Europe. Moreover, it incorporates errors concerning processes of cultural continuity and culture change at that time, as I have argued at length elsewhere (Anderson 1971), for it posits ongoing acculturation between classes when cultural differentiation was the norm.

To conclude on a positive note, I am pleased that Hughes raised again the issue of the extent to which voluntary associations were or were not prominent in the social structure of the Middle Ages. I would ask, however, that she present her findings in detail, so that we can revise our understanding of these matters if such is required, since she offers no basis for revision at this time. Above all, I would ask that she move beyond the question of fact, as fundamentally important as that is, to concern herself with the implication of such facts for theory. Our goal as anthropologists should be to identify and explicate sociocultural process, and not merely to describe. If the peasants and aristocrats of preindustrial states were as highly organized as Hughes believes, then let us consider what this might suggest as concerns the manipulation of power in that type of social system.

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Data and Theory in Paleoanthropological Controversies

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Washburn and Ciochon (*AA* 76:765-784, 1974) raise important issues in their discussion of theories regarding hominid canine evolution. I believe there is good reason not to accept their conclusion that "futile debate comes from the illusion of scientific proof, and from the emotional needs of contesting individuals." There is a deeper and more underlying reason for many of the controversies in paleoanthropology which I suspect is a necessary consequence of the scientific method and the state of the art.

My reason for not accepting Washburn and Ciochon's conclusion comes from a very different understanding of "data," and the relation of data to theory. The discussion of australopithecine canine size is an excellent example of this. The statements about canine size in the various australopithecines are rapidly forming one of the longest-lasting and least-illuminating debates in paleoanthropology.

Why did not Pilbeam, Brace, or Wolpoff use the C/P4 length ratio, the authors ask, when the tables showing this ratio (e.g., "the data") "suggest that there was more than one species"?

Perhaps the ratio was not used because the sample size is too small. The data quoted were all taken from the literature several years ago, and the sample sizes were extremely small. I have now measured all of the relevant specimens, and with larger sample sizes, for instance, six out of the ten gracile mandibles fall within the robust ($n = 11$) range. Skepticism because of small sample size would have been justified, but it was probably not the actual cause, since there is a great tendency to use whatever information is available: little is better than nothing.

Perhaps it was not used because tooth lengths are known to be modified during life by interstitial wear (Wolpoff 1971). Yet, we commonly measure numerous features that are altered during life, and this rarely seems to give paleoanthropologists pause for thought.

Perhaps it was not used because the meaning of the ratio is not clear. What possible importance can the ratio of lengths of two teeth have, occurring in different parts of the jaw and probably not performing the same function. Is the ratio supposed to show the relative size of the canine? If so, why this ratio and not some other? If the area of the canine and the third premolar is used to make a ratio (this is the criterion that Robinson originally suggested), then there is *no significant difference* between the gracile and robust samples (Wolpoff 1974). What makes the first ratio better than the second? And what about all of the other possible ratios? There are enough possible tooth measurements to tax the patience of any reasonable worker, and all their possible combinations would tax the computer budget of most reasonable anthropology departments. And to what end? Are we measuring the size of the canine relative to the back teeth or the size of the back teeth *relative* to the canine? One thing is sure, we are *not* measuring the size of anything relative to the body size, since only two specimens with either front or back teeth (in both cases a canine and the posterior dentition) have associated limbs allowing body size to be estimated. One of these is a gracile and the other a robust (STS 7 and TM 1517), and the canines and back teeth of both have the *same* ratio when compared to estimated body height. Yet, I don't believe that ambiguity over the meaning of such ratios has ever prevented their use.

I believe that the explanation lies in the idea that data can suggest something: the "data speak for themselves" approach. After all, if data can speak, a poor reflection is cast on the workers who can't hear it. As Washburn and Ciochon recognize, there is often little agreement on what constitutes a "fact." But is this the result of "the emotional needs of contesting individuals?"

I, for one, will freely admit that I have never heard paleoanthropological data speak. Data, in my view, cannot exist outside of a theoretical framework, and the relation of data to such a framework lies in their potential power of refutation. No data can really "prove" a theory correct, and there is a valid point to what the authors call the "illusion of proof." If data are to "suggest that there was more than one species," this can only result from the data refuting the hypothesis that there was only one species.

The single species hypothesis, at least as presented by Brace, Mayr, and myself, is based on an ecological contention. Competition between sympatric hominid populations would likely be over limiting resources, and would necessarily lead to extinction or niche divergence in one or both of the populations. Since there is widespread agreement that extinction did not occur in one of the (supposed) taxa for millions of years, evidence of niche divergence would be necessary to refute the hypothesis as we understand it. Canine size, relative to a posterior tooth, is not a refutation for us because we believe that the anterior and posterior teeth respond to different selection pressures: the posterior teeth function in mastication and the anterior teeth function in food preparation and environmental manipulation. The posteriors should therefore be related to mastication (Pilbeam and Gould 1974) but there is no reason to expect the anteriors to be. The relevant data for us is therefore the absolute size of the back teeth relative to body size. A large difference in the size of the back teeth relative to body size could be a refutation, as this could indicate dietary difference. Such a relative difference does not occur (Wolpoff 1974). Variation in the anterior teeth is of less importance, and in any event as the authors admit, there is no significant difference in absolute canine size (and I can add also in absolute incisor size). This is why variation in ratios along the tooth row is not refutatory data.

