + 284 pp. Apparently acid paper. ISBN 0–19–857732–X. Hardbound. $75.00.

In a broad sense, any less-than-optimal aspect of the environment is a stress on the referent organism. The authors choose to focus on relatively severe stresses of generally applicable kinds, and they don't consider biotically-caused stresses except for some interactions with physical stresses. It's still a wide topic, and the book must sample from it.

There is ordinarily quantitative-genetic variation available in a population that lets it respond evolutionarily to any sort of stress, if the stress isn't severe enough to destroy the population. Maybe the stress doesn't recur; in that case the resistance may be lost because of a cost. Or maybe the resistance is also a resistance to other kinds of stresses, as by slow growth (plants) or low metabolic rate (animals). Response to stress can be via plasticity, too, and plasticity itself is subject to selection. Stress can affect parameters like recombination and heritabilities and genetic variance. Stresses commonly determine how far geographically a species can survive, and this has implications for conservation.

These are the sorts of things the book is really about, and it covers them well, with a good mix of theory, natural history, and experiment. –L.M. Van Valen

Jurassic and Cretasceous Floras and Climates of the Earth.
V.A. Vakhrameev. 1991 (October). Cambridge Univ. Press. xix + 318 pp. Apparently acid paper. Hardbound. ISBN 0-521-40291-3. $100.00 (minus 50 cents).

This book was (posthumously) published in Russian in 1988; it is a valuable work. Vakhrameev pioneered the mostly latitudinal zonation of later Mesozoic floras, based to a considerable extent on his original descriptions of floras from the USSR.

Most of the book is a summary of the composition of individual floras worldwide, mostly megafossils although some palynological information is included. This floral information is, barely, synthesized to provide geographic and inferred climatic patterns over roughly 30-million-year intervals. (The author was one of the last people to include the Danian stage in the Cretaceous rather than the Paleogene.) The evolution of the floral provinces themselves is also summarized, here going back to the Paleozoic; their number is variable but always small. There is a short postscript of more recent work, and the translation also adds photographs of fossils. Way too many misprints, though.

Only botanical evidence is used to infer climates, with occasional supplementation from lithology. This isn't to say that Vakhrameev should have written a different and less authoritative book, but the reader should have been made aware of the existence and nature of other evidence. Marine microfossils are mentioned once. However, the rationale for climatic inferences is only implicit, and the temporal syntheses are very sketchy. But the floras are here if someone wants to use them in a reinterpretation. -L.M. Van Valen

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The Evolutionary Process: A Critical Study of Evolutionary Theory. Edition 2.
Verne Grant. 1991 (28 November). Columbia Univ. Press. xviii + 483 pp. Acid-free paper. ISBN 0-231-07324-0. Hardbound. $52.00.

I liked the first edition of this book. I like the second edition less. Grant's primary focus has been around the species level, and not surprisingly this is where the book is strongest, together of course with the botanical aspects. I appreciate his now heterodox defense of selection for reproductive isolation. Adaptation and the phenotype are appropriately more emphasized than genes, and most areas of evolutionary biology (even, praise be, development) receive some treatment. Oddly, quantitative genetics and life-history theory are among those that don't. His discussions of macroevolution and molecular evolution are painfully superficial; perhaps this is why he is still somewhat skeptical of a symbiotic origin for mitochondria. There is a remarkable misunderstanding of the neutral theory. Go with Futuyma's book (or even Ridley's) instead, but do supplement them.

-L.M. Van Valen

Human Adult Odontometrics: The Study of Variation in Adult Tooth Size. **Julius A. Kieser.** 1990. Cambridge Univ. Press. xii + 195 pp. Apparently acid paper. ISBN 0-521-35390-4. Hardbound. $54.50.

Teeth, like fingerprints, don't change after they have been formed, except by attrition and the like. This, of course, doesn't mean that there is no influence of the tangible and intangible environment. The book discusses these and other matters in a very accessible way; even the *t*-test has a few paragraphs. It's a solid piece of work, though, except for its history (e.g., it's odd to see a discussion of path analysis without a mention of Wright; the origin of Wright's analysis of drift is misplaced; Butler, rather than Bateson, is supposed to have discovered developmental fields in the dentition; Yablokov isn't mentioned when the relation of size to variation is discussed.) Developmental aspects are considered with respect to their bearing on tooth measurements, and differences among both contemporary and diachronous populations are treated. A valuable appendix, not for the most part used in the text, gives summary statistics for length and width of individual teeth of many populations; more detailed measurements are hardly ever done.

