The Pleistocene Epoch and the Evolution of Man[1]

by Cesare Emiliani

THE CALABRIAN STAGE was defined by Gignoux (1913) as the last stage of the Pliocene Epoch, characterized in the Mediterranean by the presence of *Arctica (Cyprina) islandica* and a dozen marine mollusks previously restricted to northern waters (North Atlantic, North Sea, and Baltic Sea). *Arctica islandica* and the associated mollusks belong to a rather shallow facies (less than 150 m., according to Ruggieri 1965) and are supposed to have entered the Mediterranean following a general temperature decrease. In deeper-water facies, the climatic deterioration is evidenced by the sudden and widespread appearance in abundance of the northern benthonic foraminiferal species *Hyalinea (Anomalina) baltica* (Schroeter). Although this foraminiferal species and the molluscan species *Arctica islandica* are generally not found together because of their different habitats, Ruggieri (1961, 1965) established that *Arctica islandica* entered the Mediterranean somewhat earlier than *Hyalinea baltica* and distinguished a Lower Calabrian characterized by the occurrence of the former and the absence of the latter.

The 18th International Geological Congress in London in 1948 removed the Calabrian stage from the Pliocene and redefined it as the first stage of the Pleistocene. Later, the General Assembly of the 7th INQUA Congress in Denver, Colorado in 1965 unanimously decided that the Plio-Pleistocene boundary (base of the Calabrian stage) be placed at the time when *Hyalinea baltica* (Schroeter) first appeared in the continuous Late Cenozoic marine section at Le Castella, Calabria, Italy. This section has been described by Emiliani *et al.* (1961), and the regional stratigraphy is discussed by Roda (1964). According to the decision of the INQUA Congress, the Lower Calabrian of Ruggieri should be placed in the Late Pliocene.

The age of the Plio-Pleistocene boundary is unknown at present. The long-accepted equivalence of the marine Calabrian with the continental Villafranchian should be considered questionable until substantiated by absolute dating of the marine Plio-Pleistocene boundary. An age of more than 3.3 million years for the beginning of the Villafranchian is indicated by K^{40}/Ar^{40} dating of French deposits (Curtis 1965; Obradovich 1965). The Lower Villafranchian may be the equivalent, at least in part, of the Lower Calabrian of Ruggieri; and the later Villafranchian may be the equivalent, at least in part, of the Calabrian stage.

Cooling of the earth's climate, indicated by the appearance of *Arctica islandica, Hyalinea baltica*, and the other species of northern invertebrates in the continuous Late Cenozoic sections of Italy, is but the culmination of the long trend of decreasing temperatures during the Tertiary demonstrated by both palaeontological and geochemical analysis (Emiliani 1954, 1961*a*, and references therein). The over-all temperature decrease in the high latitudes was about 12° C. during the past 75,000,000 years (Emiliani 1961*a*). The cause of this decrease appears to have been an increase in earth albedo related to mountain-building and increasing continentality (Emiliani and Geiss 1959; Emiliani 1961*b*). The observed temperature decrease was probably interrupted by numerous pauses and even significant reversals. However, the rapid temperature changes which occurred during the Pleistocene were shown, by oxygen isotopic analysis of deep-sea cores, to have been absent during the Oligocene and Miocene (Emiliani 1956*a*).

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The present paper, submitted 29 IX 66, was sent for CA☆ treatment to 52 scholars, of whom the following responded with written comments: Emmanuel Anati, C. L. Brace, Karl W. Butzer, H. B. S. Cooke, C. S. Coon, Malcolm F. Farmer, John E. Frisch, Alexander Gallus, M. Gigout, R. Dale Givens, Roger T. Grange, Jr., James J. Hester, Ralph L. Holloway, Jr., W. W. Howells, Kenneth A. R. Kennedy, J. Kukla, G. Kurth, Gabriel W. Lasker, John M. Longyear, III, M. A. MacConaill, Charles A. Reed, Karl H. Schwerin, Günter Smolla, and L. Van Valen. Their comments are printed in full after the author's text, and are followed by a reply from the author.

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[1] Contribution No. 777 from the Institute of Marine Science, University of Miami.

Glaciation eventually began. It seems logical to speculate that the first land masses to become glaciated were those occupying high latitudes and adjacent to oceanic sources of water vapor. This means, of course, Antarctica and Greenland, which probably became glaciated several million years ago (i.e., during the Pliocene) and have remained so ever since (Emiliani 1957). Evidence for cool or cold episodes at lower latitudes during the Pliocene has been accumulating recently. Thus, marked low-temperature episodes were observed by $0^{18}/0^{16}$ analysis of pelagic foraminifera below the Plio-Pleistocene boundary in the type section at Le Castella, Calabria, southern Italy (Emiliani *et al.* 1961); by pollen analysis of layers below the Pliocene Amphistegina limestone at Castellarquato, northern Italy (Lona 1963); and by $0^{18}/0^{16}$ analysis of a specimen of *Arctica islandica* collected below the Amphistegina limestone at Castellanselmo, Pisa, Tuscany (Emiliani and Ruggieri, unpublished). These temperature changes, as well as the ones occurring during the Pleistocene, are clearly shown in the Mediterranean (and probably elsewhere at similar or higher latitudes) by variations of planktonic foraminiferal assemblages at both the specific and subspecific levels (Emiliani *et al.* 1961). The temperature oscillations of the Late Cenozoic have such high frequencies (about 6.7×10^{-13} cps) that stratigraphic work of great detail must be carried out in order to obtain a clear picture of climatic change. Sampling intervals of less than a foot are generally necessary. The "mixed" cold and warm assemblages observed by Woodring *et al.* (1946) probably resulted from inadequate stratigraphic sampling (cf. Emiliani *et al.* 1961:687).

No especially strong temperature decrease was noticed across the Plio-Pleistocene boundary at the type locality (Emiliani *et al.* 1961). Oxygen isotopic analysis of the section above the boundary, 45 m. thick, showed temperature oscillations of an amplitude which, although considerable, was smaller than that of the temperature oscillations (of later Pleistocene age) detected by oxygen isotopic analysis of pelagic Foraminifera from the eastern Mediterranean core 189 (Emiliani 1955*a*). It is possible that these oscillations were caused by the growth and waning of relatively small ice caps in Scandinavia, the Alps, Labrador, and possibly elsewhere.

While the section at Le Castella terminates about 45 m. above the Plio-Pleistocene boundary, other sections in Italy, as in restricted areas of the Emilia-Marche region, extend much farther upwards with apparently continuous sedimentation. However, the somewhat abnormal and probably variable oxygen isotopic composition in the upper Adriatic Sea would make results obtained by oxygen isotopic measurements uncertain. Similar effects are to be expected in the Plio-Pleistocene sections of the Netherlands. Thus, in order to obtain a complete record of the Pleistocene, it seems necessary to bore down from the floor of the open ocean to a layer equivalent to the Plio-Pleistocene boundary at Le Castella. This layer may or may not have been reached yet, depending upon the actual absolute age of the Plio-Pleistocene boundary.

Oxygen isotopic analysis of deep-sea cores from the Caribbean and the equatorial Atlantic (Emiliani 1955*b*, 1964*a*, 1966*a*; Rosholt *et al.* 1961, 1962) revealed the occurrence of eight major temperature cycles during the past 425,000 years. With the single exception of core stage 3, the amplitude is approximately the same for all cycles (1.6-1.8‰, equivalent to about 6-7° C. after the correction discussed in Emiliani 1955*b*:543). The temperature cycles are believed to be the equivalent of the carbonate cycles noticed in a suite of deep-sea cores from the eastern equatorial Pacific (Arrhenius 1952). These cycles are especially evident in some of the Pacific cores (nos. 59, 60, and 61), but less evident in others (nos. 58 and 62). The latter cores are stratigraphically longer and include sediments containing *Pterocanium prismatium* and *Discoaster brouweri.* The age of the bottom of the stratigraphically longest core, no. 58, is estimated at about 1,100,000 years, and the extinctions of *D. brouweri* and *P. prismatium* are estimated to have occurred about 900,000 and 840,000 years ago respectively (Emiliani 1967). A strong carbonate cycle appears near the bottom of core 58. The total number of cycles occurring through core 58 (i.e., during the past 1,000,000 years) probably numbered 20.

Arrhenius (1952) observed that the amplitude of the carbonate oscillations in the upper half of core 58 is greater than that of the oscillations in the lower half (with the exception of the strong oscillation near the bottom mentioned above) and suggested that the oscillations in the upper half correlate with the major glaciations of the Pleistocene.

Safe correlations between the oxygen isotopic oscillations in the Atlantic and Caribbean cores and the carbonate oscillations in the Pacific cores, on one side, and the glacial-interglacial stages of the continents, on the other, must necessarily rest with absolute dating. C-14 dating of both continental materials and deep-sea core carbonates has shown incontrovertibly that core stage 1 is the postglacial; stage 2 the Main Würm or Main Wisconsin; and stage 3 the Laufen of the Alps and the Port Talbot-Plum Point of North America. In addition, stage 4 appears to correlate with the Early Würm of Europe and Early Wisconsin of North America (Andersen *et al.* 1960; Goldthwait *et al.* 1965).

Earlier glacial deposits of the continents have not yet been dated with sufficient accuracy to warrant a correlation with the oceanic stages. For instance, deposits from the Laacher-See region of Germany, tentatively correlated with phases of the Günz, Mindel, and Mindel/Riss stages of the Alps, have been dated as ranging from 420,000 to 110,000 years ago (Frechen and Lippolt 1965). The limits of error of the individual measurements, however, are as large as, or larger than, the wave length of the climatic oscillations (Frechen and Lippolt 1965, Tables 3-5), making it impossible to correlate unequivocally the deposits in question with the established oceanic stratigraphy. On the other hand, the marine-epicontinental interglacial deposits of Alaska, belonging to the Worozonfian, Pelukian, Kotzebuan and Middletonian stages, dated by the Th^{230}/U^{238} method (Blanchard 1963; Hopkins *et al.* 1965; Karlstrom 1965), afford a close correlation with the oceanic stages 3, 5, 7, and 9, respectively (see Figure 1).

The evidence obtained from the deep-sea cores together with theoretical considerations (Emiliani and Geiss 1959) strongly suggests that the glacial-interglacial cycles observed in the recent past are likely to continue in the future for an indeterminate length of time. Thus, the name "postglacial," commonly used for the time since the last deglaciation (i.e., the past 11,000 years), appears to be very inappropriate. The name "Holocene," also commonly used for the same time span, appears equally inappropriate in view of the numerous extinctions of animal species during the past 15,000 years, coupled with, and partly caused by, the explosion of *Homo sapiens sapiens* (cf. Martin 1965). The name "Flandrian" (Dubois 1938), conforming to classical stratigraphic procedure and denoting the time since the beginning of the last glacial-eustatic marine transgression (the past 18,000 years), would appear to be a much better choice.

The Pleistocene, with a likely duration of millions of years (past and future), bears some resemblance to the last period of the Palaeozoic Era, the Permian, from the point of view of high continentality, orogenesis, glaciation, and animal extinctions. By analogy, the Pleistocene may be considered the last epoch of the Cenozoic Era. This Era may be more conveniently divided into a Palaeogene Period (Palaeocene to Oligocene) and a Neogene Period (Miocene to Pleistocene) (cf. Moore 1958) than into a Tertiary Period (Palaeocene to Pliocene) and a Quaternary Period (Pleistocene and Recent) (cf. Kummel 1961). Thus, the names "Tertiary," "Quaternary," "Holocene," "Postglacial," and "Recent" should be used informally only. These formal observations are important because some of the confusion existing in Pleistocene literature appears to result from semantics alone.

