scopic examination of the animals has not revealed any visible gas bubble. (ii) Upon evacuation of a flask of amphipods in sea water, any gas contained in a flexible organ might be expected to greatly expand and thus increase the buoyancy of the animals, but no change in buoyancy was observed on evacuation. (iii) After the animals were maintained at several atmospheres (about 4000 mb) for 10 minutes, they showed an activity response to pressure increases of as little as 30 mb above the 4000-mb level, even though one might expect a marked reduction in the size of a contained gas bubble, due to both compression and solution of the gas in the animal's tissue fluids, with probably an accompanying larger loss in sensitivity to pressure changes than that observed.

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References and Notes

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3. One millibar equals 1000 dy/cm² and is approximately equivalent to the pressure exerted by a column of seawater 1 cm high. One atmosphere equals 1.0132 bars.
4. E. W. Knight-Jones and S. Z. Qasim, *Nature* **175**, 941 (1955).
5. E. R. Baylor and F. E. Smith, "Recent advances in invertebrate physiology," *Univ. Oregon Publs. (1957)*, p. 31.
6. This work was performed during the tenure of a National Science Foundation predoctoral fellowship. Experimental work was supported by National Science Foundation grant No. G-7141 to Dr. E. W. Fager. I wish to express my sincere thanks to my scientific adviser, Dr. E. W. Fager, for much helpful discussion and encouragement with the work. Thanks are also due to Richard Ford and Nancy Enright for their time as observers and operators in the experimental work, to Frank Snodgrass and James Snodgrass for help with instrumentation, to Dr. Robert Arthur for a helpful discussion of methods, and to a large number of other friends for valuable comments and discussions.
7. This small oedicerotid amphipod, which is abundant on the intertidal beach at Scripps Institution of Oceanography, La Jolla, California, was identified by Dr. Clarence Shoemaker (deceased) and will be formally described by Dr. Thomas E. Bowman in a forthcoming monograph on the genus. It differs from two other undescribed species of the genus found in the La Jolla area in the following conspicuous morphological characters: the first and second antennae of the mature male are roughly 80 and 95 percent, respectively, of the length of the cephalothorax; those of the female are roughly 45 percent each; comparable values for the other two species are about 50 and 220 percent for the male and 35 percent each for the female. The second and third uropods, in both sexes, are roughly 65 and 35 percent, respectively, of the length of the first uropod, compared with 80 and 65 percent for the other two species. The bulk of the adult population of this species is in the uprush zone of the beach at La Jolla; of the other two species, which are much less abundant, one ranges from low intertidal to depths of about 3 m, and the second is usually taken deeper than 5 m.
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12 October 1960

Primate Taxonomy and Oreopithecus

W. L. Straus, Jr., and M. A. Schön have reported their findings on the cranial capacity of *Oreopithecus bambolii* (*1*). The abstract from this report reads: "From a plaster reconstruction of the skull of the August 1958 skeleton, the cranial capacity of *Oreopithecus bambolii* has been estimated as falling between 276 and 529 cubic centimeters, thus within the ranges of variation of both orangutan and chimpanzee. In cranial capacity, therefore, and probably in body-brain ratio as well, *Oreopithecus is a hominoid*" (italics mine).

The authors explain that after Hürzeler's recent revaluation it seems evident that *Oreopithecus* is a member of the superfamily Hominoidea, which comprises the families Pongidae (anthropoid apes) and Hominidae (man and his immediate forerunners).

Three letters appeared last year in *Man* (*2, 3*). Their titles clearly indicate the worry of anthropologists about the thorny problem of the taxonomy of our probable or possible ancestors.

Before *Oreopithecus* "revalued"—as Huxley would most likely say—came into the picture, Simpson's (*4*) creation of the superfamily Hominoidea appeared convenient, and thus the term has been widely used. We had "hominoid," "hominid," and "pongid" equated, as the adjectival forms of the zoological groups Hominoidea, Hominidae, and Pongidae. As Wells (*2*) has commented, "This nomenclature commits us to the view that man is more directly linked in origin with the anthropoid apes than with the other Old World primates (Cercopithecoidea). Although this conception is both plausible and widely held, Straus (1949) (*5*) has argued persuasively for the alternative view that the Hominidae arose from stem Catarrhines too primitive to be classified as anthropoid apes even in the broadest sense. On this view, the Catarrhina (*sensu* Hemprich, 1820) would form a natural unit with three co-ordinate subdivisions: Cercopithecidae, Pongidae and Hominidae; *the Hominoidea of Simpson would then be an arbitrary grouping. Nevertheless, at this stage anyone who proposes to use the term 'hominoid' in any other sense than that adopted by Le Gros Clark* (*6*) *must define its meaning very carefully*" (italics mine).

