

ance of the Machalilla Phase as a cultural entity was due to the arrival of various influences bearing the stamp of Mesoamerican cultural origin.

The knottiest problem presented by this report is the question of the origin of the ceramics of the Valdivia Phase, which, on the basis of *prima facie* evidence of radiocarbon dates, is the earliest known pottery in the New World. If one were to extract all ceramic items from the Valdivia Phase, there would be little question that the remaining cultural inventory was essentially similar to other simple preceramic fishing cultures of western South America. However, pottery is present, and it exhibits sufficient sophistication to rule out any suggestion of local invention. Furthermore, despite the author's final hypothesis that Middle Jomon people of Japan introduced pottery making to a nonceramic Ecuadorian Indian fishing culture not one fragment of evidence in any of the Valdivia Phase sites excavated indicates that pottery was superimposed on an underlying nonceramic culture. In other words, all known Valdivia sites indicate that their founding societies brought with them not only a knowledge of fishing, but the art of pottery making as well.

On the basis of the factual evidence noted above, the first logical step in searching for origins is to compare the material with other pottery-making, fishing cultures along the Pacific coast of America. This has been done, even though the sites selected for comparison appear to be more recent. However, the notorious lack of success is less a reflection of the lack of adequate comparisons than of the pitifully few shell mound sites that have been excavated. Even when the authors compare the ceramic techniques and traditions of nonseafaring New World cultures, there is a seeming lack of complexes comparable to that displayed for the earliest levels of the Valdivia Phase, although individual traits may be found widely scattered in time and space.

The apparent lack of existing evidence of the American origin of Valdivia pottery would seem to suggest the same conclusion that the authors arrived at with regard to their unsuccessful search for the origin of the Machalilla Phase culture. In this latter case the lack of success brought forth the stated hope that further work in prehistoric coastal fishing cultures from Mexico to Central America might bring to light the needed evidence. However,

given the apparent lack of comparable Valdivia Phase ceramic complexes in other known prehistoric cultures of the New World, plus a seeming willingness in this case to overlook the abysmal lack of knowledge of prehistoric fishing cultures on the Pacific coasts from Mexico to Ecuador, a search for Valdivia Phase pottery origins has been carried to areas beyond the New World. This bold approach by the authors has resulted in a surprising degree of seeming success. The success is limited, however, largely to what appears to be quite valid parallels in decorative techniques, motifs, and rim and base forms of pottery belonging to the fishing and hunting culture of the Middle Jomon Period of Kyushu, Japan. To add further significance to these parallels, the range of radiocarbon dates for the Valdivia Phase is well within the range of those dating the Middle Jomon Period of Japan. Furthermore, if a boatload of Jomon fishermen did, in fact, accidentally land among the folk of an Ecuadorian village where ceramics were unknown, it is well within the realm of expectation that the Ecuadorians may have readily accepted a completely new and obviously utilitarian trait like pottery. However, because the evidence suggests that their traditional tools and equipment had proven their worth from long years of use, it is less likely that new ideas pertaining to these items would be as readily accepted.

The validity of the comparisons found in Middle Jomon and early Valdivia Phase ceramics seem acceptable to me. Moreover, the record of long-range accidental voyages in the Pacific leads me to accept as potentially possible the successful completion of an accidental voyage from Japan to the coast of Ecuador by Middle Jomon fishermen. However, these acceptances lend little credence to the hypothesis when viewed in the light of our knowledge, and especially of our lacunae of knowledge, of the Early Formative cultures of Mesoamerica and South America. Even with the little knowledge at our disposal, one must ask why, if the art of pottery manufacture was introduced to the New World by the Jomon people landing in the region of Valdivia, did not the bulk of the ceramic complex of traits move north and south to appear in the earliest known pottery horizons of Peru and coastal Colombia? This obviously did not happen, for, as the authors point out, the ceramics of the earliest

Formative cultures of Peru to the south and Colombia and Panama to the north are characterized by simple forms and are dominantly undecorated. This is precisely what one might expect if the art of pottery were independently invented in the New World, for there would be a period of simple, experimental beginnings. However, one might also reason that perhaps pottery was invented in these areas as a result of stimulus diffusion—that is, the knowledge that it was being made at Valdivia!

One could continue defending this hypothesis or tearing down its defenses to little avail. Conservative archeologists are sure to regard the hypothesis as extreme and unwarranted. Some of us who are interested in the cultural potentials of accidental, or purposeful, seafaring voyagers in the Pacific may regret a little the single-hypothesis explanation employed in this report. Many will think that more archeological knowledge, certainly in the region of Central America, is the more logical first approach than reaching across the Pacific for a hypothetical explanation. However, there is little doubt that the authors will succeed in their stated fundamental desire to stimulate field work in Colombia and Ecuador. To this I add my hope that it will also lead to more thorough distributional studies and to attempts to clarify the nature and potential results of random transoceanic contacts by peoples with different cultural traditions.

Mathematics

This textbook, **The Circular Functions** (Prentice-Hall, Englewood Cliffs, N.J., 1966. 188 pp., \$5.95), by Clayton W. Dodge, was written primarily as an introduction to trigonometry and related topics for students preparing to study the calculus and for teachers of such students. It appears to have sufficient material for a college course of two semester hours but is probably more appropriate for an advanced secondary-school course.

