claim for bimodality in the australopithecine distributions is tenuous and must be viewed as an artifact of data arrangement.

The demonstration of dental metric trends in the later Pleistocene, in which Wolpoff (1971) has played a major role, does not preclude their probability or possibility in the lower Pleistocene or Pliocene. Nowhere, however, are the australopithecine canine-breadth data examined for trends relating to age (time depth) as opposed to (or in addition to) sexual dimorphism. The canine-breadth distribution of Neandertals is offered as an example of a "sample representing wide spans of both space and time" (p. 586); the unimodal distribution observed in this sample supposedly highlights, through contrast, the significance of the "bimodal" australopithecine distribution. It is questionable, however, whether the 400,000-year Neandertal sample is directly comparable to that for the australopithecines, scattered over some 3 million years.

This discussion does not by any means refute Wolpoff's argument concerning the possibility of early hominid sexual dimorphism. On the contrary, consideration of such a model is both useful and quite probably accurate. What it does indicate is that the sexing criterion advanced remains unconvincing.

Reply

by Milford H. Wolpoff

Department of Anthropology, University of Michigan, Ann Arbor, Mich. 48109, U.S.A. 7 xii 77

Scaglion (pp. 153–54) and Anderson (pp. 219–21) suggest that the apparent bimodality of the early hominid canine-breadth distributions is an artifact of the interval chosen and claim that these distributions cannot be shown *not* normal. Both contentions are incorrect.

While the authors feel that the apparent bimodality is a fortuitous consequence of the intervals chosen in the frequency distribution that can be negated by using different intervals, this argument puts the cart before the horse. The frequency distributions were an *illustration* of the distribution characteristics, not a *proof* that these characteristics exist. Obscuring their visibility does not affect their existence, one way or another. An unambiguous illustration, although one more difficult to interpret visually, of apparent bimodality can be seen in the cumulative frequency distribution (see discussion below), since this is interval-independent.

The more difficult question of normality was first approached through use of a chi-square test, as described (contra the comment by Scaglion) on p. 586, line 17. My procedure differs from Scaglion's, but rather than debate the finer points of different chi-square procedures I decided to use an interval-independent test. To this end, I applied to the cumulative frequency distributions a Kolmogorov-Smirnov statistic as modified by Lilliefors (1967) for the specific problem of normality tests when the mean and variance of the sample observations are unknown. The modified test is unusually conservative (the probability of Type 1 error is minimized); significance levels determined by it can be taken as maximum.

The results of this test indicated deviation from normality in the maxilla with an error probability of no more than .05. The probability of error for this test applied to the mandibular canine sample was greater than .20 (the maximum value given in published tables for the modified statistic). These results do not differ from those reported from the chi-square. However, the Kolmogorov-Smirnov statistic is calculated from the maximum *deviation* between expected and observed distributions; these deviations are clearly not due to the marked skew of the male samples (a phenomenon duplicated in male distributions of the African pongids), but rather occur at the points which lie between the two modes (if the sample distribution is considered bimodal), as can be seen in my figures (figs. 3 and 4). The maximum deviations of both occur where there are markedly fewer individuals in the sample distributions than in the model normal distribution; this area in the distribution corresponds to the reduced frequency interval separating male and female subsamples (under the bimodal interpretation). While the marked significance of the maxillary canine sample is not reflected in the mandibular canine distribution, the fact remains that the *position* of maximum deviation from normality is the same.

In sum, I believe that there is no reason to abandon my original contention that the distributions are what they appear to be, a bimodal distribution of canine-breadth due to differences in average and range of males and females.