But thus far, although several ancestors of the Pongidae had been discovered corresponding to pre-Pleistocene chronology, hominids going beyond Pleistocene dating were unknown. Le Gros Clark could say in 1955 (*7*), "Similarly, the fossil Hominoidea of Miocene age may appropriately be called 'primitive anthropoid apes,' even though they had not acquired all the specialized features which are accepted as characteristics of the anthropoid apes of today."

That is, the use of the term hominoid "in any other sense than that adopted by Le Gros Clark" (*2*) seems to me, in the light of the recent *Oreopithecus* discoveries, as somehow unfitting. One may ask, Is this Upper Miocene primate a "hominoid" or a "primitive anthropoid ape," considering that he is classed under the superfamily Hominoidea? Then would *Proconsul* also be a "hominoid," and *Limnopithecus* another?

I believe that, as has been stated by several authors, while it was appropriate—in spite of Straus (*5*)—to create a superfamily which would encompass the two families Pongidae and Hominidae, it was unwise to give it a name which refers exclusively to one of them, and to that carrying a deeper emotional charge.

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17 October 1960

Dr. Santiago Genovés objects to the use, by myself and M. A. Schön (*1*), of G. G. Simpson's term, Hominoidea (and its adjectival and substantival derivative, "hominoid") to denote a superfamily comprising the families Pongidae (anthropoid apes) and Hominidae (man and his immediate forerunners). Consequently, he disapproves of our classification of *Oreopithecus* as a hominoid. In this, he is tilting with a windmill.

My paper of 1949 (*2*) has been cited by Genovés in his argument against use of the terms Hominoidea and hominoid. The implication is that my present acceptance of Simpson's definition of a superfamily Hominoidea contravenes my earlier views respecting man's ancestry, for this superfamily associates the anthropoid apes and man, with the Old World monkeys excluded as a separate catarrhine superfamily,

the Cercopithecoidea. It is true that I was not entirely happy about Simpson's classification of the catarrhine Primates when it appeared in 1945 (*3*). Simpson believed that "the usual diagnostic characters" justified the union of man and anthropoid apes in a single family; although in actual practice he placed them in separate families, Hominidae and Pongidae. Hence his superfamily Hominoidea seemed to denote an unreasonably late time of divergence of the hominid and pongid evolutionary lines. Since this feeling still persisted when I wrote my 1949 paper, I did not follow Simpson's taxonomy. Subsequently, however, this objection has come to appear unimportant, although I still believe in a relatively early separation of the hominid and pongid branches. Thus I now can see no good reason not to follow Simpson's classification. On the whole, its excellence cannot be denied. Moreover, since it has gained general acceptance, to use it is to avoid possible misunderstanding. I differ from it in one detail respecting the Hominoidea, however, in that I believe it is logical to recognize two families of anthropoid apes, Pongidae and Hylobatidae, rather than include all apes in a single family, the Pongidae. Yet this is perhaps largely a matter of personal taste. It does not alter the general validity of Simpson's taxonomy.

In any event, I still adhere to my views expressed in 1949, that the hominid and anthropoid-ape evolutionary lines separated, at a relatively early date, from a *"common ancestral stock"* (italics mine) which was "far more monkey-like than anthropoid-like." This means, moreover (and this perhaps is the major issue), that the Hominidae did not arise from animals which had undergone the specializations accompanying adaptation to brachiation but, rather, from animals which were "essentially unspecialized, monkey-like quadrupeds" and hence not "actual" anthropoid apes. But I did not then, any more than now, deny a common origin to the Hominidae and the anthropoid apes exclusive of the cercopithecoid monkeys. Indeed, I explained many of the resemblances between man and the great apes as the results of parallel evolution. This implies, as Genovés should know, inheritance of common genetic potentialities from common ancestors. A common ancestry for hominids and pongids (to the exclusion of the cercopithecoids) also is clearly expressed in the "family tree" which I then proposed (Fig. 8B) and to which I still adhere (except that I am less inclined than I was 11 years ago to branch off the hominid line before that of the gibbons). Thus, *pace* Wells and Genovés, there really is nothing in my views of 1949 against combination of the families Hominidae and Pongidae, together with their immediate common ancestors, in a superfamily, the Hominoidea. The sole possibility of my disagreeing with other workers, beyond the brachiation question, relates to the point of divergence of the Hominidae from the ancestral hominoid stock. This, however, necessarily remains a matter of opinion because the requisite fossil evidence is lacking.

