Speculative Physiology

A Cold Look at Warm-Blooded Dinosaurs. Papers from a symposium, Washington, D.C., Feb. 1980. ROGER D. K. THOMAS and EVERETT C. OLSON, Eds. Published for the American Association for the Advancement of Science by Westview Press, Boulder, Colo., 1980. xxx, 514 pp., illus. $28.50. AAAS Selected Symposium Series, 28.

In reviewing a book on the thermal biology of dinosaurs, a certain tolerance is required. A reviewer must be able to accept the inconclusiveness of "paleophysiology," rather than demand the dreary preciseness of "real" physiology or biochemistry. But if intellectual inquisitiveness and expansiveness are desired, they can be found in this excellent book.

Whereas the usual view of dinosaurs has been that they, like most living reptiles, were "cold-blooded" and had low rates of metabolism, some biologists have forcefully argued that these animals attained the position of prominence they held for some 140 million years because they were "warm-blooded" and had high rates of metabolism. The evidence adduced in favor of these high rates is derived principally from posture, predator-to-prey ratios, and bone histology. All of these lines of evidence, and more, are thoroughly examined in this book: predator-prey ratios by J. O Farlow and by P. Béland and D. A. Russell, bone structure by A. J. de Ricqlès, thermal physiology of living reptiles by N. Greenberg and by P. J. Regal and C. Gans, the parietal-pineal complex by J. J. Roth and E. C. Roth, the allometry of temperature regulation and metabolism by J. R. Spotila and by M. E. Baur and R. R. Friedl, and brain size by J. A. Hopson; J. H. Ostrom, N. Hotton, and R. T. Bakker each make a general analysis of the thermal biology of dinosaurs. My only criticism of the book is that some of the contributors could have addressed dinosaurs more directly (Greenberg), some take an unnecessarily narrow view of a complicated problem (Spotila), and some are too windy (especially Bakker, who consumes repetitiously a nearly endless 112 pages).

Even though many issues remain unresolved, there are remarkable areas of agreement and convergence of ideas. With the exception of Bakker, who retains his early commitment to the view that "the close similarity between dinosaurs and large mammals in bone histology and predator/prey ratios . . . *must* be interpreted as *direct* evidence of high rates of metabolism in *all* dinosaurs" (p. 355, my italics), most of the authors agree that the large body sizes of many dinosaurs assured them of reasonably constant body temperatures, and most of these authors would probably agree that large mass may have prevented dinosaurs from having the high rates of metabolism typical of mammals and birds. Béland and Russell and de Ricqlès argue that at large sizes dinosaurs and mammals are physiologically convergent. In fact, as body mass surpasses 100 kilograms the relative importance of surface area so diminishes and the importance of mass becomes so great that the classical dichotomies of warm-blooded vs. cold-blooded, homoiothermic vs. poikilothermic, and endothermic vs. ectothermic, which were designed for small animals, may no longer apply.

There is evidence to suggest that some small theropod dinosaurs, namely the coelurosaurs, may even have been endothermic to some (unknown) degree. For example, not only do they have bone histology indicating thermal constancy (which de Ricqlès might prefer to call endothermy), Roth and Roth show that some small coelurosaurs had a pineal body and no parietal eye, a condition found today in most living endotherms. Living ectothermic vertebrates normally have both the parietal eye and a pineal. Most dinosaurs, however, lacked both the pineal and the parietal eye, a state found in some present-day mammals that have low body temperatures and very low rates of metabolism (such as edentates and dugongs). Furthermore, Hopson shows that the only dinosaurs to have a brain size relative to body size equivalent to the lowest values found in mammals and birds are coelurosaurs.