The volume treats aspects of elementary analytic geometry, including lines, circles, and arc length; circular functions, including their graphs, basic identities, and conditional equations; trigonometry, including the right triangle, and the laws of sines and cosines; and complex numbers. Unique features are chapters on harmonic motion and the history of trigonometry

which are much more extensive than those in most texts at this level. A brief treatment of sets and set notation is relegated to the appendix because it is really not essential to the development of the text.

The author introduces terminology consistent with modern curricular developments but at times appears to find their use somewhat cumbersome. For example, a function is defined early in the text as a special subset of the real Cartesian plane, but the author quickly points out that "equations" will, at times, be referred to as "functions," although such usage is not in strict conformance with the stated definition. The concepts of "variable" and "solu-

tion set" are introduced, but "unknown" occasionally slips into the writing.

The style of exposition is clear and concise, and the exercise sets contain many problems of a nonroutine nature. In fact, instructors should emphasize that many of the exercises are critical to the text development and very important for later study of the calculus. The author has made a fine selection of topics that need emphasis prior to a course in the calculus, and he seems to have developed his writing in a style which should be understandable by the intended audience.

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Second International Pharmacological Meeting

Drugs and Enzymes (Pergamon, New York, 1965. 516 pp., \$15), edited by Bernard B. Brodie and James R. Gillette, presents a valuable record of symposia held in 1963. The discussions by many of the speakers, most of them authorities of world renown, were so filled with solid facts and sound speculations that much of the volume is stimulating reading today and an important reference for research worker, graduate student, and teacher, despite subsequent research in most of the areas. A thorough reading should greatly increase the sophistication in the approach of the pharmacologist who is trying to relate his observations to possible cause-and-effect involvement of enzymes.

The first section contains 20 papers on the relationship between biochemical effects of drugs in vitro and their pharmacological action in vivo. Brodie's introduction points up important considerations not recognized in the past and not always clear to investigators today. However, it does not make clear that tentative proposals about where a drug may act are determined to a significant degree by the methods of study available or being used at any given time. The net influences of reversible binding are considered in great detail by Gillette, some in detail unnecessary for the more sophisticated but perhaps important for neophytes. McIlwain's discussion of ion movements in the nervous system seems dated, but Repke's discussion of the cardiac glycosides and membrane ATPase is excellent. Green-gaard and Giacobini's reports on the relation of metabolism to activity in

nerve tissue are valuable. Bacq and Liebecq present a long and complete consideration of radio-protective materials. Spector summarizes the ramifications that result from monoamine oxidase inhibition. The detailed analysis of carbonic anhydrase inhibitors (by Wirz, Maren, and Wistrand) illustrates so clearly the depth of understanding that we must have before we can with certainty ascribe physiological effects to an observed effect on an enzyme.

The second section contains an important and timely series of 21 discussions of the biochemical mechanisms of drug toxicity. The rapid increase in our appreciation of hereditary factors in individual variation and what it does to the "normal distribution" and "average dose" is discussed by Kalow and Netter, while Fouts, Conney, and Remmer present an extensive discussion of the adaptive changes in drug metabolizing systems and how they affect individual responses. Axelrod, Adler, and Williams thoroughly cover the conversion of substances to active drugs and to more toxic products. The papers by Horning, Poggi, and Heimberg give an extremely valuable summary of the mechanisms by which CCl_4 and certain other substances influence liver lipids. This work has added significantly to our knowledge of normal lipid-transport processes. The cumulative effects of reserpine on the pituitary-adrenal system, drugs causing porphyria, and a very detailed discussion of photosensitivity to drugs are also included. The great problem in determining the biochemical mechanism when a drug,

such as thalidomide, is converted to 12 metabolic products is carefully outlined by Faigle and others.

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An Introduction to Genetics

Faced with two more books on the history and significance of genetics, a reviewer cannot help but ask, "Is this book necessary?" With several excellent books of this sort already at hand, a new entry into the field should justify its existence either by unusual excellence or by unique qualities.

F. A. E. Crew's **The Foundations of Genetics** [Pergamon Press, New York, 1966. 202 pp., \$3.75 (paper)] covers familiar ground, from plant hybridization before Mendel to the human implications of genetics. It is almost entirely concerned with transmission genetics; less than 4 percent of the book is devoted to molecular genetics. There are 20 portraits of prominent geneticists, only two of whom are still alive (and both have retired). Within this framework, Crew's book is a good, though not inspiring, work. It might be useful to a student who wanted to review the development of genetics to 1953, but the general reader is not likely to find the book of much interest.

Broadly the same ground is covered in **The Language of Life** [Doubleday, Garden City, N.Y., 1966. 256 pp., \$5.95] by George Beadle and Muriel Beadle—but in a completely different manner. To begin with, it is a rare example of a book that is consistently addressed to the same audience throughout—the ignorant but intelligent layman. The authors are unusually suited to the task they undertake, one being a Nobel laureate in genetics, and the other a former newspaper woman. As they explain in the foreword: "The writing was done by Muriel, who is not a scientist; in fact, until she attempted this book, she had learned only enough jargon to know that geneticists who refer to their children as F_1 's are describing them quite precisely—if in scientific shorthand—as 'first generation hybrids.' The theory behind our collaboration, therefore, was that if George could explain genetics in terms simple enough for Muriel to grasp, *anybody* could understand it. It was our hope, initially, that we could