Of the three letters in *Man* to which Genovés refers, only that of Wells (1959) is truly pertinent to the question which he raises. Moreover, by omitting the opening sentence of the paragraph cited from Wells' paper, he has failed to make it clear that Le Gros Clark's use of the term "hominoid" derives from Simpson's Hominoidea. Even if one accepts Clark's 1955 diagnosis of all the then known Miocene Hominoidea as "primitive anthropoid apes," it does not necessarily follow—as Genovés naively assumes—that *Oreopithecus* (which was but poorly known at that time) automatically falls into that category. To label *Oreopithecus* (which, incidentally, is generally regarded as Lower Pliocene in age; not Upper Miocene, as Genovés states) a "hominoid" is merely to state superfamily assignment. It does not involve allocation to any particular family of the Hominoidea. One does not imply that the skunk, *Mephitis,* is a dog by including it within the superfamily Canoidea. The same sort of thing applies when "hominoid" is attached to both *Proconsul* and *Limnopithecus.*

Although Genovés apparently is resigned to recognition of "a superfamily which would encompass the two families Pongidae and Hominidae," he protests against giving it *"a name which refers exclusively to one of them"* (italics mine). In this he exhibits innocence of established zoological taxonomic procedure. A noteworthy example of similar taxonomic artlessness has been cited by Simpson (*4*), who wrote: "Dart's placing of † *Australopithecus* in a family 'Homo-simiadae' (1925) only served to exemplify the total ignorance of zoology so common among the special students of these higher primates (although, of course, Dart's work is excellent in his own field)." The name of a higher category, whether it be that of a superfamily, family, or subfamily, is derived from that of *one* valid genus. In consequence, it must be "exclusive," since it cannot be *compounded* from the names of lower categories. Although a superfamily name is derived from that of a genus, not from that of a family, in common use its root is the same as that of *one* of its valid families. Consequently, one may ask, would Pongoidea (or Hylobatoidea) carry a less profound "emotional charge" than Hominoidea? Or, if one could employ the name of any available valid genus, would Gorilloidea, Panoidea, Symphalangoidea, Dryopithecoidea, or the like, be any more dispassionate?

I can think of only one possible solution which might satisfy those whose emotions are unduly aroused by the term Hominoidea. Since it now appears that the dentition of the Oligocene catarrhine primate, *Propliopithecus,* is more generalized than was originally thought, so that its classification as an actual pongid (or hylobatid) may well be questioned, this genus could be regarded as the type of the family, Propliopithecidae, which gave rise to the Pongidae (and Hylobatidae) and Hominidae. Accordingly, Propliopithecoidea could supplant Hominoidea as the superfamily name embracing all of these families. I seriously doubt, however, that this suggestion is likely to gain anything like general acceptance.

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27 December 1960

Facultative Heterotrophy in Some Chlorococcacean Algae

Abstract. All known species of the genera *Bracteacoccus, Spongiochloris,* and *Dictyochloris,* and some of the species of *Neochloris* and *Spongiococcum* are capable of growing heterotrophically in darkness in a glucose-salts medium. In contrast, all known species of *Chlorococcum* are obligate photoautotrophs. Possible relationships between these results and certain morphological characteristics are discussed.

Since the studies of Bristol-Roach (*1*), numerous investigations have dealt with facultative heterotrophy (that is, organotrophy) in algae. Especially significant in this area are the works of Petersen (*2*), Saunders (*3*), von Ernst and O. Pringsheim (*4*), Lewin and Lewin (*5*), and Belcher and Miller (*6*). Recently the family Chlo-