The climatic history of the Late Cenozoic has a direct bearing on the problem of human evolution. Unfortunately, there is an extreme dearth of absolute ages, beyond the range of C^{14}, for evidence directly related to the various phases of human evolution. K^{40}/Ar^{40} dating of Olduvai Gorge Bed I at 1,750,000 years ago shows that man (defined as a primate capable of systematically manufacturing tools) was already in existence at that time. (It will not come as a great surprise if it should appear, when the Plio-Pleistocene boundary has been dated, that the genus *Homo* dates from well back into the Pliocene.) In addition, Pa^{231}/Th^{230} dating of the temperature maximum of the last interglacial (Eemian) at about 95,000 years ago indicates a similar age for the speciation of *Homo sapiens sapiens*, *if* the Fontéchevade remains are assigned to this subspecies (rather than to a "praesapiens" type) and *if* they indeed come from a layer identifiable with the Eemian maximum. On the other hand, if the Fontéchevade remains are assigned, together with Swanscombe, to a "praesapiens" type, *H. sapiens sapiens* would appear to have speciated between 100,000 and 50,000 years ago (cf. Emiliani 1964*b*).

In spite of the present chronological uncertainties, the sequence *Australopithecus robustus–A. africanus–Homo erectus habilis–H. erectus erectus–H. erectus pekinensis–H. sapiens "praesapiens"–H. sapiens neanderthalensis–H. sapiens sapiens* appears to form a rather orderly temporal and morphological succession suggesting a phylogenetic lineage. According to modern views on speciation and evolution, however, none of the large, fully developed populations belonging to the above-mentioned taxa can have given rise *directly* to the next one. Rather, we must assume that from each large, polytypic stock, a small population became separated, evolved independently, and spread to compete with, and eventually replace, the original stock.

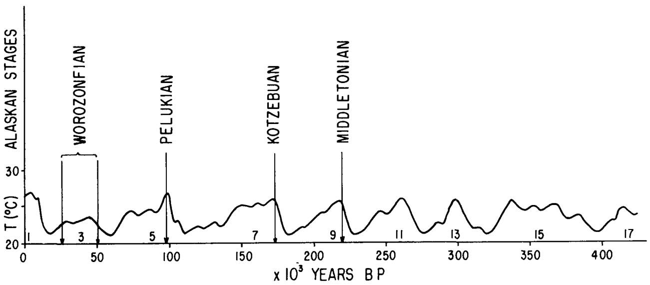

FIG. 1. Generalized palaeotemperature curve, obtained from deep-sea cores, for the surface water of the Caribbean-equatorial Atlantic region, referred to an absolute time scale based in part on C^{14} and Pa^{231}/Th^{230} measurements and in part on extrapolation. Absolute ages of the Alaskan interglacial stages obtained by Th^{230}/U^{238} measurements.

The pre-*Australopithecus* evolutionary step which led to bipedal posture is likely to have been selective because of the ensuing improvement in perception and communication, a marked advantage for an animal conspicuously lacking physical strength and specialized mechanisms for offense and defense. (In other animals, an increase in the length of the neck has served the same purpose.) Following the acquisition of bipedal posture, *Australopithecus africanus* may have evolved from an early stock of *A. robustus* by developing a taste for meat and by discovering that only through the imaginative usage of tools and the employment of sophisticated strategies could other animals, including *A. robustus* and his own peers within *A. africanus*, be persuaded to provide a meal. Indeed, cannibalism may have played a very important role during this and later stages of human evolution.

The cranial capacity of *H. erectus* is such that he should have had little difficulty in making for himself a comfortable living under ordinary conditions. Unfortunately, *H. erectus* was not living under ordinary conditions, but under the severe environmental stress of major glaciations. As mentioned before, as many as 20 glaciations may have occurred during the past 1,000,000 years. Populations of *H. erectus*, which had spread from eastern Africa to Morocco, Asia as far as China, and Europe, were *temporarily* and *repeatedly* separated from each other by newly created ecological barriers. The European and western Asiatic populations, in particular, found themselves in a very difficult situation, sandwiched between the great ice caps to the north and a cold, hostile sea to the west and the south, and isolated by secondary ice caps in the Pyrenees, the Alps, Carpathians, Apennines, Balkans, Caucasus, etc. The palaeontological and palaeobotanical record, together with oxygen isotopic analysis of pelagic foraminifera from the eastern Mediterranean deep-sea core 189 (Emiliani 1955*a*) and of littoral marine shells (food refuse) from the Haua Fteah and Arene Candide caves (Emiliani *et al.* 1964) indicate that the European summers during the glacial ages were even colder than the European winters of today, and suggest that the European winters must have been very dreadful indeed. It appears, therefore, that, during the past 1,000,000 years, Europe and western Asia functioned as a typical evolutionary mill, manufacturing the *"praesapiens," sapiens,* and *neanderthalensis* subspecies from original, scattered populations of *Homo erectus.* Indeed, only through a capable brain and consummate skills could man survive the harsh winters of the glacial ages, for surviving a single glacial night must have been a very difficult task. While the fossil finds of *Australopithecus* and *Homo erectus* point to Africa as the cradle of the early Homininae, the finds of such "praesapiens" or early *sapiens* types as Steinheim, Swanscombe, Ehringsdorf, and Fontéchevade point to Europe as the cradle of modern man.

The extinction of the highly successful Neanderthals about 30-40,000 years ago remains a mystery. Some authors (e.g., Brace 1964*b*) maintain that the classic Neanderthals evolved directly into *H. sapiens sapiens,* but this seems to be in conflict with modern views on speciation and evolution. If we assume that the Neanderthals were as intelligent as modern man or more so, then we must conclude that their demise resulted from factors other than those connected with the brain. The Neanderthals appear to have been particularly subject to deforming arthritis and perhaps, because of their larger sinus cavities, also to colds, flu, and other diseases associated with cold weather. If so, modern man replaced them more because of a physiological advantage than by warfare or other means.

Comments

by Emmanuel Anati☆

Jerusalem, Israel. 21 II 67

After reading the appealing title of this paper, I was disappointed to find that the text deals primarily with geological dating, and therefore the title is not fully appropriate. I shall confine my comments to some of the views expressed concerning the "evolution of Man."

If I interpret the author correctly, by "evolution" he means "changes in average cranial capacity." This is certainly not the only aspect of the physical evolution of man. Furthermore, other aspects of human evolution and development, such as cultural and social change, are not considered in the present paper.

While I accept the author's claim that some of the current terms for periods are creating misunderstandings among scholars, it is my belief that rather than drastically changing the terminology—an unrealistic proposition—anthropologists should reach an agreement on the definitions of terms (a task that might be undertaken by CA).

Like history, prehistory is currently looked at from the particular angle of the period in which *we* are living. Furthermore, both ecologically and culturally, the end of the last glaciacion marked a turning point in human evolution. Therefore, in my opinion, the term "postglacial" for what came thereafter is fully justified.

Nobody will disagree with Emiliani when he states that "... climatic history ... has a direct bearing on the problem of human evolution"; but climatic change should be considered as one of many causes, and probably not even the main one, as man appears to have evolved in widely different climatic conditions, from tropical to sub-arctic.

The presently available data on the hypothetical "cannibalism" of Pleistocene men are totally inadequate to justify the statement that "cannibalism may have played a very important role during this and the later stages of human evolution." I trust that some other colleague will qualify this assertion in detail. I will comment only that even should the scanty evidence on cannibalism turn out to have been interpreted correctly, we have enough "uncooked and unchewed" skeletal material today to affirm that anthropophagy has never been, in any age or in any prehistoric culture, a common practice.

The statement that "... the finds of such "praesapiens" or early *sapiens* types ... point to Europe as the cradle of the modern man" is very daring indeed. The statement will probably be received with reservations by colleagues who are specialists in this field. The discoveries of skeletal material in Palestine and elsewhere in Asia and Africa do not seem to agree with this view, and it is probable that future discoveries on the Asian continent will prove it to be completely wrong.

The statement that "the cranial capacity of *H. erectus* is such that he should have had little difficulty in making for himself a comfortable living under ordinary conditions" follows a rather simplistic and evolutionistic-minded line of thought which I have some difficulty in following.

The last paragraph illustrates several misconceptions: "If we assume that the Neanderthals were as intelligent as modern man or more so ...." It is high time to discard the legend that intelligence depends on cranial capacity. It would be like stating that the power of an engine depends on its size. The theory that Neanderthals may have disappeared as consequence of their tendency to catch colds or flu, interesting as it may be, is in the present state of knowledge no more than science fiction. These and other misconceptions and misuses of terms make of the author's quest for more semantic rigor a rhetorical commonplace.

Wherever it is accompanied by a logical and scientific understanding of the available data, there is nothing against a dry, detached, narrowly technical approach to the study of early man. On the other hand, our science would not suffer from a broadminded humanistic approach. Let us not forget that the history of human evolution *is our own history.*

by C. L. Brace☆

Santa Barbara, Calif., U.S.A. 17 III 67

Emiliani's paper consists of two distinct parts, and these appear to be only slightly related to each other. The first part deals with the techniques of Pleistocene dating and climate evaluation as well as with the conclusions that can be drawn at present. Much of the work has been done by Emiliani himself, as his recently published survey indicates (Emiliani 1966*b*), and represents a solid and well-documented contribution of great value.

The second part of the paper involves speculations on the course of hominid evolution, and, while these are of great interest to scientists in general and anthropologists in particular, they remain simply speculations. Emiliani offers virtually no documentation in support of these speculations. Some of his statements would be arguable even if supporting evidence were cited; in the absence of references they should be regarded more as the guesses of an interested amateur.

The suggested temporal sequence of *robustus-africanus-erectus* is confounded by the facts: where remains have been dated, it appears that the sequence is *africanus-robustus-erectus* (assuming that the specific designations are valid) (Robinson 1965*a*; Campbell 1965). The logic suggested for interpreting the morphological succession should be much more extensively documented in view of the disagreement which currently exists (Robinson 1963*b*, 1965*a*; Brace 1963*a*:90-91; 1963*b*:547; 1967). The argument that *africanus* evolved from *robustus*, actually based on Robinson's dietary hypothesis (Robinson 1961, 1962, 1963*a*), has been questioned on the basis of both fact and theory (Brace 1963*a*:90-91; 1967; Brace and Montagu 1965: 225-28).

These are details, however, and, although they are not unimportant, the issues relating to Emiliani's reference (repeated) to "modern views on speciation and evolution" are of much greater significance. A definition of "modern views" should have been included, and an extended discussion should have followed. Presumably Emiliani refers to the position of Vallois (1959:155) and perhaps that of Mayr (1963*a*, *b*), but it should be noted that opposition has been offered which is no less modern (Brown and Wilson 1954; Brown 1958; Whyte 1965; Brace 1966).

To be more specific, why *must* we believe that no fossil population as a whole can have given rise to the next one directly? Why *must* we assume that evolution only occurred when a small population became isolated? Why, indeed, *must* we assume that fossil hominid populations were polytypic? Presumably the idea that fossil hominids belonged to polytypic species is derived from the assumption that modern humans do (cf. Dobzhansky 1955:182, 333; 1963:353-54); but even this has been questioned (Brown 1958; Livingstone 1962; Brace 1964*a*, *c*), and there is certainly no justification for projecting questionable assumptions into the unknown past. The small, isolated population model is necessary only if genetic drift is considered to be the main force producing evolutionary change (Wright 1931:150; 1943, 1946, 1948; Cavalli-Sforza, Barrai, and Edwards 1964), but it is widely agreed that natural selection is the principal evolutionary force (Dobzhansky 1962: 128-61), and this can operate unhindered in the total absence of isolation.