In Robinson's view (1972), dietary difference is reflected in the anterior teeth. He believes that relatively large anterior teeth bespeak of a meat eating adaptation, by analogy to carnivores. For him, the tooth ratios are relevant because in his view they show niche divergence, and therefore refute the single species hypothesis.

A third framework is purely morphological, without regard to ecological considerations (and one may add insensitive to the temporal limitations imposed by radiometric dating). In this view, variation in the australopithecines is compared with variation in living primate populations (Pilbeam and Zwell 1973). The relatively greater the australopithecine variation, the more probable the likelihood that different taxa are mixed together. In this approach, any observations would suffice. It tends to lead to a "the more the better" viewpoint which ultimately appears in the form of a multivariate analysis. In any event, there are neither refutation nor proofs, but rather only probability statements for which any and all observations are appropriate.

Where this gets us into insoluble controversies is the result of the fact that these approaches do not articulate with each other. That is, they do not agree on what constitutes refuting evidence for the single species hypothesis. Without such agreement, the dialogue is bound to result in "competent scientists with access to the same facts, drawing different conclusions, and then becoming involved in acrimonious debate" (p. 765).

My point is that this is *not* a reflection of the individuals involved or their alleged "false sense of intellectual security" and "emotional needs." Instead, it is a prime characteristic of a scientific field that does not share a universally accepted paradigm (Kuhn 1970). Paleoanthropology is not in a "mopping up" period of "normal science" (*contra* Tuttle 1974) which is supposed to follow a scientific revolution (in this case the development of the synthetic theory of evolution). Instead, the obvious presence of nonarticulating theories and of dialogue in which the same words are used with very different meanings shows rather clearly that paleoanthropology is still in the throes of this revolution. Therefore, controversies such as those discussed by Washburn and Ciochon are a result of the state of the art and not, I suggest, the result of the internal motivations of the scientists.

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The Single Species Hypothesis

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It is always difficult to comment on a comment, especially one which raises so many issues. Our paper was addressed to some very general problems, and not particularly to the single species hypothesis, as one might infer from Wolpoff's remarks. To maintain the one species hypothesis it is useful to: stress competitive exclusion, compare differences one at a time stressing overlapping of each independently, miminize variation, and attack the limitations of those who disagree. Each of these contains a large element of what MacLean (1970) has called "emotional cerebration." Granted that scientists have access to the same theories and information, differences arise primarily from the uncertainties of personal evaluation. The recognition of the importance of the subjective factors runs counter to the values of our culture, and the illusion of science leads to futile controversy and slows progress in understanding human evolution.

Ecology. The single species hypothesis is based largely on ecological considerations. Because of competitive exclusion, two species of hominids could not occupy the same niche (Wolpoff 1971a). This is a useful principle, but its application to the early hominids is not a simple matter. The reasoning may just as easily be reversed—one can say that the presence of two species of hominids shows that at least two ways of life were possible. There are at least three possible points of view.

(1) The early hominids were so behaviorally and adaptively versatile that there never was more than one species.

(2) It was not until after *Homo erectus* had appeared that one species of hominid became so successful that all others were eliminated.

(3) Because of the history of discovery it was reasonable to think of two species, one of which became extinct, and one of which evolved into *Homo erectus*, but more recent discoveries do not fit this model in any simple way. If the lineages of ape and man separated some 8 ± 2 million years ago, and if the early hominids were present in Java (Robinson 1953) and China (Jian 1975), only a very small part of the hominid adaptive radiation has been sampled. The longer the time and the greater the extent of the hominid radiation, the less any simple hypothesis is likely to describe the events.

We are not concerned with trying to establish the probability of any particular theory, but only to point out that ecological considerations do *not prove* any one point of view. In the case of the australopithecines, competitive exclusion involves personal judgment, and there is no one conclusion which everyone must accept.

Data. The australopithecines of South Africa have been regarded as so distinct that they should be placed in separate genera (Robinson 1972), or so similar that they should be considered subspecies (Wolpoff 1974). Wolpoff (1974:137) concludes that the differences "... seem entirely due to differences in body size." But the canines of the large form are smaller than those of the small one, the opposite of what would be expected on the basis of size or sexual differentiation. This was described by Robinson many years ago (1956), stressed again by him in 1972 (1972:224; fig. 109), and is also shown in Wolpoff's own published tables (1971b:123, tables 139, 140). As Robinson on many occasions has clearly stated, it is the pattern of the dentition which distinguishes the two forms, not the measurements of individual teeth.

Given the small size of the samples and the fragmentary nature of most of the