I do have a couple of substantive quibbles. Compensatory interaction in development is tested using only the first and second molars, while it is most likely to occur with the third, which develops later and with a potentially limiting amount of dental lamina and jaw space; moreover, positive regional interaction can outweigh negative local interaction to give a positive correlation. And Kieser doesn't mention the statistical pitfalls lurking in allometric analysis. Otherwise, statistical and conceptual aspects are both well done, and it's the best review available. —L.M. Van Valen

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Symbiogenesis: A Macro-Mechanism of Evolution. Progress Towards a Unified Theory of Evolution Based on Studies in Cell Biology. **Werner Schwemmler.** 1989. Berlin and New York: Walter de Gruyter. x + 226 pp. Apparently acid paper. ISBN 0-89925-589-2. Softbound. DM 70 (about $43).

Unlike molecular evolution, cell evolution is a neglected subject. Schwemmler is a major worker in this field, with a specialty in endosymbiosis. The parts of the book dealing with endosymbiosis are useful treatments, but as the subject gets farther from this the quality of the book progressively decreases to the unsatisfactory. The level of treatment follows a similar trend, being suitable for the biologically knowledgable when considering endosymbiotic topics and descending to an embarassingly superficial level of popularization at the extremes (cosmic and human evolution). There are also some odd and unsupported evolutionary processes proposed.

A prominent conclusion is that there is a natural periodic table of cells, like that of the elements. Cells are arranged by number of endosymbiotic events on one axis and (predominant, or perhaps evolutionarily latest) method of energy transformation on the other. So we have a two-way classification. So what? There are other dimensions by which cells can be classified too, cutting across these and also of functional and evolutionary interest. —L.M. Van Valen

Mammoths, Mastodonts, and Elephants: Biology, Behavior, and the Fossil Record.
Gary Haynes. 1991 (December). Cambridge Univ. Press. xi + 413 pp. Apparently acid paper. ISBN 0-521-38435-4. Hardbound. $69.50.

Haynes is a zooarchaeologist, and although modern elephants receive a good deal of attention in this book the attention is directed toward the demography and other aspects of death assemblages of the sort preservable as fossils. For the fossils, on the other hand, there is no mention of pre-Pleistocene proboscideans, and almost none of those outside North America, northern Eurasia, and to some extent Africa. In fact, the center of attention of the book is on the Holarctic proboscidean part of the megafaunal extinction a few thousand years ago. The book is for the most part a research monograph. Much of it reports on original work by the author, including both living elephants and extinct forms. This is good stuff. The descriptive science is rather tersely written and the discussions are anthropologically verbose.

So. There's a lot of meat on a narrow topic, including a 33-page appendix on age determination. Does it help solve the problem? I don't think so, although the results may later prove to be valuable for it. Haynes provisionally concludes that mastodon(t)s and mammoths went extinct from climatic change, although human hunting may have been peripherally involved. Unfortunately, the sites on which this conclusion is based are all in the southwestern United States, where there was a considerable increase in dryness. But proboscideans also lived in regions which didn't have severe droughts, in the United States as well as elsewhere. They still went extinct, and at about the same time. I evaluated dozens of arguments in both directions (including this one) over twenty years ago, and while there has been some progress since then it doesn't seem to me to have appreciably advanced either position at the expense of the other. -L.M. Van Valen

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Falkland Islands (Islas Malvinas) Hepaticae and Anthocerotophyta: A Taxonomic and Phytogeographic Study.
John J. Engel. 1990 (30 November). Fieldiana: Botany, New Series, No. 25. Field Museum of Natural History, Chicago. viii + 209 pp. Acid-free paper. No ISBN. Softbound. $40.00.

