cerned with the marine geology and micropaleontology of the Chinese seas will find this book very useful.

Acta Micropalaeontologica Sinica has just begun appearing, but clearly it will be a premier paleontologic journal. The papers in the first two issues cover all aspects of micropaleontology from Precambrian to modern studies. Charophytes, conodonts, and algae, as well as the more common foraminifera and nannofossils, are represented. Again, the authors are very much aware of the world's current literature on their groups. Although all the contributions are in Chinese, each is followed by a long English abstract. Plates showing photomicrographs of thin sections, scanning electron micrographs, and reflected-light photographs are reasonably well done. The articles in this journal will be of interest to a wide variety of micropaleontologists.

These two publications demonstrate that Chinese micropaleontologists are very busy, that they are attacking problems of importance to the discipline in general, and that their work is worthy of the most careful consideration by workers elsewhere. Chinese micropaleontology has extended itself to the rest of the world, and we should pay attention.

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Glirology

Evolutionary Relationships among Rodents. A Multidisciplinary Analysis. W. PATRICK LUCKETT and JEAN-LOUIS HARTENBERGER, Eds. Plenum, New York, 1985. xvi, 721 pp., illus. $110. NATO Advanced Science Institutes series A, vol. 92. From a workshop, Paris, July 1984.

Rodents are all around us, living without invitation in our homes, gnawing at our crops and stored supplies, giving us diseases, and serving as laboratory animals. There are thousands of species of them spread over the world at present, and thousands more in the known fossil record. Yet until recently we have known little about their interrelationships and, indeed, even their place among other orders of mammals.

Until the 1930's, known rodent diversity was largely confined to the living fauna, with few students devoting even a portion of their time to fossils. Robert W. Wilson and Albert E. Wood had the field of fossil rodents pretty much to themselves in those days. Since World War II the situtation has changed spectacularly. Like primatology within mammalogy, there is now "glirology," whose practitioners hold their own meetings and symposia. Even within glirology, sizable splinter groups—for example, students of the family Muridae—threaten to form subdisciplines.

Two schools of glirological thought have long waged war on one another. The issue is whether the porcupine-like rodents (Hystricognathi) of the world are a natural group. Wood has long held that parallelism in rodents is rampant and that close similarities between South American and Old World porcupine-like rodents were independently derived. Stuart Landry and René Lavocat have championed the naturalness of the porcupine-like rodents but have not been able to explain their geographic distribution without recourse to unknowable dispersals across the South Atlantic Ocean or long cryptic occupancy of landmasses on which their fossils should have become known by now if the animals were truly resident. Now the fight seems to be ending, as new studies of comparative biochemistry and morphology of living rodents are combined with study of new fossil finds. New perspective is emerging. Willi Hennig's techniques of phylogenetic analysis are also being brought to bear. The time has been ripe for the appearance of a major work on rodent interrelationships. Luckett and Hartenberger have produced one; a new era of tighter intellectual discipline and of many-faceted approaches to the subject has begun.

The book addresses two major questions: what is the position of the orders Rodentia and Lagomorpha among other orders of mammals? and how are the rodents interrelated?

Certainly, the meatiest discussions in the book result from new information about the early Cenozoic Asian family Eurymylidae, long considered early members of the order Lagomorpha rather than Rodentia. However, nearly complete eurymylid skulls described in a chapter by C.-k. Li and S.-y. Ting are unmistakably rodent-like. Moreover, both M. J. Novacek and Li and Ting note various cranial similarities of eurymylids to Lagomorpha as well as to rodents, thus supporting a superordinal collocation of rodents and lagomorphs in Cohort Glires. Novacek's cladogram (p. 77, fig. 5) is not the most parsimonious one that could be constructed from his data (pp. 68–71, table 2), but it is interesting because Dermoptera are hypothesized to share some features with rodents and lagomorphs. Inasmuch as Dermoptera are similar in some ways to primitive primates, it may be that McKenna (*Am. Mus. Novit.* **2037**, 1 [1961]), Wood (*Trans. Am. Philos. Soc.* N. S. **52**, no. 1, 1 [1962]), and P. M. Butler (this volume) are not far off the mark in suggesting that the rodent dental pattern is somewhat like that of very primitive primates, even though the astragalo-calcaneal structure of Primates diverged early from a more primitive structure. My analysis of Novacek's data suggests that, among the animals analyzed, Primates are a sister group of Dermoptera, Rodentia, and Lagomorpha. However, Butler's attempt to derive the dental pattern of lagomorphs from that of an early eurymyloid does not work, as one can see from studies of *Pseudictops* (which lacks gliriform incisors but has a lagomorph astragalus and calcaneum) and *Hsiuannania*. Lagomorph foot structure is very different from that of rodents, which implies either long evolution apart or rapid divergence of the lagomorph foot from an earlier rodent-like foot.

In chapters devoted to different approaches, V. M. Sarich, J. Shoshani et al., N. Lopez Martinez, and F. S. Szalay reach independent but similar conclusions for both molecular and anatomical reasons, but Luckett puts in a strong bid for naturalness of Glires and remoteness of Glires from Primates on the basis of dental homologies and a penetrating study of fetal membranes. (In a note added in proof, Shoshani supports the Glires hypothesis on unspecified morphological grounds.) If Glires is valid, differentiation into rodents and lagomorphs was nevertheless probably a Cretaceous event. Indeed, as Li and Ting note, an Asian late Cretaceous eurymyloid with a gliriform incisor may already have been found.

The treatment of the second major question addressed by many contributors, naturalness or convergence of the hystricognath rodents, leaves me with the impression, in spite of some strong rearguard action by Wood, that Landry and Lavocat are vindicated. Porcupine-like rodents of both the New and the Old World are a natural group with an ancient but unknown continuous distribution. However, in agreement with Wood, I see no need for an unlikely South Atlantic oceanic crossing in order to explain their distribution.