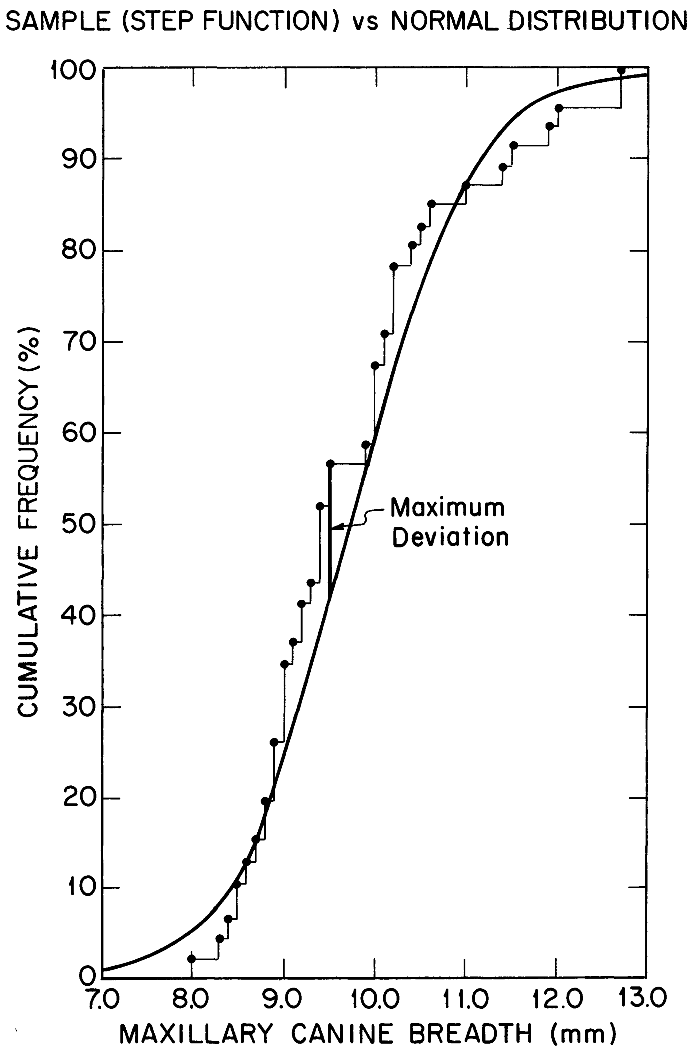

Fig. 3. Cumulative frequency distribution of maxillary canine transverse breadth in early hominids. The corresponding normal distribution is shown. The position of the maximum deviation is indicated.

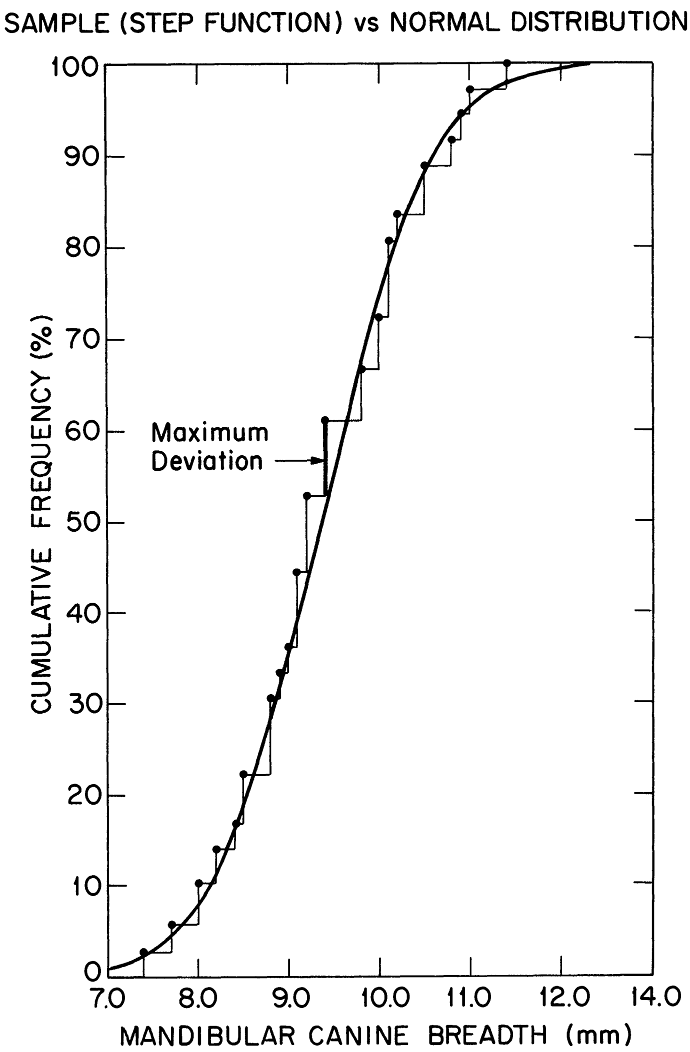

FIG. 4. Data corresponding to figure 1 for the transverse breadths of early hominid mandibular canines.