Liverworts and such. There aren't any trees on the Falklands, except for a few planted a while ago, but it does rain a bit most days. Not surprisingly there is a great floral resemblance to the Magellanian Zone of South America, with the rain-forest genera of the latter being absent. Most genera are leafy rather than thallose. Engel's treatment is primarily taxonomic, critical and with new keys which are applicable to the Magellanian also, as are the distribution maps. In addition, Engel reviews the vegetational communities of the Falklands, with special reference to their bryophytes (*sensu lato*). The first review, and a good one.

In this series the cover is so dark that it is an effort to read the print. -L.M. Van Valen

Neues zur Geologie und Paläontologie der Messel-Formation (Mittel-Eozän, Fundstätte Messel).
Edited by **Stephan Schaal**. 1991 (15 October). Courier Forschungsinstitut Senckenberg, vol. 139. Senckenberganlage 25, 6000 Frankfurt/M., Germany. (v) + 159 pp. Apparently acid paper. ISBN 3-924500-75-4. Softbound. DM 50 (about $32).

As announced in this volume, the magnificent fossil locality of Messel has at last been saved from being turned into an enormous garbage pit. Its scientific importance has been made more than potential only rather recently, with the development of techniques for preserving the specimens as the saturated shale dries out after being exposed. A new technique is the subject of one of the nine papers in this volume. Most of the others are probably of interest mostly to Messel-fanciers, e.g. on the heterogeneity of the deposits. Aquatic insects and cladocerans are reported for the first time, and are restricted to one facies. A new genus of elaterid beetle, formerly thought to be a buprestid, gets a thorough treatment. -L.M. Van Valen

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Der Messeler Ölschiefer – ein Algenlaminit.
Kurt Goth. 1990 (31 December). Courier Forschungsinstitut Senckenberg, vol. 131. Senckenberganlage 25, 6000 Frankfurt/M., Germany. 143 pp. Apparently acid paper. ISBN 3-924500-70-3. Softbound. DM 48.50 (about $31).

The Messel Formation is an oil shale where most of the organic material came from a single species of green alga in lake plankton. This alga bloomed annually, and the resulting dead cells sank and formed a lamina on the top of the sediment. For the most part this sediment was disturbed only by slumping, as the lake was deep and persistently anoxic. Counts of the laminae lead to an estimate of about a million years for the entire deposit. Guth also presents some geochemistry which helps reconstruct the nature of the lake, and he identifies three main facies of the shale. -L.M. Van Valen

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Die Koprolithen mitteleozäner Vertebraten aus der Grube Messel bei Darmstadt.
Michael Schmitz. 1991 (16 September). Courier Forschungsinstitut Senckenberg, vol. 137. Senckenberganlage 25, 6000 Frankfurt/M., Germany. 159 pp. + 20 plates. Apparently acid paper. ISBN 3-924500-73-8. Softbound. DM 50 (about $32).

Messel has produced several thousand coprolites, the category here being used broadly to include fossils of regurgitation pellets as well as of feces. A number of shapes, sizes, and diets occur. The different forms can be ascribed, with more or less certainty, to the various groups of aquatic vertebrates reported from Messel. (One type looks like it came from a lungfish, though; lungfishes are otherwise unknown in the Cenozoic of the northern continents.) Microstructure is preserved and well illustrated. Chemical analysis suggests acidic water, perhaps a blackwater environment. -L.M. Van Valen

Solnhofen: A Study in Mesozoic Paleontology.
K.Werner Barthel, Nicola H.M. Swinburne, and **Simon Conway Morris.**
1990 (December). Cambridge Univ. Press. ix + 236 pp. Apparently acid paper. ISBN 0–521–33344–X. $59.50.

The classic late-Jurassic locality Solnhofen is best known as the source of all seven known specimens of *Archaeopteryx* (one apparently stolen very recently), but its excellent preservation potential results in lesser treasures also. The book is officially a revised translation of Berthel's 1978 book in German, but Swinburne has added as much that is new and has increased its scientific level.