Baur and Friedl and Hotton argue that mammals and dinosaurs differ radically in thermal biology. One of the most important bits of evidence favoring this difference may be that various mammals, including multituberculates, marsupials, and eutherians, survived the transition from the Cretaceous to the Paleocene, whereas no dinosaurs made this transition. If dinosaurs were endothermic, at least some species should have survived to the Cenozoic. In fact, one surprising aspect of this symposium is how little space is given to the extinction of dinosaurs, which surely bears some relation to their thermal biology. Hotton has suggested that if dinosaurs were endotherms they should have also occupied small-tetrapod niches. The use of thermal inertia by dinosaurs might explain why they remained large; it may be significant here that small coelurosaurs are the most likely candidates for endothermous dinosaurs. If (large) dinosaurs are judged to have been inertial homoiotherms, then Bakker asks a perceptive question: why should mammals use the expensive process of endothermy to attain homoiothermy, while dinosaurs used thermal inertia? It may be necessary to question Bakker's assumption that the "superiority" of both dinosaurs and mammals was based on endothermy. There is reason to believe that the high rates of metabolism in mammals are related to factors other than temperature regulation.

This symposium also raises some new questions and suggests new answers. Hotton suggested that the early dichotomy in the evolution of reptiles between the pelycosaur-therapsid line and the thecodont-dinosaur line may also be an early dichotomy between the use of urea and of uric acid, respectively, as nitrogenous waste products. Could the success of dinosaurs during the Mesozoic have been related to the water conservation associated with uric acid secretion? The recent demonstration that some tree frogs in Africa and South America use uric acid as their principal nitrogenous waste product indicates, however, that nitrogen excretion may be evolutionarily plastic. Baur and Friedl suggest that the reduction in body mass during the evolution of mammals, which apparently was associated with their attainment of endothermy, was caused by a great reduction in the oxygen content of the atmosphere in the Mesozoic, an "explanation" that requires its proponents to explain why this reduction in mass was not universal and why such a reduction in atmospheric oxygen content occurred.

Even though there remains much controversy concerning the thermal biology of dinosaurs, this book has made an outstanding contribution, not only to this subject but as a demonstration of how the tools of modern biology can be used in the service of paleobiology. The correctness or erroneousness of the idea of homoiothermy in dinosaurs, of course, cannot be determined by balloting. Only time will tell whether additional information will weigh on the side of inertial homoiothermy, endothermy, or some other, unforeseen form of thermal behavior. In any case, this symposium will be viewed in retrospect as a milestone in the examination of this fascinating problem.

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Tectonic Evolution

The Origin of the Gulf of Mexico and the Early Opening of the Central North Atlantic Ocean. Proceedings of a symposium, Baton Rouge, La., March 1980. REX H. PILGER, JR., Ed. Louisiana State University School of Geoscience, Baton Rouge, 1980. iv, 104 pp., illus. Paper, $15.

Included in this volume are nine papers and 11 abstracts on the geologic development of the Gulf of Mexico, the Caribbean, and the western North Atlantic. Of the nine papers, seven deal with the Gulf of Mexico, one with western Colombia (by W. D. Mooney), and one with the Late Paleozoic-Early Mesozoic reconstruction of the continents based on paleomagnetism (P. Morel and E. Irving).

In his paper Mooney proposes that the Western Cordillera of Colombia represents an accretionary wedge or was formed as a result of the stacking of oceanic crust following a westward jump of a subduction zone during the Cretaceous. He proposes a similar origin for the Pacific Coastal Range, with the Atrato-San Juan basin between the Coastal Range and the Western Cordillera representing a fore-arc basin. Morel and Irving describe two continental reconstructions. The one for the Late Carboniferous-Early Permian has northwestern South America opposite the eastern seaboard of North America. The one for the Early Jurassic has northwest Africa against eastern North America. According to Morel and Irving the change from Late Paleozoic to Early Jurassic continental distribution occurred in the Late Permian-Triassic by right lateral motion between Gondwana and the northern continents.