References Cited

LILLIEFORS, H. W. 1967. On the Kolmogorov-Smirnov test for normality with mean and variance unknown. *Journal of the American Statistical Association* 62:399–402.

WOLPOFF, M. H. 1971. *Metric trends in hominid dental evolution.* Case Western Reserve Studies in Anthropology 2.

More on Views of the Swat Pathans

by AKBAR S. AHMED

13 York Terrace East, London, N.W. 1, England. 24 x 77

Although my essay has been seen as "a tilt at some anthropological potentates" (Singer, *Times Literary Supplement,* May 13, 1977), the object was not to denigrate Barth's work, but to open a debate centering around it. Indeed, my essay may be seen as a complement to his Swat analysis. For obvious reasons, I do not wish to be drawn into what the Pathans call a *ma way–ta way* ("I said–you said") debate, but I will make three general points with reference to Dupree's review of my work and the comments on it (CA 18:514–18):

1. A reiteration of my question as to the validity in a science based on man and the society around him of subordinating ethnography to theory: The masters of a theory—whether "holist/individualist," "substantivist/formalist," etc.—risk becoming its slaves. Intellectual traditions and thought systems tend to go sour and even sterile if confined to water-tight categories. Social ontology and its manifestation—human society—in the 20th century are polychrome, changing, and complex. The fundamental strength of anthropology remains its fieldwork data, allowing reexaminations and reassessments (for instance, I deliberately attempted to confine myself as far as possible to Barth's social data).

2. The importance of defining the "ethnographic present" while at the same time allowing for diachronic analysis: The "ethnographic present," heuristically a useful analytic concept in anthropological monographs, may be part fiction in reality, especially in Third World societies, where social revolution, national disintegration, political dictatorship, discovery of vast natural resources, etc., affect social structure and the quality of life on all levels. For example, the hitherto "closed" systems in the tribal areas of the North-West Frontier Province of Pakistan are undergoing vast structural change, in an anthropological sense, as a result of economic change, as understood in terms of "development economics" (Ahmed 1977). Swat is thus no exception, and indeed Dupree has summarized at least seven distinct stages in its political history over the last 100 years. Here the importance of a direct relationship between theoretical models and empirical data that tell us of societal processes—a point raised by Pettigrew in her comment—is apparent.

3. Fieldwork method and duration: Can one really present, except in an abstract form, a picture of a highly complex foreign social system on the basis of six or nine months' total study? From my own experience, I would hesitate to answer in the affirmative. I have been directly and indirectly working for four years among the Mohmand tribe in Pakistan and am only now coming to grips with the theory and method involved.

Judging from the wide attention and range of reviews my book has drawn, it may have touched a sensitive nerve in the discipline and thus satisfied one of its primary aims: to generate debate on the above issues.

Reference Cited

AHMED, AKBAR S. 1977. *Social and economic change in the tribal areas.* London: Oxford University Press.

Our Readers Write

Moles (CA 18:235–43) re-raises important issues regarding the need to make ethnography more "empirical," replicable, and measurable, but how can he ignore the "etic ethnography" school and its attempt to do just what he asks? Harris's *The Nature of Cultural Things* may be disagreed with, but at least it should be *reckoned* with, as should the work of many of his students. Otherwise we are simply reinventing the wheel with periodic pleas for making our science scientific.

JOHN R. COLE

Hartwick College, Oneonta, N.Y. 13820, U.S.A. 6 VI 77

To encourage young scholars, CA might devote some space to the publication of standard papers on microcosmic studies.

RAMENDRA KUMAR KAR

Department of Anthropology, Dibrugarh University, Pin 786004, Assam, India. 22 x 77

Me gusta y apoyo *la idea de una "CA discussion of French anthropology"* (CA 18:576) y sugiero que se haga sistemática-