The bestiary is still here (also depictions of some plants and algae, but they aren't as well preserved). And it's well done, except that the general discussion of them is now out of keeping with the rest of the book. Most of the major animal phyla occur, with both marine and terrestrial species represented. Both faunas are relatively depauperate and consist mostly of organisms which lived outside the area of deposition. The basin was inimical to most life apparently because of hypersalinity inside a fringing reef, as is reasonably well shown from several lines of evidence. Much of the book, in fact, is devoted to paleoenvironment and deposition, as well as the geology. Storms seem to have moved both calcareous mud and organisms into the multi-basined lagoon, where the mud settled slowly from suspension. Other mud arrived via slow water movement. A microbial mat may have covered the sediment, but if so the evidence for its existence is surprisingly scant. An appendix contains a putatively complete list of the biota, to species for the protists and genus for the rest, but without familial identification. A water–strider (Gerridae, Hemiptera) is here somehow referred to the Phasmida.

–L.M. Van Valen

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Theoretical Morphology, an Annotated Bibliography 1960-1990.
Wolf-Ernst Reif and **David B. Weishampel.** 1991 (30 December).
Courier Forschungsinstitut Senckenberg, vol. 142. Senckenberganlage 25, 6000 Frankfurt/M., Germany. vi + 140 pp. Apparently acid paper. ISBN 3-924500-80-0. Softbound. DM 30 (about $20).

The compilers restrict their subject to "the realm of biological forms that can be created by a morphogenetic program or a growth program by modifying the parameters of these programs." One would think that this includes developmental genetics, but there seem to be no papers by either geneticists or mainstream developmental biologists. A paper of mine published in Evolution is included but not one, probably more relevant to the restricted criterion actually used, in Developmental Biology. Nevertheless, this is a genuinely valuable compilation. It covers animals, plants, and protists (here missing Winfree on the spiral waves which can be produced in slime–mold aggregations). The relevant literature is widely scattered and is not otherwise easily retrievable from a few entries into it.

–L.M. Van Valen

Grzimek's Encyclopedia of Mammals.
Edited by **Bernhard Grzimek.** 1990. McGraw-Hill. Translated from German of 1988. 5 volumes, each (viii) + 648 pp. except 643 pp. in vol. 3. Apparently acid paper. ISBN 0-07-909508-9 for set. $500.00 for set.

Mammals! Celebratory, yes, but also the best single reference on the natural history of the worldwide fauna of mammals. There are several thousand gorgeous colored photographs (even, may the saints preserve us, two of live *Solenodons* twitching their tubular noses apparently in their natural habitat: this is a rare and interesting insectivoran.) The photographs are almost all of animals doing something. But there's still enough room for lots of text. This deals with natural history in a broad sense and is held together by synopses of phylogeny and fossils written by Erich Thenius. Mostly it's mammals alive, though, and by experts rather than hacks. The gorilla gets 40 pages, some by Dian Fossey shortly before she was killed, and the common hippopotamus 16 pages, but for bats only the European species, for the most part, are mentioned individually. Otherwise there is little Eurocentrism. There is a lower amount of inaccuracy than one probably ought to expect in a general work like this, and a half page or so of references is hidden away at the end of each volume. The translation is rarely awkward, but there are many too many misprints of scientific names and of people. The volumes here shouldn't be confused with the four volumes on mammals in Grzimek's Animal Life Encyclopedia, published in German in 1968 and in English in the early 1970s. A booklet by the publisher says that each volume of the current set is available separately too, but an apparently later flyer says that they aren't. Grzimek, a major figure in conservation of mammals, died shortly before publication of the German edition. It's worthy and it suits him. -L.M. Van Valen

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Individual Development and Evolution: The Genesis of Novel Behavior.
Gilbert Gottlieb. 1991 (14 November); stated 1992 in book. Oxford Univ. Press. xii + 231 pp. Acid-free paper. ISBN 0-19-506893-9. Hardbound. $35.00.