This brings up the final major assumption that can be questioned. Why must we assume that hominids of the *erectus* species (I prefer to use the designation "Pithecanthropine stage") were temporarily and repeatedly separated from each other long enough for major differentiation to take place? Human beings are neither foraminifera nor mollusks, and consequently the ecological barriers that serve as isolating mechanisms must be very different from those which dictate the habitation areas of those invertebrates. Certainly the claim that "newly created ecological barriers" served to cut off human populations from others deserves some documentation. The distribution of Lower Palaeolithic artifacts suggests that continuous cultural exchange and not isolation characterized human existence, and where there was cultural exchange, there must have been genetic exchange as well. It is possible to suggest that the wide-ranging foraging activities of hominid groups from the Australopithecines on would have prevented any kind of isolation of a major or protracted sort. If man has specialized in anything, it has been in adaptability. Even during the Lower Palaeolithic, only major oceans and cold temperatures limited human distribution, and since the archaeological record provides us with a picture of continuous human occupation of those parts of the Old World south of the cold-temperate and arctic areas, it is difficult to imagine how isolation could have taken place. To be sure, the northern boundary of human occupation shifted with the climatic fluctuations which Emiliani documents, but this cannot have affected human continuity to the south.

Even during the last glaciation, the assumption that the Neanderthals of western Europe were isolated (Howell 1951, 1952, 1957; most recently defended by Howells 1967:243) is hard to maintain. Why, for instance, if isolation and speciation did not take place in either the mammoth or the mouse (or any other mammal) should it have done so in man? Human cultural adaptation should have been effective enough to prevent isolation, and it is significant that Mousterian tools show greater similarities in form and evident function in an area running from western Europe through the Middle East and from southern Russia all the way around the Mediterranean shores (north and south) than the tools in this whole area do with their contemporaries in sub-Saharan Africa, India, and points east (Golomshtok 1938; McBurney 1958, 1960; Clark 1959; Bordes 1961; Allchin 1963; and Solecki 1963). Finally, the similarities in form between La Chapelle-aux-Saints, Monte Circeo (Sergi 1948), Petralona (Kokkoros and Kanellis 1961; Kanellis and Savas 1964), Shanidar (Stewart 1958, 1961), and Jebel Irhoud (Ennouchi 1962) are so great that this alone would make claims for isolation most unlikely.

"The Pleistocene epoch and the evolution of man" is a very significant subject. Emiliani has treated the Pleistocene epoch with great professional competence. It is to be regretted that he has not done the same for the evolution of man.

by Karl W. Butzer☆

Chicago, Ill., U.S.A. 15 III 67

Emiliani's paper presents an interesting outline of Pleistocene chronology and related problems, a presentation that should stimulate considerable discussion. My only general criticism is that, in view of the many controversial topics touched upon, the paper is excessively brief and incompletely documented. This commentary will be confined to certain aspects of the Pleistocene stratigraphy discussed by Emiliani.

Although Emiliani would accept the first appearance of *Hyalinea (Anomalina) baltica* in the Calabrian marine sequence at Le Castella as the Plio-Pleistocene boundary, this motion was never placed before the General Assembly of the 7th INQUA Congress (nor, for that matter, does it represent a recommendation of the Plio-Pleistocene Boundary Commission of INQUA [G. M. Richmond, personal communication]). Before such a specific redefinition could, in fact, be attempted (and before continental or littoral lithostratigraphic, mammalian, and palynological sequences are relegated to second place in matters of Pleistocene stratigraphy), an internally consistent radiometric framework—employing different isotopes (cf. Broecker 1965) and substantiated by paleomagnetic correlations (cf. Cox *et al.* 1965)—must be obtained for a greater number of potentially suitable type localities. Only then can it be decided whether continental or marine sequences should best be given preference, or whether a boundary definition based on climatic change be indeed more practical than one based on organic evolution or conventional biostratigraphic zones. The first appearance of erect bipedal hominids is by no means irrelevant for a consideration of such criteria.

Many of the problems of inference and interpretation in the early Pleistocene stem from the lengthy twilight zone of incoherent and controversial mid-Pleistocene stratigraphy. Emiliani either ignores or glosses over two of the rather critical, dated sequences available for this time range, and it seems essential to evaluate these in order to demonstrate the present state both of our knowledge and of our ignorance.

The first such sequence involves the alluvial terraces of the lower and middle Rhine Valley, which are interdigitated with the remarkably complete Pleistocene continental stratigraphy of the Netherlands. Frechen and Lippolt (1965; also Frechen and Boom 1959), as I read them, provide a suite of consistent and apparently sound K/Ar dates for these Rhine terraces: (*a*) upper terminus of the *ältere Hauptterrasse* ("older Main Terrace") *ca.* 420,000-390,000 years (5 dates); (*b*) upper beds of the *jüngere Hauptterrasse* ("younger Main Terrace" *ca.* 350,000-340,000 years (4 dates); (*c*) basal beds of *ältere Mittelterrasse* ("older Middle Terrace") *ca.* 300,000 years (1 date); (*d*) basal beds of *mittlere Mittelterrasse* ("middle Middle Terrace") *ca.* 220,000 years (1 date); and (*e*) early downcutting phase following deposition of the *mittlere Mittelterrasse*, *ca.* 140,000-150,000 years (2 dates). This entire sequence predates the *jüngere Mittelterrasse* ("younger Middle Terrace"), which directly underlies the moraines of the Drenthe stadial of the Saale glaciation (De Jong 1956; Paas 1961; Kempf 1966). Since German and Netherlands workers now generally ascribe the "older" and "middle" Middle Terraces to stadia of the Elster Glacial, the Holstein Interglacial, as defined in northern Germany and the Netherlands, appears to have begun a little before 150,000 K/Ar years ago. However, Zagwijn (1963) gives substantial reasons why the upper units of the "younger" Main Terrace must be correlated with the Weert Zone of the uppermost Sterksel Formation, i.e., with an early phase of the Elster Glacial. Thus the Elster complex, as defined in a critical area, appears to date from a little before *ca.* 350,000 to a little before 150,000 K/Ar years ago. It might be remarked in passing that these particular isotopic dates are no less consistent or theoretically justified than the rather more scanty Pa/Th dates available from the Caribbean deep-sea core A254-BR-C (see Rosholt *et al.* 1962; Emiliani 1964a).

On a variety of subtropical and tropical coasts transgressive beach deposits, with related molluscan faunas, have been correlated with non-glacial intervals of the later Pleistocene, while regressional eolianites are attributed to periods of low, glacial-age sea level. With the recent advance of Th/U dating, a second chronology of sorts has been developed for the high, non-glacial sea-level stages (cf. Stearns and Thurber 1965; Thurber *et al.* 1965; Broecker and Thurber 1965; Richards and Thurber 1966). One of the more instructive stratigraphic sequences of this kind has been described from the Balearic Islands by Butzer and Cuerda (1962*a*, *b*) and partly dated by Th/U on Mallorca as well as on the Spanish mainland and Morocco (Stearns and Thurber 1965; also unpublished). A glacial complex of at least two regressional phases precedes the Tyrrhenian I transgression (with several dates of "greater than" 200,000 or 300,000 years). A second glacial complex of four regressional phases ("Penultimate Regression") precedes the Tyrrhenian II, a complex regression generally characterized by *Strombus bubonius* and other Senegalese mollusks in the western Mediterranean Basin. Although the Th/U dates for the Tyrrhenian II are not all consistent, they suggest one time unit of *ca.* 165,000-120,000 years ago, with a number of earlier, and possibly significant, dates falling between 230,000 and 190,000 years. It is curious that eolianites did not develop, except on a very localized scale, during the seemingly minor regression between the Tyrrhenian II and III. The latter stage, with equivalent beaches in the Caribbean and Pacific areas, would date *ca.* 90,000-70,000 Th/U years.

Comparing the Rhine Valley and Balearic Island sequences, it appears that the Tyrrhenian I is not, as once widely assumed, equivalent to the Holstein Interglacial. Instead, on present evidence, it would seem to be contemporary with the Cromerian, thus setting the "Penultimate Regression" of Mallorca parallel to the Elster Glacial and the Tyrrhenian II to the Holstein Interglacial. The potential implications of such a stratigraphic revision for mammalian and archaeological correlations between North Africa, Spain, and continental Europe are difficult to overview at the moment. Assuming that Th/U and C^{14} dating are temporally consistent, the Tyrrhenian III would appear to be correlated with the Eem. But this immediately raises another problem: why the Saale Glacial, occupying the interval between the Tyrrhenian II and III, should have left virtually no eolianite record in the Mediterranean region. Equally disturbing is the assignment of this, the greatest continental glaciation of Europe, with its complex of several major stadials, to a time span of only 30,000 years, i.e., *ca.* 120,000-90,000 Th/U years (D. L. Thurber, personal communication) or *ca.* 130,000-100,000 Pa/Th years (see Rosholt *et al.* 1962; Emiliani 1964*a*).

If we now attempt to compare these two sequences with the deep sea core stratigraphy of Emiliani (1964*a*, 1966; Rosholt *et al.* 1961, 1962; cf. also Ericson *et al.* 1964), we seem to reach an impasse beyond the cold maximum of about 115,000 B.P. If the generalized paleotemperature curve of Emiliani proves to be correct in detail, the "interstadials" of the Middle Pleistocene, as they have been called by glacial geomorphologists, compare in wavelength but not in amplitude with the mid-Würm interstadial. This would render specific marine-continental correlations difficult for decades to come. One might speculate that Emiliani's four cold maxima between *ca.* 330,000 and 170,000 (extrapolated) Pa/Th years ago represent the Elster complex, or one might propose any of a dozen other possible correlations. The status and nomenclature proposed for the earlier Pleistocene cold intervals would, accordingly, be liable to almost arbitrary interpretation. In view of the inadequacy of the best available radiometric sequences to bridge the stratigraphic chaos of the Middle Pleistocene, I feel quite strongly that correlations of deep-sea paleotemperature curves with continental or littoral stratigraphies are premature for the early-to-middle-Pleistocene time range.

Two final, brief comments to the late Pleistocene chronology: It is disturbing that Emiliani's cold peak of *ca.* 60,000 Pa/Th years ago broadly coincides with the complex of interstadial-type zones known as Broerup, Amersfoort, Rodebaek, as defined by C^{14}. Are there in fact systematic inconsistencies between our radiometric yardsticks? The poor fit of the one C^{14}-crossdated Pa/Th date in the A 254-BR-C core (Rosholt *et al.* 1962) seems to suggest this, as does the abovementioned time lag between Th/U and Pa/Th in the late Middle Pelistocene.

