

John Himes's article suffers from similar drawbacks. It concentrates on U.S. data and the quality of the earlier historical data is not discussed. In addition, it restricts itself to commenting on the relationship between average values of growth measurements, rather than the relationship between individual measurements. With much of the historical data this may be inevitable, but it would have been useful to have had a discussion of the difference this makes. In particular, a study of averages says nothing about the individual variability of relationships. Perhaps the most interesting data discussed in this article are the parent-child data collected on families of students attending Harvard from 1840 to 1930, and it would have been useful to see a discussion of some other parent-child series.

Malina presents a review of dietary changes from the beginning of the 20th century and discusses how these may have affected growth—especially in the infant period. He notes the difficulties in attributing causation from time series correlations and is sufficiently cautious in his conclusions. He discusses secular changes in mortality, child labor, and urbanization, again pointing out that all these changes are confounded with other factors, which makes any direct causal inferences highly problematic. There is a section dealing with the effects of various secular trends, for example, on age at marriage and the consequent implications of earlier childbearing. There is a final brief discussion of the educational and social implications of the decreasing age of physical maturity.

Neanderthal Man. *Myra Shackley.* Hamden, Conn.: Archon Books, 1980. x + 147 pp. \$19.50 (cloth).

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With new discoveries of European Neanderthals reported associated with the earliest European Upper Paleolithic, and because of the increased number of analytical papers dealing with Neanderthals, what an exciting time this is to bring out a whole book concerned with the topic. What an opportunity to bring together all the new information and ideas. Perhaps someone will do it soon.

This book would have been an important and appropriate contribution a decade or so ago,

before some of the more recent discoveries, before so many of the earlier errors had been corrected, and before the realization of sex in prehistory doomed phrases like "fossil man" and "Neanderthal man" to extinction without issue. Indeed, this book stands as an example of what happened when such phrases were used persistently—they were taken literally. This is why it is impossible to glean any information about Neanderthal *women* or their activities from this book. They were presumably left shoeless in their caves, busy producing baby Neanderthals.

This is not really meant to be a popular book, although it will probably be sold as such. It could have been a good text for an introductory course where there were other books used to explain the terms and concepts assumed. Shackley spends some time discussing who the Neanderthals were (although the term is never really defined and the group it refers to is never clearly delineated). Intermixed in this discussion is a partial history of ideas surrounding their interpretations, mainly in Western Europe and America. The strong point of this book is its subsequent discussion of Neanderthal behavior, ranging from their archaeology to their inferred patterns of hunting, use of ritual, and patterns of adaptation. These central sections bring together much diverse information which is well integrated and discussed in a useful context for better understanding these prehistoric Europeans. The book ends with the inevitable question of Neanderthal extinction, and a discussion of whether Neanderthals are alive and well in Outer Mongolia (or perhaps voting in the supreme Soviet). The end of the text is followed by an annotated, but weak, bibliography.

If it is true that the book covers all this material, discussed in a readable manner and presented so that unsuspecting generations of new college students might be unwittingly drawn into this interesting topic, what then is my gripe?

Simply put, my reaction is to the unacceptable number of erroneous statements upon which so much of this book rests. I know all of the principals involved in the Neanderthal controversy, and I have the feeling that as a group, we are very close to a series of explanations and solutions that are acceptable to all of us. What stands in the way of this, more than anything else, is the amount of noise, irrelevant information, and misinformation that must be cleared away every time the topic is discussed. Obviously this book is a victim of the problem, and not its source, and moreover, it is not the only re-

cent publication contributing to the problem's persistence (consult last winter's *Scientific American* for instance). But contribute it will, and for this reason, I feel that potential readers should be warned.

As to what kind of information problems the book has, I believe that a large number simply reflect ignorance of the last decade's literature. They involve statements such as the following: the Amud "man" was short (he is the tallest premodern fossil); the back of the Tabun skull is particularly "Neanderthal-like" (it is the *least* European Neanderthal-like aspect of the vault); the Krapina remains have never been adequately described (Smith's monograph is not in the bibliography); Neanderthals have chinless jaws (almost all of the more recent ones have chins); no modern remains are found with Mousterian industries (what about Skhul and Qafzeh); no Neanderthals are found with Upper Paleolithic industries (what about Vindija and Saint Cesaire); Steinheim and Swanscombe are described as having thick browridges (on page 8, although by page 11 Swanscombe has become a small-browed skull and the fact of the matter is that there is no frontal at all); a small nose is described as a better adaptation to cold climate than a large one; the brow ridges are said to help anchor the chewing muscles (presumably *temporalis*, but this is anatomically and functionally incorrect for hominids and indeed any primates); dental disease is described for the teeth of La Chapelle (the specimen is toothless but for two very worn premolars); etc.

The "etc." above also covers numerous interpretations that are probably incorrect, and certainly in many cases of more historical than current interest. I am tired of reading about the sapient brows of Swanscombe and Fontchevade, or the modern aspects of Ehringsdorf, but I am not going to present another long list, and in any event, interpretations should be matters of discussion. It is the fact that by and large they weren't discussed, rather than the question of whether or not I agree with the interpretations presented, that I have reacted to with the rather negative tone of this review. My objection is not to interpretation, but to interpretation presented as fact.

In the end, what is most aggravating about this book is its potential. What it could have been is a tantalizing goal. I hope it will be reached.

Human Physiological Work Capacity. R. J. Shephard. International Biological Programme,

15. New York: Cambridge University Press, 1978. viii + 303 pp. \$47.50 (cloth).

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In assembling and evaluating the data pertaining to human physiological work capacity that resulted from the efforts of the many participants in the human adaptability (HA) project of the International Biological Programme (IBP), Roy Shephard has produced an extremely useful reference volume that bears careful examination by all students of human ecology. The reviewer's task has been greatly simplified because the volume contains a first-rate review of methodology and overall results; I shall concentrate, therefore, on describing the book's contents.

Before presenting the basic data, Shephard devotes two chapters to the all-important matters of "Sampling and population studies," and "Methods for the measurement of physical fitness, working capacity and activity patterns," making it clear that the subjects of sampling and standardization of protocol have received too little attention. Although "overkill" (i.e., large sample size) was recommended at the outset of the project, this was not always possible and many IBP investigators failed to specify the potential sample and the method of sample reduction. Shephard discusses sources of bias for "primitive" and more developed populations and supplies a worthwhile example drawn from his exemplary studies of the Iglolik, along with some suggestions for improving generalizations derived from biased data. Important methodological problems are raised, as the author notes alarming variations in maximum oxygen intake ("the best single measure of man's fitness for endurance-type activities" [p. 39]) of 10 to 25 percent for the same subjects, when measured by different laboratories. The advantages and disadvantages of several ergometers are discussed, with the step test proving superior for measuring centrally limited aerobic power in older people and in people unfamiliar with the bicycle.

The fourth chapter presents a wealth of data concerning the effects of "Climate, season and local geography" on working capacity and demonstrates the superior fitness of circumpolar populations as well as the need for investigators to take into account the sometimes radical seasonal variations in activity patterns that occur in primitive groups.