The author thinks that we should stand amazed at the existence of environmental input into development. He even thinks that it shows that development isn't programmed – has he never written, or even seen, a program with conditional feedback? He thinks that it is heretical to advocate behavioral change occurring before morphological adaptation to it. He is a psychologist, and his lack of understanding of evolutionary genetics is sometimes embarrassing, as in his discussion of speciation. Nevertheless, his background does let him provide lots of examples I didn't know, and he gives an unusually good, if rather brief, history of some aspects of development in evolutionary thought. -L.M. Van Valen

Die mikroendolithischen Spurenfossilien im Alt-Tertiär West-Europas und ihre palökologische Bedeutung.
Gudrun Radtke. 1991 (16 September). Courier Forschungsinstitut Senckenberg, vol. 138. Senckenberganlage 25, 6000 Frankfurt/M., Germany. 155 pp. + 14 plates. Apparently acid paper. ISBN 3-924500-77-0. Softbound. DM 50 (about $32).

Small borings in shells and the like are common in the fossil record, but until a few years ago they were difficult to study except for those, as by some bryozoans, which remained open to the surface throughout. They can now be injected like school preparations of a vertebrate vascular system, and the enclosing calcareous substrate removed by acid. The result is often spectacular detail. Radtke has looked at material from diverse facies from the early Eocene to the late Oligocene of northwest Europe. A fair number of groups seem to have produced different sorts of burrows, and various new forms of burrows are identified. Their morphological identification fits pretty well with where the putative makers of different kinds live, and the endolithic biota varies in composition with respect to several environmental variables. -L.M. Van Valen

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Intervening Sequences in Genetics and Development.
Edited by **Edwin M. Stone** and **Robert J. Schwartz**. 1990. Oxford Univ. Press. xii + 203 pp. Acid-free paper. ISBN 0-19-504337-5. Hardbound. $40.00 (minus 5 cents).

Introns commonly occur at boundaries between structural and functional domains of the protein coded for by their gene. Many genes of eukaryotes (where introns now mostly occur) seem to have originated as a result of shuffling of pre-existing exons, and during development some genes have whole or partial exons variously included or not in the mRNAs of different cells. The idea that introns were present in protein-coding genes of the common ancestor of all surviving organisms is intriguing and has some evidence. In fact the editors show how such compound genes could have originated by the advantage of having the DNA for parts of a single protein be together. The complementary view, however, also has some evidence and is not represented in this book, although one chapter does give mechanisms by which two kinds of introns can be inserted. Nongeneticists may find a bit too much unexplained, but the book is good for what it does. -L.M. Van Valen

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Genes IV.
Benjamin Lewin. 1990. Oxford Univ. Press. xxii + 857 pp. Apparently acid paper. ISBN 0-19-854268-2. Hardbound. $45.00.

I.e., the fourth edition of Genes. It and the books by Watson et al. and by et al. and Watson are the standard treatments of molecular cell biology as now practiced. Lewin's focus throughout is on nucleic acids; even proteins are pretty much ignored after they get synthesized and placed where they belong. There is a bit of development, but it is restricted to *Drosophila* and homeoboxes elsewhere, as if this is to be a basic paradigm for all multicellular organisms. The existence of real differences among organisms is recognized, of course, for genome size and organization. Not much evolution, but one writes on what one knows. -L.M. Van Valen

Brutkammen der Stenolaemata (Bryozoa): Konstruktionsmorphologie und phylogenetische Bedeutung.
Priska Schäfer. 1991 (16 September). Courier Forschungsinstitut Senckenberg, vol. 136. Senckenberganlage 25, 6000 Frankfurt/M., Germany. 269 pp. Apparently acid paper. ISBN 3-924500-74-6. Softbound. DM 90 (about $58).

In bryozoans, more than in most colonoids, individuoids often become specialized for different functions, thereby becoming organs of the whole colonoid. Brood chambers are one such specialized type; like most others, they are absent in some bryozoan groups. Stenolaemates were the dominant bryozoans of the Paleozoic but still survive in a depauperate but speciose manner. Schäfer treats mostly the post-Triassic radiation. Bryozoans grow in many ways, for good adaptive reasons, and although growth forms characterize some higher taxa most are evolutionarily plastic in this respect and there is much homoplasy. Sometimes a brood chamber expands so as to cut off putative feeding individuoids from the surface. There is too much in Schäfer's work to summarize; it's well done and well described. A byproduct of the analysis is a revised phylogeny of the Cyclostomata (whose well-known name may as well be preserved, particularly since its homonym in the Chordata is now in disuse). -L.M. Van Valen

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Morphogenesis: The Cellular and Molecular Processes of Developmental Anatomy.
Jonathan Bard. 1990. Cambridge Univ. Press. xi + 303 pp. Apparently acid paper. ISBN 0-521-36196-6. Hardbound. $54.50.