Although there is still no suitable name for the mid-Würm interstadial complex ("Paudorf"? "Arcy"?), Emiliani's revival of the term Laufen is undesirable. Originally the Laufen-Schwankung was thought to represent an Alpine interstadial, but in 1922 Penck relegated all allied phenomena to the Riss/Würm (Eem) Interglacial. Despite the possible validity of the weathering horizon developed on the Laufenschotter at Hörmating as mid-Würm, use of the name would be a source of confusion.

by H. B. S. COOKE☆

Halifax, Canada. 18 III 67

The decision in 1948 by the 18th International Geological Congress to define the base of the Pleistocene as the bottom of the Calabrian also included a statement that this boundary should be placed "at the horizon of the first indication of climatic deterioration." The subsequent decision by the 1965 INQUA Congress to place the boundary at the first appearance of the foraminifer *Hyalinea (Anomalina) baltica* in the type section at Le Castella has the merit of precision, but the appearance of this foraminifer is not the first indication of climatic deterioration. Barbieri and Medioli (1964), for example, have shown that the indigenous foraminifera displayed fluctuations suggestive of Pliocene cooling long before the arrival of *Hyalinea baltica* and its close associate, the mollusk *Arctica (Cyprina) islandica*. The former favours a moderately deep-water muddy environment and the latter a shallow-water sandy habitat, so lithology must be borne in mind in interpreting the presence or the absence of either form; indeed, local factors may control the order of first appearance at different localities. There is evidence that *Arctica islandica* appeared before *Hyalinea baltica*, and this suggests that climatic deterioration was quite severe at a time before the Plio-Pleistocene boundary as defined in 1965.

As these two "critical" elements of the marine fauna normally inhabit northern latitudes, it is clear that their occurrence in the Mediterranean is the result of cold water spilling in through the Straits of Gibraltar. *Arctica islandica* and *Hyalinea baltica* would not be likely to arrive at exactly the same time and, furthermore, their spread eastwards and northwards through the Mediterranean would depend to some extent on the different mechanisms and rates of dispersal of the respective embryonic forms. Despite these variables, the spread would be rapid enough for the first appearance of these forms to constitute a reference point probably more accurate than any of the other climatic indicators. Nevertheless, colder northern waters must have been moving down the west coast of Europe for some time before it was possible for the invasion of the Mediterranean to take place, and it is presumably this pre-Calabrian cooling that is evidenced in the indigenous microfauna of the Mediterranean. In short, although the definition of the Plio-Pleistocene boundary at Le Castella provides a valuable time line for the Mediterranean marine sequences, it does not render very much easier the task of defining this boundary, even in marine sequences, in other critical areas such as the Netherlands and East Anglia. Presumably in these areas the climatic deterioration would have been manifest long before the cold waters moved as far south as Gibraltar. Furthermore, the terrestrial deposits which constitute the "Villafranchian" are not equivalent to this precisely defined Calabrian, and the Villafranchian undoubtedly extends back into the Pliocene of this definition (see, for example, Azzaroli 1964). How far back into the Pliocene the Villafranchian must go is uncertain, but one might surmise that *Elephas meridionalis* is at least as old as the base of the Calabrian and that the older deposits with *Anancus arvernensis* and *Elephas planifrons* would be Pliocene, although still "lower Villafranchian."

These observations might seem pointless except insofar as they help to resolve to some extent the apparent conflict between the K/Ar dates for terrestrial deposits and the time scale from oceanic cores. The important mammal-bearing deposits of East Africa seem to be faunistically Villafranchian, yet the K/Ar date of 1.75×10^6 years for Olduvai Bed I seems great in relation to the glacial/interglacial sequence in Europe and the core data. The mammals in the Kaiso Beds of Uganda seem to be at least as old as those of Olduvai Bed I (Cooke and Coryndon, unpublished; but see Bishop 1965) and the "Earlier Kaiso" assemblage parallels some of the "Lower Villafranchian" of sites in the Maghreb of North Africa. The lower Kaiso beds pass downwards into relatively thin passage beds and then into the Kisegi Beds. The molluscan faunas have been studied by Gautier (1965, 1966, 1967), who is puzzled by the fact that the characteristic mid-Miocene Kisegi fauna passes up smoothly into that of the lower Kaiso beds without any suggestion of a major hiatus, and yet the mammalian evidence has pointed to the Kaiso as "Villafranchian." There does not seem to be much place for the Pliocene, and Gautier "feels that probably the Pliocene has suffered amputation at both its boundaries in sub-Saharan Africa." Coryndon and I have been forced by a study of the mammals to reach a similar conclusion and to suggest that the early Kaiso fauna must range down into the Pliocene. This provides support for the suggestion by Emiliani that "the genus *Homo* dates from well back into the Pliocene." Furthermore, as Africa has been a source of indigenous development from which mammals emigrated to Eurasia, time lines are difficult to determine in Africa in relation to the European deposits and their faunas. It would seem that the correlation Calabrian = Villafranchian has been misleading and there still exists a great deal of confusion in defining the boundaries in the terrestrial deposits and in relating them to the marine sequence. A reevaluation of the nomenclature for the mammal-bearing sediments is clearly necessary.

by C. S. COON☆

Gloucester, Mass., U.S.A. 7 III 67

Emiliani's account of the history of official attemps to define the Plio-Pleistocene boundary is detailed, lucid, and useful. His discussion of the role of marine invertebrate fauna in such dating is instructive. It surprises me a little to see Fontéchevade, Swanscombe, and Steinheim dated at about 95,000 years, much younger than geologist's previous figures.

The only part of this wellwritten paper on which I venture to go into details is that in which he relies on other people's conclusions. Here, if he is occasionally confused or even wrong, he has only fallen into a wide pit already inhabited, at one time or another, by almost everyone who has aimed at broad interdisciplinary coverage.

He dates *Australopithecus robustus* first, *Australopithecus africanus* second, and "*Homo erectus habilis*" third in sequence, but where is his evidence? He postulates scattered populations of *Homo erectus* in Europe before the appearance of *Homo sapiens*, but no such animal has yet been found there. Vertesszölös II, with a cranial capacity reckoned at 1556 cc., is called by Thoma (1966) *Homo (erectus seu sapiens) palaeohungaricus*, and on size alone it is fully *sapiens*.

Emiliani uses the term *Homo sapiens sapiens* to designate all races of living men, although the term has priority as Linnaeus' trinominal for living Europeans. He further states that the presence of "... Steinheim, Swanscombe, Ehringdorf, and Fontéchevade point to Europe as the cradle of modern man." If he had said "the most ancient cradle of modern man," he might have been closer to the truth.

by MALCOLM F. FARMER☆

Whittier, Calif., U.S.A. 2 III 67

With the rapid accumulation of knowledge of the geological history of coastal regions and continental shelves, and of marine deposits in these regions and from the bottom of ocean basins as well as deposits from the formerly glaciated areas, it is becoming increasingly clear that revision of concepts concerning the Pleistocene is in order. Emiliani's paper is a very welcome contribution, offering a report on progress and some guidance for the future.

The concepts of glacial and interglacial appear to be under revision. The earth has not been free of ice since the Pliocene, although the amounts and areas of ice accumulation have varied. Emiliani's suggestion of a resemblance between the Pleistocene and the Permian offers perspective on the Pleistocene. The traditional view of that epoch reflects a kind of ethnocentrism which regards the present as a unique period, a time of climax, the time of the great modern man, rather than just a point in a sequence. This has needed revision for a long time.

Emiliani suggests that during the Pleistocene, and perhaps earlier, populations became separated and isolated and then in periods of "better" climate had an opportunity to be rejoined (hence the polytypic stock). Such populations were never separated long enough to have become so sharply differentiated as to prevent hybridization. Culture and the nature of learned behavior may have contributed to this situation in that behavioral pattern changes occured instead of anatomical changes, and differences in learned behavior patterns were not insurmountable as anatomical or physiological differences might have been.

The possibility that *Australopithecus africanus* served as a meal for *A. robustus* is one on which studies of primate behavior may throw some light.

With regard to the centers of development of the varieties of man during the Pleistocene, there may be four, perhaps five: eastern Africa, western Eurasia, northeastern central Eurasia, southeastern Eurasia, and perhaps west Africa. The southern margins of Eurasia and Africa appear as primary areas for early primates with Africa, western Eurasia, northeastern central Eurasia, and southeastern Eurasia all playing a part in the development of the primate, man.

Emiliani suggests that physiological and anatomical disadvantages, particularly those which made it difficult to live in cold conditions, were involved in the extinction of Neanderthals. Invasion by people from outside the Neanderthal areas appears to have been part of the situation. Both superiority of cultural achievement and differences in standards of physical attractiveness may have been involved. The new peoples may also have been more prolific. Perhaps the displacement of the Australian aborigines by Western Europeans may be a model for the Neanderthal situation.

by John E. Frisch☆

Tokyo, Japan. 14 III 67

Emiliani's article is a very useful summary of the many contributions for which he is known to all students of paleoanthropology. It helps particularly to understand on what points these students may sometimes disagree with him. Several such points arise in the second half of the article, where he attempts to apply his dating method to human evolution:

1) It is assumed that man can be "defined as a primate capable of systematically manufacturing tools." To my knowledge, no other living animal form has ever been defined taxonomically by its behavior. Moreover, it should not be overlooked that the word "systematically," crucial for the proposed definition, is as likely to refer to a continuous variable such as, let us say, brain size, but is less susceptible to exact measurement than the latter trait. I have recently attempted a morphological definition of the genus *Homo* (Frisch 1965).

2) Inferring the capacity of making a comfortable living (under ordinary conditions) from the cranial capacity of *Homo erectus* would seem to imply an understanding of the connection between brain size and technological skill which I wish very much we had.

3) When the author says "European summers during the glacial ages were even colder than the European winters of today," one wonders what part of Europe he refers to. Does the statement hold both for the surroundings of Brno in central Europe and for those of the Guattari cave on Italy's Mediterranean shore? Can it be stated of the latter environment that "surviving a single glacial night must have been a very difficult task"?

4) Many palaeoanthropologists might feel unhappy at seeing the Steinheim, Swanscombe, Ehringsdorf, and Fontéchevade remains lumped together as "praesapiens" or "early *sapiens*" types.

The above questions are meant merely to stress how much prudence is called for in using the valuable data offered here by Emiliani for a better understanding of human evolution.

by Alexander Gallus☆

Melbourne, Australia. 13 III 67

Biological evolutionary theory seems to have influenced scientific thinking in other disciplines only in a simplified form. Emiliani's paper is a case in point. His views on human evolution can be reduced to a few simple postulates:

1) *Homo* evolved in a single phylogenetic lineage, each "taxon" represented by a single polytypic "stock" which succeeded the one preceding it in a "rather orderly" way.

Comment: The case has been argued convincingly for at least two synchronic, even sympatric hominids of at least species rank in the Early and Middle Pleistocene (Tobias 1965, 1966; Patterson 1965:42-43; Leakey 1965: 11, 106; Robinson 1963*a*:394; 1965*b*: 49). An ancestral *Australopithecus robustus* before *A. africanus* reverses relative dating (Tobias 1966), and there seems to be no proof that the "taxon" *Homo erectus pekinensis* is ancestral to *Homo sapiens* "praesapiens." The phylogenetic succession from *H. sapiens neanderthalensis* to *H. sapiens sapiens* is still *very controversial* (Brace 1964b; Pradel 1966). It seems premature to construct a phylogenetic lineage of this sort until difficulties are fully settled.

2) Species formation affecting *Homo erectus* and leading to *H. sapiens* proceeded through isolation following climatic changes after *H. erectus* had spread to many parts of the globe. One small population evolved independently in isolation and spread to replace the original stock.

Comment: Species formation is by no means as simple as that. The factors involved are intricate and many (Huxley 1942). Some of the most important problem complexes should be mentioned here:

a) *long-range trends,* like the development of the central nervous system in man, which requires constancy of some factor or factors and which occur in spite of change of environment (Huxley 1942; Decker 1963; see also "canalisation," Fraser 1961).

b) *constant factors influencing selection pressure,* of which the prevalence of aggressive competition within or between human species and "culture" acting as a novel and constant environment are examples (Huxley 1942:128-30; Dobzhansky 1960; Breitinger 1961; Robinson 1965*b*:33). "Culture" is seen by Dobzhansky (1960:117) as a new factor in evolution, arising with *Homo:*

> The pressure of natural selection has always been exerted towards the maintenance and improvement of those genotypes which confer on their possessors the capacity to acquire, to use and to transmit culture.