In the papers dealing with the Gulf of Mexico, J. R. Garrison, Jr., *et al.* use rubidium-strontium data from the Precambrian granulite and Paleozoic greenschist near Ciudad Victoria, Mexico, to reconstruct the Paleozoic tectonic evolution (one of subduction) of the eastern continental margin of Mexico. The other papers deal with the tectonic evolution of the present Gulf. Although all the contributors believe that the present Gulf is due to large horizontal motions of the surrounding continents, they disagree as to the nature and magnitude of these motions. S. E. Cebull and D. H. Shurbet, for example, in their paper on the Ouachita belt suggest that the Gulf of Mexico is a Paleozoic basin that began to enlarge in the early Mesozoic to its present dimensions. The rest of the writers propose that complete closure of the Gulf of Mexico occurred at the end of the Paleozoic and that the present basin formed by sea-floor spreading in the Jurassic. Stratigraphic, paleontologic, and tectonic data described by R. Schmidt-Effing from the Huayacotla aulocogen in eastern Mexico would indicate that the rifting phase was initiated during the Hettangian and the drifting phase (continental separation) began in the Sinemurian. According to R. T. Buffler *et al.* only the deep Gulf of Mexico is underlain by oceanic crust, a crust emplaced by a sea-floor spreading episode that began during the Late Jurassic and terminated in the Early Cretaceous. According to these authors this oceanic crust is surrounded by a thinned continental or transitional crust. J. L. Walper, on the other hand, places the location of the continental-oceanic crust boundary inboard of the Texas coast. W. R. Dickinson and P. J. Coney propose that prior to the Jurassic opening of the Gulf the Yucatan peninsula nestled against the southern United States and eastern Mexico. Walper and W. A. Gose *et al.*, on the other hand, postulate that Mesoamerica prior to the Jurassic was not located in its present position and was displaced eastward along megashears. Buffler *et al.* suggest that salt deposition (Louann salt) was restricted to the thinned continental crust. Walper, on the other hand, proposes that evaporite deposition took place in the shallow epicratonic seas behind the elevated continental edge and atop the young oceanic crust. Walper also believes that the salt basin was originally continuous and was split in two as segments of Mexico were displaced eastward. Buffler *et al.*, however, suggest that the salt was deposited in two different basins separated by a median high. Later this high became the locus of sea-floor spreading that separated the basins into their present positions.

As a whole I found the book well written and adequately illustrated. To those working in the Gulf of Mexico it will be a welcome addition to their libraries. To me, however, the most striking thing about the proceedings is what is left out rather than what is included. There is no discussion of the nature of rifting prior to continental decoupling, and only in passing is there any discussion of the distribution of oceanic crust. Information on both of these subjects is needed to reconstruct the geologic history of the Gulf of Mexico.

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Genetics of *Homo sapiens*

Human Genetics. Problems and Approaches. F. VOGEL and A. G. MOTULSKY. Springer-Verlag, New York, 1979. xxviii, 702 pp., illus. $49.50.

Human genetics has suffered too long from being treated as general genetics with a few human touches. For that matter, medical genetics has itself been similarly treated by human geneticists. This book, written by geneticists, both with diversified experience and both physicians, should do something to restore perspective. It is not the first book in the field, but it is the first big book. It runs to 700 pages of small type and has some 1800 references, 400 figures, nine long chapters, and nine appendixes; the table of contents is itself 20 pages long. The graduate student who really wants to study human genetics at last has a major source and method book, one that is not divorced from experimental genetics but transcends it. The book is handsomely printed on fine paper in double columns with good quality half-tone diagrams.

After a short historical review (with just a little too much ancestor worship for my taste) the authors lead off with chromosomes—a tribute, one supposes, to the traditional primacy of morphology. Unexpectedly, sketches of abnormal phenotypes are widely used in place of photographs. There follows a long chapter on genetic analysis that contains all those cardinal matters that tend to be neglected in doctoral programs in human genetics. As one might imagine, the treatment is most assured in the classical segregation analysis, but there is extensive discussion of methods for the study of twins, and the authors do heroic battle (although in strictly orthodox terms) with the much less coherent field of quantitative genetics. The fourth chapter covers gene action in some detail; it is largely concerned with the genetic biochemistry of enzymes, protein polymorphisms, antibodies, and pharmacogenetics, with concern for clinical applications. The chapter has a novel