The book deals with processes after pattern formation. Thus developmental genetics is peripheral, perhaps even too peripheral for a book published as late as 1990. But it's good to see a treatment where molecular knowledge isn't regarded as the unknown touchstone which will solve all problems when discovered. Bard's approach is unapologetically phenotypic – causal and subtle, and phenotypic. The book considers only animals, mostly vertebrates at that, with a nod to the developmentally rather animal-like alga *Volvox*. Developmental biologists usually do forget about plants and such; plants are different, indeed, but some aspects of their morphogenesis have interesting parallels with some colonoid animals. But even the latter are unfortunately outside the usual scope of such work. Repetitive structures aren't, though, and even they hardly get mentioned here.

Enough of that. What the book does do is explain phenomenologically, and causally to the extent possible, how cells form tissues and then larger structures. There are lots of examples, some described in detail, and a major concern is with relevant experiments. Not details of experiments, but their structure, rationale, and significance. Self-assembly of tissues is an interesting repeated theme. Bard's concern is elsewhere, but I found a good many examples I hadn't known, of homologous structures with appreciably different development. Well written, and overall one of the best books in developmental biology as a whole. -L.M. Van Valen

Die Beschriebung der Messelornithidae (Aves: Gruiformes: Rhynocheti) aus dem Alttertiär Europas und Nordamerikas.
Angelika Hesse. 1990 (10 October). Courier Forschungsinstitut Senckenberg, vol. 128. Senckenberganlage 25, 6000 Frankfurt/M., Germany. 176 pp. Apparently acid paper. ISBN 3-924500-67-3. Softbound. DM 44 (about $27).

Messelornis is a half-meter-tall, short-winged, vaguely crane-like bird known from hundreds of specimens from the Eocene of Messel. The complete skeleton, and more, is known. Hesse has referred it to its own family, which underwent a small radiation in Europe and North America during the Paleogene. This volume mostly describes and figures the material from Messel. There is also a phylogenetic analysis, in which the family forms a justified clade with the extant monotypic families Rhynochetidae (New Caledonia) and Eurypygidae (Neotropica). Within this group, Hesse associates the Eurypygidae and Messelornithidae as both a clade and a taxon. The taxon is justified by the characters given but the clade is not, because the polarity of the distinguishing characters is unclear. Perhaps the common ancestor of the three families would be referable to a slightly expanded Messelornithidae (the known members of the latter do form a clade of their own), and in any case the geographic disjunction of the recent families is ameliorated. In addition, Hesse removes the rather recently extinct New Zealand family Apterornithidae from the Gruiformes.

-L.M. Van Valen

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Response to review

Received 2 June 1992

I very much appreciate V. C. Maiorana's review of my book THE ROOTS OF THINKING. I must, however, correct a false attribution. My book does not represent a "quest to find the origins of unique human traits."On the contrary, my book is devoted to demonstrating evolutionary continuities and to showing that fundamental human concepts have not arisen *de novo*. In addition, insofar as my book focuses on showing the relevance of,animate form and tactile-kinesthetic experience to the formation of fundamental concepts, its thesis in no way fails to apply "to chimps and even gorillas." After all, chimps and gorillas are also animate forms; and they too are conceptually astute. Not only this, but my book gives examples of such nonhuman animal intelligence. I call attention, for example, to Emil Menzel's work with chimpanzees (pp. 119-120) and call attention as well to his astute observation about why chimpanzees do not point (p. 123). —**M. Sheets-Johnstone**

In that Sheets-Johnstone claims not to be seeking the origin of human uniqueness, my criticism on this point was unwarranted. Nevertheless, she does explore what are considered unique human traits, and why do this except to seek the roots of human differences from other animals? I would look forward to her explicit exploration of this topic. —**V.C. Maiorana**

Evolutionary Theory 10: 182 (December, 1992).