This new type of evolution is influenced by conscious choice or decision ("freedom"); functional adaptability has much increased through conscious choice. Similar arguments have been put forward by Decker (1963), Strong (1953), Spuhler (1960), Debetz (1961), Kortlandt (1962), Hewes (1961), Weinert (1938:4-5, 29, 58-61), Gallus (1953:57-61), Wasburn (1965:101), and Rendel (1959).

c) *polymorphy and clines* within *Homo sapiens* (Huxley 1942:208-9, 354), which presuppose non-interruption of interbreeding.

d) *foetalisation* (Huxley 1942:527-32).

3) Man had spread previous and became isolated during an Ice Age.

Comment: It is impossible at present to be dogmatic about the chronology of human migrations. In fact this question is in urgent need of attention and clarification.

4) *Australopithecus africanus* "may have" evolved by beginning to eat meat and by discovering the "imaginative use of tools" and hunting strategy.

Comment: This question has been argued in detail by Robinson (1963) and Hall (1963), the latter denying the critical significance of tool-use for the transition from ape to human form. I would like here only to put in a plea for a deeper penetration of the subject than Emiliani's. "To discover" means either "to detect" or "to invent." To "detect" tool use is out of the question, for something can be detected only if it is pre-existent; thus the meaning of "to discover" is here "to invent," and the real problem is whether or not cultural innovation depending on abstract thinking (i.e., "invention" or "choice" of a certain new type of adaptive behavior) was an important factor in human evolution (Ascher 1965:48, Dobzhansky 1965: 64).

Emiliani's treatment of the relative and absolute chronology of climatic events is lucid, but his comment's on human evolution are unsatisfactory in that they are based on a simplified version of evolutionary theory. It is necessary to take into consideration the effects of human behavior on adaptation and selection pressure and thus on evolution (De Vore 1965). It is further necessary to take into consideration all possible biological factors which have been analysed by evolutionists. Human evolution is surely complex, and one cannot even assume that the variables involved remained the same throughout the Pleistocene. Finally, I question whether "isolation as a result of climatic change" was really a dominant factor of species formation in *Homo erectus* and *H. sapiens* times, rather than one which had only a modifying or disturbing effect on other, much more important, factors in the evolution of Man.

by M. GIGOUT☆

Orléans, France. 11 III 67

Emiliani will forgive me for emphasizing only the points on which I am not completely in agreement with him. It is difficult for me to follow him when he identifies some 20 glaciations in the last million years. Of course, glaciations and pluvials are subdivided by interstadials with warmer climates; but the interlocking of glacial and pluvial formations and of littorals of glacio-eustatic origin certainly seems to limit the major climatic episodes to five or six (Gigout 1965; Choubert, Joly, Gigout, Marcais, Margat, and Raynal 1956; Fisk and McFarlan 1955; and Bernard, Leblanc, and Major 1962). Even if we count the subdivisions of glaciations, we arrive at only a dozen stages (Venzo 1964).

The equivalence Würm = Wisconsin cannot be considered as established. The Wisconsin is often double, and some authors attach it to the Riss + Würm group (Richmond 1965).

For my part, I will defend the uniqueness of the Quaternary. It is well-known that it is not comparable in duration to other eras; but it has its own particular methods of study, different from those of the Tertiary, e.g., morphology, stratigraphy, prehistory, palaeopedology, etc. This is what makes a crossroads of the Quaternary. There are Quaternarists; there are no "Tertiarists."

by R. DALE GIVENS☆

San Antonio, Texas, U.S.A. 8 III 67

It is gratifying to know that geologists and palaeontologists are moving closer to an adequate chronolgy of the Pleistocene and before. It is also gratifying to find support for the "we are still in the Pleistocene" idea; a number of us have long felt this to be the case. However, a number of questionable interpretations appear in the latter half of Emiliani's article. The statement that it would not be surprising to find the genus *Homo* dating from well back into the Pliocene is presumably based on the Olduvai Gorge material which Leakey calls *Homo habilis*. Designating this form as *Homo*, however, seems completely unjustified on present evidence, since the characteristics reported to date fall within the range of variations we can expect for *Australopithecus* and certainly closer to the latter than to any definite *Homo* —*erectus* or any other. Thus it is very unlikely that this genus will ever be dated back into the Pliocene.

Classifying Fontéchevade and Swanscombe either as a *sapiens* subspecies or as "praesapiens" is unwarranted at present, especially since it implies ancestry to the later *H. sapiens sapiens* to the exclusion of other types. It seems more reasonable to look at them and Steinheim, etc., as belonging to an early and variable group of *H. sapiens* that were ancestral in general to later types.

Emiliani's suggested sequence of fossil types in a phylogenetic lineage would also be strengthened if subspecies were ignored. And again we are presented with two equations—*A. robustus* = vegetarian and *A. africanus* = carnivore (or at least omnivore) —for which there is not as yet adequate evidence.

by ROGER T. GRANGE, JR.☆

Tampa, Fla., U.S.A. 16 III 67

CA review articles frequently offer to students a striking illustration of the complexities which usually underlie the seemingly simple textbook statement. They can also be stimulating for the professional scholar. Emiliani's summary of the problems of pleistocene dating is just such a review.

The latter part of the article is sure to produce considerable confusion in its use of such classifications as *Homo erectus habilis*. This seems particularly objectionable in view of the fact that even the term *Homo habilis* is open to serious question (LeGros Clark 1967: 116).

The hypothesis Emiliani advances concerning the morphological sequence of fossil men is an interesting one despite the disagreements which are sure to arise over his terminology. The expert discussion this hypothesis will certainly evoke should produce a valuable review of the issues, and this, after all, is the way we advance our knowledge.

by JAMES J. HESTER☆

Bethesda, Md., U.S.A. 11 III 67

This paper by Emiliani represents, in my opinion, an outstanding contribution to the study of human ecology during the Pleistocene. He is definitely on the right course in his attempt to correlate environmental changes with the evolution of man. The fact that the Pleistocene was a period of extreme climatic stress undoubtedly served to isolate populations and to affect in general their evolution in the manner he outlines. However, when we proceed to specific details of this problem we lack much critical data. Of special importance to understanding the fate of the Neanderthals, for example, would be the collection of pollen and other types of environmental data from their occupation sites or from temporally correlated continental deposits. Also of importance would be more detailed studies of the cultural means by which the Neanderthals coped with the climatic stresses of the Pleistocene. Such studies might support Emiliani's inferences as to the nature of the European continental climate based on the deep-sea core data or, on the other hand, might bring about some revision of these inferences.

I agree with the methods that Emiliani has employed in reconstructing continental climate from pelagic foraminifera and littoral marine shells.

Further, I believe that the climatic shifts during the Pleistocene were regular and worldwide and that it should therefore be feasible to make climatic generalizations. The only problem I perceive is that his reconstruction is of a climate so severe that it must have provided an extreme test of human survival capability. Before we can accept this reconstruction, we must seek supporting evdience from the human occupation sites in question.

by RALPH L. HOLLOWAY, JR.☆

New York, N.Y., U.S.A. 1 III 67

I will limit my remarks to the palaeoanthropological portions of this paper, because here a number of confusions concerning taxonomy have led Emiliani to speculate unduly under the guise of modern knowledge about speciation and evolution. I think that much of the speculation and the interpretation of deep-sea cores for Pleistocene environments follows from his uncritical acceptance of certain taxonomic rubrics. The K^{40}/A^{40} date of 1.75×10^6 years at Olduvai Gorge, Bed I, relates to the Australopithecine group. I fail to see the logic behind his statement that it will come as no great surprise to find the genus *Homo* "well back into the Pliocene." Emiliani accepts without question a number of dubious sequences and taxonomic labels for the early hominid fragments. "*Homo habilis,*" at least those reported for Bed I, may not be significantly different from *Australopithecus africanus,* in spite of the proclamations based on such low-valence attributes as cranial capacities, tooth dimensions, or various post-cranial fragments. Similarly, the sequence of *A. robustus, A. africanus,* etc., is based on a sort of ecological logic and not on empirical evidence that allows only a single interpretation. The South African evidence clearly reverses the sequence, and Olduvai Gorge can be interpreted in the same manner if one assumes that "pre-Zinj" is actually *A. africanus.*

I find myself intrigued by the suggestion that the "pre-*Australopithecus* evolutionary step which led to bipedal posture is likely to have been selective . . ." (are not all "steps" by definition adaptive?); but bipedal posture can hardly in itself have led to improved perception or communication, and one wonders where the evidence is that "in other animals, an increase in the length of the neck has served the same purpose." Similarly, the acquisition of meat-eating by the *africanus* taxon is seen as a reason for *africanus*' evolving from *robustus* groups. Next, cannibalism is offered as an important dynamic in the development of the imaginative use of tools and strategies for getting a good hominid meal; but no attempt is made to weigh the possibilities of obtaining enough protein without preying on other hominids: e.g., scavenging, cooperative hunting; hunting of immature animals, etc.

Were *Homo erectus* groups "under the severe environmental stress of major glaciations" in Java, North Africa, East Africa, and China? And is there any evidence for Emiliani's assertion for that European summers were colder than present European winters? The evidence from deep-sea cores, at least from the Caribbean and Equatorial Africa, suggests, according to Emiliani, an amplitude of 6-7° C. Does this tally with the above claim? Can the flow of colder waters from melting ice masses and their effects on foraminifera tell us about the rigors of continental life in areas where *Homo erectus* and subsequent finds existed?

The latter part of Emiliani's paper suggests to me a reversion to certain environmental theories (e.g., Huntington's) about the evolution of the brain, behavior, cold weather, and modern man. I gather from Emiliani's comments that only in Europe, with the rigors of glaciers, do the "capable brain and consummate skills" develop which lead to such "clear-cut early *sapiens*" types as Steinheim, Swanscombe, etc. "Europe as the cradle of modern man" seems like an unfortunate way to characterize the complexities of hominid evolution. According to Emiliani's logic, the rigors of European summers and drastic winters should have led to better-adapted groups than arthritic, sinusitis-suffering, flu-ridden Neanderthals. We know so little about *populations* of hominids that it is pointless to claim that one group had a particular sensitivity to arthritis or any other malady. Neanderthals apparently adapted to a wide number of ecological conditions (Europe, Middle East, Far East, East and South Africa) and were replaced (?) or evolved into *sapiens sapiens* types in many places.

I am frankly mystified about the purpose of this paper. Is it to offer a number of speculations about the dynamics of early hominid evolution, or to provide evidence regarding the environments of the early hominids, or to show that deep-sea cores are of value in talking about the other two questions? As far as I can tell, Emiliani has succeeded only with respect to the first question. The taxonomic distinctions he uses are particularly open to criticism, and I find no convincing arguments to support his speculations.

by W. W. HOWELLS☆

Cambridge, Mass., U.S.A. 20 III 67

To one outside the field, the first part of this paper is an informative treatment of the dating of marine deposits and their correlation with late Pleistocene events; what seems especially interesting is the uniformity in amplitude, and to an extent in duration, of the temperature cycles. Emiliani is duly conservative about absolute dates for the earlier glacial phases; I wish he might go in for more discussion of the results of other dating methods and their mutual reconciliation.