Early Asian ctenodactyloid rodents also emerge as closely similar in dental pattern to the archaic eurymyloids. W. George shows that ctenodactyloids have a number of hystricognath characters, although they are usually put with the sciurognaths. On the basis of Lavocat and Parent's studies of the ear region, paramyids lose ground as ur-rodents. On the other hand, W. W. de Jong shows that muroids, which have primitive ear regions for rodents, are unique among vertebrates in the possession of a duplicate alpha crystallin A chain (a lens protein) that has an intron of 23 amino acids between loci 63 and 64 of the chain.

I was struck by the fact that most of the contributors use phylogenetic methods and are willing to explore new ideas without bias. Disagreements abound, but generally the discussion is refreshingly civil. The editors were able to achieve fairly uniform standards, given the diversity of viewpoints of the contributors. A modicum of pompous graffiti did sneak through in one contributor's effort, but then you can't win them all.

The book is outrageously expensive in view of the fact that the editors did most of the work and it was prepared from camera-ready copy, but it is absolutely fascinating.

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The Study of Polymers

Polymers. The Origins and Growth of a Science. HERBERT MORAWETZ. Wiley-Interscience, New York, 1985. xviii, 306 pp. $47.50.

Despite the enormous technical importance of polymers and their crucial role in living organisms, polymer chemistry makes up a very small part of the formal training of most chemists. Further, although polymers have intrigued (and plagued) chemists since the beginning of their science, the history of polymer science has had little attention.

In tracing its development since the early 19th century (the term "polymeric" was apparently first coined by Berzelius in 1833) to 1960, Herbert Morawetz makes a major contribution to this branch of the history of chemistry, since he not only has read innumerable original papers (from which he quotes copiously) but has consulted company files and interviewed surviving leaders of earlier days. The result, though not always easy reading, gives a revealing picture not only of the growth of polymer science but also of the halting and confused manner in which scientific understanding actually develops.

Indeed, from reading this book one concludes that the development of polymer chemistry was particularly halting and confused. Although polymeric natural products such as rubber and cellulose were available and were studied since the beginning of organic chemistry, the possibility of understanding their structures had to wait for the acceptance of the covalent bond model for small molecules in the later 1800's. (Morawetz gives a good account of this and of the lack of communication between organic and physical chemists of the period, with the former actively using structural concepts in their work while some of the latter still questioned the existence of molecules.) The embarrassing fact is that it actually took another 50 years and much controversy before simple covalent structures were universally accepted for polymers. E. E. Slosson's *Creative Chemistry* (1920), which introduced me to organic chemistry, still described rubber as an isoprene dimer, and Conant's *Organic Chemistry* (1934), my college textbook, still said cautiously that vinyl polymers were "probably" long linear chains. If there were villains in this controversy, they were the colloid chemists, who grouped an ill-assorted mixture of gold-sols, micelles, and high polymers together as a unique state of matter with its own laws and properties. The primary hero was Hermann Staudinger, who devoted a career to establishing the linear covalent chain structure of most polymers, but a number of others appear as well: W. H. Carothers, P. J. Flory, and H. S. Mark, to whom the book is most appropriately dedicated, to name perhaps the best known.

Not only did it take a long time for the covalent structure of polymers to be established, many other important principles had to be repeatedly restated and rediscovered. The idea that unit cells detected by x-ray scattering could be smaller than the molecules producing them was proposed by Polanyi in 1921 but was still being debated several years later. Again, cryoscopic and osmotic molecular weights of polymers were repeatedly discarded when they failed to agree with preconceived ideas, and I can still remember debates in the 1940's as to whether the solution properties of polymers arose because they were loose coils or extended ellipsoids.

Morawetz terms the period 1914–1942 the classical period of polymer science. True, by 1942 the different strands of knowledge that now make up polymer science had been largely drawn together, but the results were known only to a few (mostly European) chemists. I think the enormous impact of World War II is somewhat understated. The United States had to build a synthetic rubber industry virtually from scratch, and polymers had many other uses in the war effort. Impressive resources and manpower were thrown into polymer science, and to those who were involved the period 1943–1949 was really the golden age of polymer chemistry. Herman Mark had personally brought European polymer chemistry to the United States and through regular Saturday meetings at the Polytechnic Institute of Brooklyn was actively spreading the word and providing a center for the exchange of ideas. The theories and principles that had been advanced were being applied and shown to work, new techniques for determining polymer structure and molecular weights were being validated. The mysteries of emulsion polymerization were being unraveled, and the study of copolymerization and chain transfer in vinyl polymerization was providing a whole new basis for understanding free-radical chemistry.

Morawetz is a physical chemist, and he emphasizes the development of physical-chemical concepts: rubber elasticity, crystallinity, polymer solution properties. He seems less at home with topics such as polmerization kinetics and many aspects of polymer technology, although these are also treated. Also, in spite of his exhaustive reading of the literature, he occasionally has trouble with questions of who did what in fields where much of the original work was of proprietary nature or (during World War II) classified. To cite two examples from my own experience, he notes (p. 172) that, "ironically," no mention of mercaptans as transfer agents was made in Mayo's classic 1943 paper on the subject and (p. 185) that the fact that different polymerization mechanisms led to different copolymer compositions was first published in 1950. I can report that both phenomena were well understood by the Mayo group and others at the U.S. Rubber Company in 1943, although publication was withheld for several years.

These are minor criticisms. This is an excellent book that should be an important source, both to polymer chemists and to historians of science.

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Brave New Workplace. Robert Howard. Viking, New York, 1985. xii, 224 pp. $16.95.