means as the antilog of the logarithmic mean, since he has automatically converted all data to logarithms at the beginning. These then are geometric, not arithmetic means.

The major criticism of this work concerns the sample sizes employed. One basic requirement of multivariate statistics such as $D^2$ is for adequate samples, specifically ones that exceed the number of input variables. The entirety of the 13 Sulawesi samples fall below this norm; 7 of the n's are smaller than 10! Furthermore, 16 of the 28 non-Sulawesi samples number less than 10. In fact, 17% of all the samples are of 3 or less specimens. The makeshift quality of these data just does not justify monographic treatment, and this slim volume's price is completely beyond reason. Albrecht should have pared down the introductory sections (the historical overview is typical thesis padding and not necessary to the published version), eliminated the copious raw data and univariate graphs (copies could be supplied on request to the few workers interested in them), and then submitted the remaining medium-sized article to AJPA so we could all get it for free.

ROBERT S. CORRUCCINI
*Carbondale, Illinois*

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DIE FOSSILGESCHICHTE DES MENSCHEN. By Wilhelm Gieseler. Gustav Fischer Verlag, Stuttgart. 1974. ix—357 pp., figures, tables, bibliography, index. DM 36.—(paper).

The pace of the discovery of hominid fossils has increased to such an extent that any book that attempts to summarize the human fossil record will inevitably be out of date by the time it appears in print. In the case of the book by the late Wilhelm Gieseler of Tübingen the publication date was 1974 and only a few sources as recent as 1972 are mentioned. In effect, then, the book is really an attempt to present the state of the art as of 1971. In fact, however, this is actually the third revision—now separately published for the first time—of the identically titled section that Gieseler originally wrote for Gerhard Heberer's *Die Evolution der Organismen* in 1943. If the reportage of the fossil record was more than a bit behind the times at the date of publication, the views that condition the descriptions and interpretations are even more a reflection of the intellectual past. The interpretive models most often credited are those of the French anthropologist Henri-Victor Vallois and the late Göttingen scholar Gerhard Heberer. During his life, Heberer had observed that his principal intellectual inspiration in approaching the hominid fossil record had been provided by Marcellin Boule in the first edition of *Les Hommes Fossiles* (1921). Vallois, of course, revised this work after Boule's death for its final edition but the views remained substantially the same. Gieseler, then in following the approaches of Vallois and Heberer, is actually reflecting the orientation established by Boule more than fifty years ago.

Even the order in which the chapters are presented is essentially the same as in Boule's classic work. The history of European discoveries is followed by accounts of European geology and European paleolithic archaeology. European fossil hominids are then treated—Neanderthal, "Präneandertaler," "Präsapiens," Upper then Lower Pleistocene specimens from Europe and the Near East. The latter part of the book is then devoted to finds from Asia and finally, Africa, and the farther from Europe it strays, the more perfunctory and out-of-date it becomes.

In keeping with the model on which it is so clearly based, the book is almost entirely silent concerning the processes of evolution. Mutation and selection are mentioned just once and only for the purpose of denying that there is any point in discussing their possible role in altering Neanderthal form in a modern direction. True to the paradigm of what we might call "Boulean logic," Neanderthals are considered specifically distinct from *sapiens*. They are portrayed as a peculiar and specialized form that became extinct without giving rise to any descendants. What is meant by "specialized" is not stated, although in context it would appear to mean anything that is different in appearance from the modern human condition. Adaptation in a Darwinian sense is not discussed at all, and Darwin's name is mentioned only in association with his prediction that Africa should be the most likely place to search for hominid origins.

In sum, the book is a monument to the truth of Thomas Kuhn's observation that the paradigms of what had been the normal science of yesteryear do not change with the addition of new evidence but only with the death of their holders. Heberer and Gieseler are no longer living, and it seems highly unlikely that any of their successors will produce a work that treats the hominid fossil re-

cord at such length but solely from the perspective of static typology.

C. LORING BRACE
*University of Michigan*

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FIGURING ANTHROPOLOGY: FIRST PRINCIPLES OF PROBABILITY AND STATISTICS. By David Hurst Thomas. Holt, Rinehart and Winston, New York. 1976. xi—532 pp., figures, tables, examples, bibliography, index. $13.50 (cloth).

When the second edition of this book appears, and the astonishingly large number of typos and just plain errors are removed (a lengthy Errata is furnished but less than half the mistakes noted are corrected), this book will serve as the basic introduction to probability and statistical inference for a generation or so of anthropologists. It is written to be pertinent to all anthropologists, and biologically oriented students will find it very useful. In combination with *Biometry* (Sokal and Rohlf, 1969), a strong background in statistics can be soundly begun.

Since the book is, by virtue of its coverage, excellence in writing style, and presentation of numerous examples, bound to be important in the training of the next generation of physical anthropologists, and since it is probably pretty common for a physical anthropologist to offer an introductory course in statistics in many anthropology departments, the book deserves careful consideration.

Happily, the strongest subject considered is that of the normal distribution. Thomas' treatment will be well received by most physical anthropologists. Variation is especially well covered. The book spends almost 400 pages on probability theory as a framework for understanding discrete random variables, hypothesis testing, Student's *t*, nonparametric statistics, correlation and regression, and sampling procedures. Since Thomas explicitly lists the assumptions of various tests as including randomization, it is difficult for a student to understand why the numerous examples Thomas presents are rarely, *if ever*, based on random sampling. Contributors to this journal, including the reviewer, have presented significance levels *as if* the sample was randomly selected. Thomas is aware of this difficulty (pp. 446-47) and tries to rationalize such usage by visualizing a hypothetical universe from which the observed (biased) sample *could* be thought of as being drawn. This is not convincing. I think what Thomas means is that alpha levels, even if quite incorrect due to sample bias, still serve us well as aids in reaching a decision. The problem is that by selecting an extremely biased sample, the investigator can easily get on the wrong side of the null hypothesis, virtually insuring its "rejection." One must feel uncomfortable with Thomas' opinion that "we don't have as much to lose from bending the rules of statistics, since most anthropologists probably only half-believe their statistics anyway (p. 447)." In any case, the editor of this journal should seriously consider requiring all statements of probability or confidence intervals or point estimations which are obtained from non-randomly selected samples to carry the caveat *as if drawn from a random sample* in order to remind us all of the weakness of such statements.

Of the numerous errors in the examples, problems and formulae, a few are especially unfortunate. On p. 351, for example, Model I least squares regression (where no error is present in the predictor variable) is inappropriately used to demonstrate a study of weight vs. mean annual temperature. This error is one frequently committed in this journal. Yet elsewhere (p. 374) Thomas has an excellent discussion of Model II regression. But unfortunately, he suggests that either regression line, $Y$ on $X$ or $X$ on $Y$, will serve to predict $Y$, which is not correct. Another example of internal inconsistencies lies in an example on p. 393 where an $r$ of 0.61 with a sample of 7 did not reach significance at the 0.01 level, whereas Thomas incorrectly suggests that "almost surely" the results would have been significant with a larger sample. Yet on p. 464 he correctly points out the frequent misunderstanding of the relationship between $n$ and $p$ (a larger sample should just as likely produce a larger as a smaller probability value, with unbiased samples). The common intuitive misunderstanding of statistical inference is not helped by such an uneven treatment. Other minor misuses occur. Thomas suggests that Spearman's Rho can only be compared with parametric $r$ "through tortuous analogy" while in fact any Pearson $r$ formula applied to ranked data will give the same numerical result as the shortcut computing formula of Spearman's Rho (which takes advantage of