I do not see the obvious connection of the second part of the paper with the first. Here Emiliani is decidedly less cautious than before, occasionally landing in a terminological swamp: we have a subspecies *(Homo sapiens sapiens)* "speciating" and subspecies of *sapiens* being manufactured from *Homo erectus.* It seems likely that the anthropologists, while deriding older blunders in nomenclature may be getting into new difficulties by being overspecific in trying to name in a systematically correct way, what are really very small samples of fossil hominids. None of this actually hides Emiliani's substantive suggestion—which I think is probably right—that successive forms of hominids arose primarily from some fraction of the preceding form, not from its totality.

by KENNETH A. R. KENNEDY☆

Ithaca, N.Y., U.S.A. 14 III 67

In his endeavor to relate the climatic history of the Pliocene and Pleistocene epochs to hominid evolution, Emiliani puts forward two ideas suggested a century ago when the case for early man was argued from the evidence of a few fossils from sites in France, Belgium, and Germany: (1) modern-type man *(Homo sapiens)* evolved in Europe out of a more primitive human stock which had its origins in Africa; (2) Neanderthal Man, an early occupant of the European cradle of modern man, met extinction with the debut of sapient humanity.

With regard to the first idea, I doubt that the European fossil record is sufficiently well understood to support the contention that

> While the fossil finds of *Australopithecus* and *Homo erectus* point to Africa as the cradle of the early Homininae, the finds of such "prae-sapiens" or early *sapiens* types as Steinheim, Swanscombe, Ehringsdorf, and Fontéchevade point to Europe as the cradle of modern man.

Granted that the antiquity of European fossil hominids is somewhat more certain than the ages of human remains in other parts of the Old World, the fact remains that the bony traces of *Homo sapiens* have been recovered from Pleistocene deposits in Siberia, China, Borneo, the Sudan, and South Africa. Does this theory assume that all non-European or non-Western Asiatic *sapiens* form fossil hominids are the vestiges of a Pleistocene dispersal out of the northwest corner of the Old World?

In the days when it was still acceptable to refer to Neanderthal Man as representative of a species other than *sapiens,* the problem of his extinction was interpreted in terms of superior power in warfare and intellect on the part of the Cro-Magnon successors. Emiliani's suggestion that the Neanderthals' demise might be related to physiological disadvantages, particularly osteoarthritic and respiratory maladies, is provocative. But if the Neanderthals were cold-adapted in their periglacial milieu, a climatic situation that they appear to have borne through several millenia, it seems curious that their disappearance is explicable by a sudden ineffectiveness of such a favor-

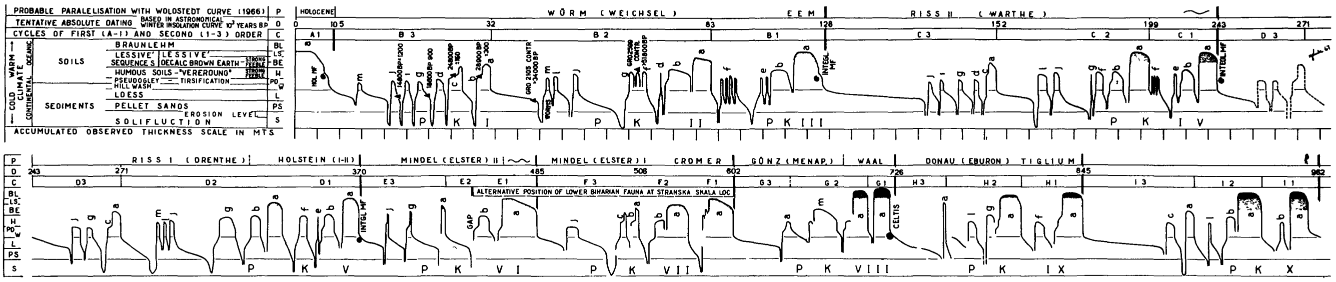

FIG. 1. Loess-section curve of the Prague and Brno areas. The "x" ordinate represents the observed thickness of soil or sediment in meters; the "y" ordinate represents the types of soil or sediment in the order in which they are supposed to succeed each other as the climate changes from mainly cold to mainly warm. Each soil recorded is identified by the cycle to which it belongs and by a letter (e.g., E_{2-a}). C^{14} dates are all from Vestonice (Kukla and Klíma 1961). Warm forest malacofaunas are found buried in Ca-horizons of lessive soils (Ložek 1964). Absolute chronology is based on comparison with the winter insolation curve for 55°N latitude (cf. Figs. 2 and 3). Cyclical division is according to Kukla (1961).

able adaptation. To be sure, cases of osteoarthritis occur in the fossil series from Krapina, La Ferarssie, La Quina, and La Chapelle-aux-Saints; but rheumatic diseases also occurred among the Cro-Magnon population as well as in later Mesolithic and Neolithic communities in Europe, and osteoarthritis has been identified in such Pleistocene mammals as *Bos primigenius* and the cave bear. If the Neanderthals disappeared in the dramatic fashion so often described, the causes may reside in factors which leave no trace in the fossil record: the capability of the Cro-Magnon peoples to form socially cohesive units with a slightly higher margin of efficiency and success than could be realized in the social traditions of Neanderthals. More effective methods of communication and cooperation between individuals of a social unit may be of critical adaptive importance in the competition for food in an area undergoing climatic and biotic changes during and between glacial optima. While the case for extinction by physiological disability cannot be ruled out, the morphology of the large sinus cavities of Neanderthal Man and the incidence of rheumatic disease recorded in his fossil remains cannot in themselves stand as evidence for extinction by flu and other ailments popularly associated with life in a cold climate.

by J. KUKLA☆

Prague, Czechoslovakia. 17 V 67

Emiliani's newly adjusted O^{18}/O^{16} temperature curve is welcome. The placing of the Kotzebuan peak at 170,000 years encourages comparisons with events of continental glaciations. However I am still skeptical of many isotope dates from sea cores for the period between 70,000 and 300,000 years because so little is known of the geochemistry of the ocean floor. The Recent-Pelukian section of Emiliani's curve shows close resemblance to the Pelukian-Kotzebuan division, where the same number of minor temperature cycles seem to be registered. Each division may easily be considered to some periodic climatic variation causing glacials and interglacials.

A loess-section curve recently constructed after detailed studies of a number of well-preserved loess and soils sections resting on river terraces of the Bohemian massif may be of particular interest in this connection. Sedimentation pockets filled with richly divided strata, including palaeontologically evidenced interglacials, show the same sequence of sedimentation and pedogenesis in two areas 200 kilometers apart (Kukla, Ložek, and Záruba 1961). The sequences recorded must therefore reflect wide-range climatic changes rather than local sedimentation conditions. Carbon-14 dates from Vestonice fix chronologically the more recent part of the loess curve (Kukla and Klíma 1961). (Sample Gro 2105 has recently been re-dated by Vogel [personal communication to Klíma] at more than 34,000 years B.P. instead of the 52,000 years originally reported.) Sequences of clearly different ages show surprisingly close correspondence. This observation has led to the definition of sedimentation cycles of first and second order (Kukla 1961). Six first-order cycles, each of them representing the timespan of one glacial and one interglacial, were originally described (Kukla and Ložek 1961). Today much new evidence is available, demonstrating the existence of eight cycles below the Holocene at the principal site of Cerveny kopec. Almost completely developed soils and loess sequences C to I underlie the incompletely preserved soils of cycle B. Two pedocomplexes below PK V, cycle D, each corresponding to one first-order cycle, have recently been discovered. Drilling has shown that soils of the Stranska skala locality, 6.5 km east of Cerveny kopec, which contain a rich Biharian fauna (Ložek and Fejfar 1957), must, because of their relations to terraces, be younger than cycle I and older than cycle D. Soil-development comparison has placed them equivalent to cycle E or F (Kukla, unpublished). A closer fix is expected to be obtained in the future by petrographical methods.

The loess-section curve (Fig. 1) shows the observed sequences of soils and sediments in the best-preserved sections of Bohemia and Moravia. Whereas sediments of cycles I to E have been studied only at Cerveny kopec, the well-developed cycle D is known from six sites, cycle C from twelve sites, and cycle B from several.

The curve is tentatively dated on an absolute time scale based on the Croll-Milankovich theory of changes in the earth's orbit but using for dating the changes in insolation in the Northern Hemisphere in the winter, rather than the summer, half-year. This point should be explained in more detail.

An attempt was originally made to apply either the Milankovich (1941) or the Brouwer and van Woerkom (1953) summer half-year insolation curves to the data, but no logical parallel appeared for the most recent, C^{14}-dated part of the loess-section curve. The primarily cold period between 30,000 and 65,000 years B.P. cannot be explained in terms of any reasonable response time (Broecker 1965). Nevertheless, it is obvious that the cyclical repetition of loess and soils must have been caused by some astronomical factor. Two possibilities remain: either it is the Southern Hemisphere that determinates world climate—which can hardly be accepted—or it is not the summer half of the year, but the winter one, that plays the decisive role.

So, using Milankovich's (1941) table XXV (calculated for every five milen-

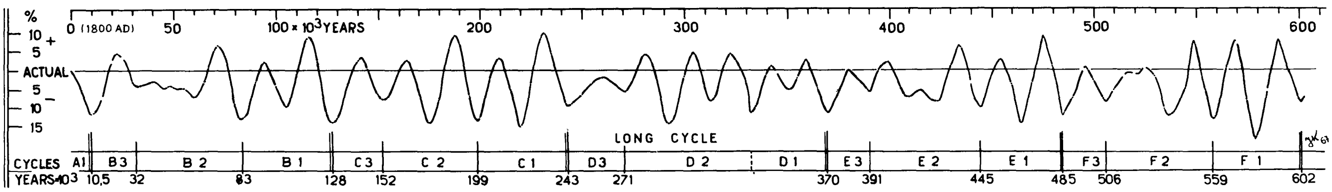

FIG. 2. General curve of winter half-year insolation for 55° N latitude, based on Milankovich's (1941) calculations. Parallel with loess-section cycles is shown (cf. Fig. 1).

nia, I constructed the general winter half-year insolation curve for 55° north latitude (Fig. 2) and compared it with the known C^{14} and Th^{230} Pa^{231} evidence (Fig. 3). A surprising degree of correspondence was found. Main Würm (Wisconsin) period shows generally low winter insolation. The Holocene and Paudorf (Plum Point) interval began when winter insolation abruptly increased. Similar rapid increase accompanies the high sea stands of Tyrrhenian 2 and Tyrrhenian 1 (Stearns and Thurber 1965). An especially close general correspondence to Emiliani's curve exists up to about 65,000 years ago. More than a general correspondence of major oscillations can hardly be expected without a more detailed calculation of the insolation curve. There exist, for example, differences in the location of the Paudorf insolation peak (22,000 B.P. according to Milankovich, *ca.* 25,000 according to Brouwer and van Woerkom). It is evident, however, that the main cold-climate episodes are synchronous with winter insolation decrease and vice versa. Turning points seem to have some special importance, perhaps as an impetus to climatic change. The detailed mechanism of the response of world climate to insolation changes is without any doubt very complicated and remains unclear (cf. Mitchell 1965). When this problem has been clarified and the importance of winter accepted, the chance selection of summer as decisive in determining climate will probably be considered the most serious mistake in Quaternary research in recent years.

Some arguments in favour of winter insolation changes as determinants of world climate are the following:

1) According to the Milankovich tables, mean summer insolation at 55° N latitude was about 1% higher during last 100,000 years than today. On the basis of this fact alone, no glaciation at all would be expected. Winter insolation, on the other hand, was more than 3% lower.

2) The summer insolation minimum over the past 10^5 years was only 3% lower than the present level, the maximum 4.4% higher. Some authors (Öpik 1953) have argued that so slight a difference cannot have caused glaciations. The winter insolation minimum, on the other hand, was 13% lower, the maximum 7% higher. The increase that accompanied the Eemian transgression reached 23% at 12,000 years B.P., enough to influence world climate substantially.

3) The albedo during glacial winters must have been considerably higher than during winters of interglacials due to the large snow-covered areas in front of the glaciers. In summers the difference would have been much less, with high albedo being restricted only to glacier-covered areas. The *austausch Koeffizient,* twice as high in winter as in summer, must also have played an important positive role.

4) The observed lowering of sea level in Holland between 1730 and 1820 was accompanied by a decrease in mean January temperatures but practically no change in July temperatures and an increase of mean annual temperature (Fairbridge 1961).

5) The comparative studies of microclimate of glaciated and non-glaciated areas at similar latitudes that have sometimes been used to support the relative importance of summer insolation (Köppen and Wegener 1924) have a serious defect: the areas compared are under the influence of the same insolation.

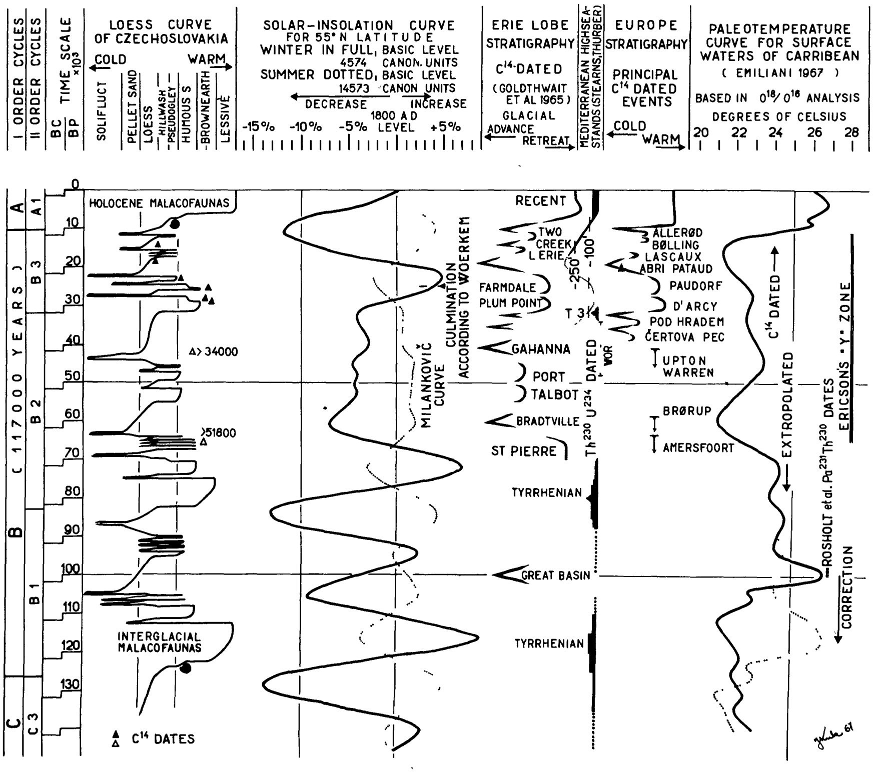

FIG. 3. Comparison of loess sections of the last glacial cycle with major isotope-dated Pleistocene events, with winter and summer insolation curves, and with Emiliani's palaeotemperature curve, Definite C^{14} dates are marked by a solid triangle, others by an open triangle and an estimate in figures. Sources other than those mentioned above are Kaufman and Broecker (1965), Leroi-Gourhan (1965), Morisson and Frye (1965), Movius (1960), and Musil and Valoch (1966).

For the purpose of loess-curve dating, however the summer vs. winter dilemma is of secondary importance. The principal task is to find some periodic variation in insolation which might correspond to the first-order cycle of loess sections. Two approaches are available: (1) In Figure 3 a time-span of 117,000 years lies between the beginning of the Holocene and the probable beginning of the Eemian. The general insolation characteristics of this time span closely correspond to those of the preceding 115,000-year period, thus revealing the existence of a cycle of 115-117 millenia. (2) The only periodic variation in the insolation curve longer than 50,000 years (the minimum possible C^{14}-dated duration of the last glacial) is a cycle of 115,000-117,000 years. Every third cycle seems to be longer, from 125,000 to 127,000 years (cycles D and G). The normal 116,000-year cycle consists of five minor periodic repetitions (probably caused by variations in the precession of the equinoxes) 20,000-23,000 years long. The first three have much greater amplitude than the last two. The first and the third insolation peaks, about 40,000 years apart, are contemporaneous with the fictitious two-fold glaciations of the Milankovich curve. The last twin of cycle B corresponds to the doubled forest soil of the lessivé sequence in the loess sections. This comparison is supported by the high sea stands of 85,000 and 120,000 years B.P. and by the interglacial fauna that accompanies both the lower lessivé and the older high sea stand.

I consider the evidence sufficient to reintroduce the insolation curve to Quarternary research. If the loess-section glacial cycle has some astronomical cause, then it can hardly be other than insolation changes, and if such is the case the glacial cycle must logically be 115,000-127,000 years in duration, at least for the last million years. I have therefore added tentative absolute dating to the loess-section curve and, for better comprehension, superimposed on it the terms of classical European stratigraphy according to Woldstedt (1966). There is a general correspondence between the curve and the classical sequence as to the position of the Stranska skala Lower Biharian fauna. The total length of classical European glaciation agrees roughly with the latest estimates (Evernden and Curtis 1965). The soils of cycles G, H, and I are much more intensive than any younger soil (cf. Brunnacker's [1964] Riesenböden). Our curve shows that care must be taken in evaluating sections from the remote Pleistocene; complicated sequences of climatic oscillations such as occur in the more recent part of the Pleistocene, must also be expected there. The tentative dating of the loess-section curve will remain only a working hypothesis until sufficient isotope data has been collected to support it.

A similar approach would surely be useful in deep-sea core research, where sedimentation rates cannot be supported by isotope dating. Emiliani's Eem temperature maximum, placed at about 98,000 years (Rosholt *et al.* 1961), disagrees with the generally accepted dates of 85,000 and 120,000 years for high sea stands which must have been separated by a cold episode with low sea level. At least a general parallel may be drawn between the Holocene and the first part of the Eemian. In the Holocene, the high sea stand is evidently synchronous with Emiliani's sea-water temperature peak; but the Eemian high sea stand of 120,000 years would be accompanied, according to Emiliani's curve, by a very low sea temperature, comparable with to the one recorded during the Wisconsin glaciation. Since this can hardly be the case, I have recommended relocating the Eem maximum as shown in Figure 3. The Broecker *et al.* (1958) estimate of 150,000 years for the age of the "X-V" boundary partly supports this recommendation.

Emiliani's objections to established nomenclature are more than logical. For geologists working on continents, however, it is practical to continue with the established division of Holocene and Pleistocene, maybe setting off the Villafranchian (or the Eopleistocene of Russian authors) as a separate period. Subdivisions of the Pleistocene should be based on cyclical climatic changes, whereas the delimitation of the Villafranchian can only be based on biostratigraphy (Zubakov and Krasnov 1959).

As for Paleolithic populations resisting the harsh glacial winters of Europe, we must point out that no evidence is available of the presence of man in the middle latitudes of Europe during a glacial winter. It seems much more likely that the Paleolithic hunter followed his quarry into the North in glacial times only in summer and retreated in winter to southern France or the Black Sea basin. The incredibly long migrations of the Great Lakes Indians, recorded for instance by Tanner (1940), make this seem probable.

by G. KURTH☆

Braunschweig, Germany. 14 III 67

Discussing Emiliani's methods and results in dating Pleistocene levels is not the palaeoanthropologist's business. His proposal that we use terms like Tertiary, Quaternary, Holocene, and Post-Pleistocene only informally will meet general agreement and should perhaps receive more emphasis if we are to arrive at a better understanding of the ongoing process of evolution. Whether or not average temperatures based on criteria specific to coastal areas are relevant also for other parts of a continent seems less clear; for example, the protected area of Bohemia and Moravia appears, on present evidence, to have had relatively high summer temperatures during the last glaciation and to have therefore supported what was for that time a dense population.

Regarding the long process of achieving erect posture, Kummer (1956) has shown that *Oreopithecus* probably had a genu valgum. This indicates a trend toward bipedal walking already in the Miocene, although it is impossible to trace direct connections to the bipedal human hominids of Africa. Generally we should be very careful, in our present stage of knowledge, not to deduce too much as to the geographical location of any particular process, much less of speciation. Differences in the intensity of research and in the opportunities to reach fossil-bearing levels may make our views very one-sided.

It is not clear that one can deduce *Australopithecus africanus* from *A. robustus* (cf. Heberer 1965). The known fossils of *A. robustus* are generally more recent, some even belonging to the early Middle Pleistocene. Mixed diet is well known from ground-living primates (e.g., baboons) and is probably very old among hominids. I do not have enough space here to discuss the taxonomy of the human hominids (but see Kurth 1967). In any case, their probably slow rate of increase and the length of their generations suggest that successful migrations over large areas with different environmental conditions would have required a very long time. Our fossil finds are scattered and still insufficient for tracing lineages even though we can describe morphologically specific or subspecific groupings. I would hesitate to deduce from the relatively richer European fossil record that the "centre" of *sapiens* speciation was in Europe, despite the fact that conditions there favored isolation. The few human fossils and the similarly scarce cultural remains before the Würm cover such a long time span that consideration of direct connections can be speculative only. Is it not enough that fossils for one time-horizon demonstrate one particular trend of differentiation? Why do we need more special and speculative interpretations when the facts demonstrate the validity of our general theory? We are still too much in the stage of gathering facts toward a better reconstruction of our past; it will be a long time before we can correlate the data gained by different methods.

by GABRIEL W. LASKER☆

Detroit, Mich., U.S.A. 8 III 67

Since Emiliani (1956*b*) is one of those who has proposed relatively recent dates for the glaciations of the Pleistocene, it is very helpful to have his views on how the Pleistocene dates of human evolutionary events should be revised on the basis of the new evidence. When he calls on "modern views on speciation and evolution" to help relate the fossils, however, perhaps he is putting the weaker case forward. There are differences as well as similarities between man and other organisms, and one should develop concepts of human evolution on the basis of human biology, the fossil record, and archaeological dating and context.

For instance, Emiliani's casual remark that cannibalism may have played a very important role gives me an excuse to point out that, since the population could not continually decline without extinction of the species, cannibalism never can have been a very important source of human food for the species as a whole for any appreciable length of time. Let us assume that man's remote ancestors used for existence only half the present recommended dietary intake, but grew to nearly present-day size at each age of life. Then a pregnant female would eat some 1,600 calories per day for 280 days of pregnancy, but the newborn infant would yield as food only about 3,200 calories—140 calories to nurture the infant for each calorie it could yield as food. By one year of age the child would have consumed another 182,000 calories (even if one made no allowance for a nursing mother), and the ratio would be about 70 calories per calorie available to the cannibals. By three years the ratio would be about 84 to 1, by six years 104 to 1, by nine years 111 to 1, and so on. Furthermore, some individuals must survive to adulthood, and no species could utilize all its dead as food. Therefore, whatever the importance of cannibalism in magic, warfare, and other aspects of human life, a warm-blooded animal with long gestation time, usually single births, and relatively slow growth could not get more than a small fraction of one per cent of its food from cannibalism. The low ratio of *Australopithecus* remains to other fossils at Makapansgat is in accord with this interpretation.

If there were two sympatric species of *Australopithecus* (or one Australopithecine and one hominine), and one were carnivorous and one not,then the remains of the vegetarian would be relatively more numerous if it were an important food resource of the carnivorous species. However, the anatomical evidence adduced to support the existence of such different adaptations in specimens assigned to each of two sympatric species is weak. Furthermore, the known remains of the two kinds are similar in frequency.

by JOHN M. LONGYEAR, III☆

Ithaca, N.Y., U.S.A. 10 III 67

Although I intend to confine my remarks to a few points in the last section of Emiliani's paper, I would like to note in passing that his discussion of the Pleistocene as revealed by deep-sea and other sediments is a welcome synthesis to a non-specialist such as myself.

The assumption of bipedal posture by our remote ancestors was surely more important than Emiliani suggests. This step was not only "likely to have been selective," it was crucial, and the consequent improvement in perception and communication was unquestionably of far less importance, from a selection standpoint, than was the freeing of the forelimbs for manipulation, carrying, etc. This same observation, of course, has already been made by many authors: LaBarre (1954), Washburn (1960), Hewes (1961), and Hockett and Ascher (1964), just to name a few.

Emiliani speaks of Pleistocene Europe and Asia as a "typical evolutionary mill." In truth, this region at that time must have been occupied by small, isolated, and inbreeding groups of humans, hemmed in by barriers of ice and water, and only occasionally meeting other bands and interbreeding with them. It was under these circumstances, according to Wright (1931) and others, that repaid, adaptive evolution was most likely to take place. The question remains, however: just what, exactly, was *adaptive* under those circumstances? Was it modification of the body for greater tolerance of cold? This has been suggested; in fact, the Neanderthal type itself has been explained at least in part, as an example of adaptation to cold (Coon 1962). But there are obstacles to this line of reasoning. One is the disappearance of Neanderthals at about the Würm maximum, just when they should have been most successful. Another is the interpretation, by Emiliani and others, of certain Neanderthal peculiarities as *maladaptive* rather than *adaptive*, to cold.

Given the course of hominid evolution and history up to mid-Pleistocene times, it seems to me that the *only* trend which could be considered adaptive from then on, no matter what the environment, would be a trend toward any modifications of mind or body which would heighten man's ability to invent, use, and maintain culture. Neanderthals may have had brains as large as, or larger than, that of *Homo sapiens sapiens*, but I do not think we can assume (with Emiliani) that they were as "intelligent." They simply did not have the *cultural* ability of *Homo sapiens sapiens*, and therefore were not as well adapted to the human way of life.

by M. A. MACCONAILL☆

Cork, Ireland. 10 III 67

Emiliani's article is a very valuable contribution, particularly from the non-specialist's angle. Its value lies as much in its concision and clarity as in the information it supplies. An objective definition of epochs is highly desirable. Our author is voicing the feelings of many of us when he says that "some of the confusion existing in Pleistocene literature appears to result from semantics alone."

His general sequence from *Australopithecus* onwards is reasonable enough when it is combined, as he suggests, with overlap. His suggestion that the Neanderthals could have been even more intelligent than ourselves is delightful. How angry it would have made those older anthropologists whose canon was so often *Facies eius non mihi placet, ergo stultissimus est.* (I just don't like his look, therefore the fellow's a fool)!

Reverting to the matter of semantics, some semantic rectifying could well be performed on the current naming of the real or presumptive ancestry of *Homo hodiernus*. Emiliani defines Man as "a primate capable of systematically manufacturing tools," thus making *behaviour* his criterion of humanness. This is commendable, for it is the canon of a complete anthropologist, one who cares for culture as much as for crania. His criterion is respectable insofar as *c* (the capability of systematically manufacturing tools) implies *h* (humanness); but if it is to be a true definition, then *h* must also imply *c*, that is, *c* and *h* must be equivalent. Our author suggests that *Australopithecus africanus* may have evolved from *A. robustus* by developing a taste for meat and by discovering the need for "the imaginative usage of tools and the employment of sophisticated strategies" to keep the larders filled. But are not the latter two "aids to evolution" precisely those that distinguish man from all other animals? What is the computer but an imaginative use of tools? What is the theory of games but an attempt to develop even more sophisticated strategies? In short, the powers connoted by the aforesaid "aids" are precisely what is intended by the term *sapiens.* Consistency, then, requires either that we be told the conditions under which "*h* implies *c*" or that we change *Australopithecus africanus* to *Homo sapiens australis* (or *H. sapiens* with some other adjective). To make *h* and *c* equivalent is to add a second postulate to the acceptable "*c* implies *h*." It does not follow from "All grass is green" that "Everything green is grass." The question *When, if ever, did a primate become human?* is an affair of an unwritten and unwitnessed history. Here, as in other branches of science, we have a realm of abiding uncertainty. Unlike the problem of the epochs, it cannot be solved by isotopes. All we can do is to determine the epochal range within which the thing must have happened. Towards this knowledge Emiliani has advanced our steps significantly.

by CHARLES A. REED☆

Chicago, Ill., U.S.A. 21 III 67

I am delighted to have this succinct summary of Emiliani's research and the relevant work of others, combined with his interpretation of the meaning of the fluctuating paleo-environment to the history of man. Much of this literature, including some of the author's own papers, does not come automatically to the attention of those of us whose training and experience has been primarily biological.

General agreement is at last emerging, from what has seemed to be a welter of dispute, concerning the time and changing events marking the Plio-Pleistocene boundary. To the date of "more than 3.3 million years for the beginning of the Villafranchian" in France can now be added the near-identical date for the inauguration of mountain glaciation in the Sierra Nevadas of California (Curry 1966).

I wonder if geologists who specialize in the marine Pleistocene always record temperature (climatic) oscillations lasting approximately 50,000 years each in terms of cycles per second (cps). Emiliani reports that the temperature oscillations of the Late Cenozoic happened with a frequency of 6.7 x 10^{-13} cps. My grade-school arithmetic translated this into 21.13 cycles per million years, and in the next column Emiliani agreed: "The total number cycles occurring . . . during the past 1,000,000 years problably numbered 20." Why not say so the first time and avoid the possible errors to my arithmetic?

If each such cycle has occurred *ca.* each 50,000 years, for a million years past, can we somehow correlate 20 such cycles with the four (or five?) textbook glacial periods, even allowing for the addition of stadials, or should we abandon the old system as unworkable? Also, what can be said of the preceding more than two million years of the Pleistocene? Should we now expect to find evidence for three million years or more of glacial cycles? The pattern becomes more complicated when we think of the possibility of correlated pluvial cycles in more tropic latitudes.

I will also welcome an explanation of the statement that "other animals" have evolved long necks in lieu of mechanisms for offense and defense. Long necks, relative to body size, have evolved in giraffes and gerenuks, but the length of neck in each case is a part of a complex feeding mechanism and is not used for either offense or defense, in the common meaning of the terms. (Contests between male giraffes, with the heads being swung as battering rams on the ends of the long necks, are part of their social behavior.) The necks of some carnivores (polar bear, cheetah, weasel) are longer than are those of their respective close relatives, but would also seem to have evolved as a part of a feeding mechanism. Such a mechanism in a carnivore may be considered "offensive"; but basically a giraffe lifting its head to browse and a weasel using its length of neck to aid in catching a mouse are utilizing similar kinds of feeding adaptations. No Primate, with the capabilities inherent in the grasping hand, needs an elongated neck as a feeding adaptation; the Primate has instead the whole length of forelimb.

Does Emiliani perhaps overstress the relationship between glacial climates and the evolution of *Homo erectus*? We welcome all suggestions, certainly, but *H. erectus* lived throughout much of the Eastern Hemisphere (from southern Africa to Java to northeastern China to central and southwestern Europe). Over this area many environmental variables existed; glaciation and its accompanying climatic changes, however dramatic in our eyes, may not have been the selective factors responsible for molding what we call *erectus* to a population we define as *sapiens.*

We know nothing of the differences, if any, in rates of human evolution in glacial and non-glacial regions and/or periods. Indeed, there seems little evidence at present that *Homo erectus* lived in extreme glacial cold. The conditions at Choukoutien, admittedly cold-temperate (but certainly not with summers as cold as those of European winters today), may have been as difficult as any survived by *Homo erectus,* since the Hungarian site at Vertesszöllös has been described as having been occupied during an interstadial period within the Mindel (Kretzoi and Vértes 1965).

Even during a period of continental glaciation, I cannot believe that the circum-Mediterranean area would have had summers as extremely cold as postulated by Emiliani. Even under conditions of increased cloud cover, the summer sun must have accomplished considerable warming, quite as it does in the Arctic today. Under summer conditions postulated as being as cold as those of the winter of many parts of Europe today no insects would fly and no insect-pollinated plants survive. What would such conditions mean in terms of floral and faunal succession, and of total biomass? What would be the number of frost-free days, what the length (if any) of the growing season? What would the surviving flora provide in the way of food, not only directly to man but to a fauna upon which a human population could also depend? I have no answers at present to most of these questions, but in the circum-Mediterranean area, I cannot believe that the environment during a glacial stage was as extremely frigid as Emiliani's phraseology must certainly suggest to most readers.

Our knowledge of glacial environments is best derived from studies of the last maximum of the Würm-Wisconsin, when conditions over most of Europe would admittedly have been difficult for a hominid at the cultural level of *Homo erectus.* At this time a few hundred miles of tundra separated the ice-front from the forest, and the livable habitats for *H. erectus* in Europe (if conditions were similar during some part of his period of occupancy there) would probably have been reduced to the Mediterranean coastland and the peninsulas of Iberia, Italy, and the Balkans (Wright 1961: Fig. 6). Were conditions in these areas during the Würm maximum as extreme as Emiliani has postulated them to have been in that prior glacial period during which *H. erectus* was supposedly occupying these same regions? I think not.

However, as I mentioned before, the circum-Mediterranean was not the only region in which *Homo erectus* lived, nor was the glacial environment the only region in which he could have evolved. Earlier hominids, in southern and eastern Africa, had seemingly not needed the stimulus of a peri-glacial environment for their particular biological and cultural passage through the australopithecine grade.

by KARL H. SCHWERIN☆

Albuquerque, N. Mex., U.S.A. 13 III 67

Clearly one of Emiliani's objectives is to clarify our understanding of recent geological epochs and to sharpen the definition of boundaries. I will limit my comments to these considerations.

Understandably he is approaching these questions from the point of view of a geologist-palaeontologist. Boundaries are defined by the appearance of certain marine species at a particular locality in space and time, or by geological evidence of past temperature variations with which the occurence of such marine species is correlated. This approach leads him to state,

> It will not come as a great surprise if it should appear, when the Plio-Pleistocene boundary has been dated, that the genus *Homo* dates from well back into the Pliocene.

If, however, we are to define *Homo* as "a primate capable of systematically manufacturing tools," i.e., a *culture-bearing* animal, I think one could argue on evolutionary and logical grounds that the appearance of culture-bearing hominids should represent at the very least a new geological period, if not the start of a new era. The evolutionary process has certainly not ceased, but it has clearly taken a new direction with the development of culture.

Of course, from a strictly geological point of view, one might argue that the existence of man, along with his culture, has had little impact on the Pliocene and Pleistocene record. Though we do find cultural remains, they and human skeletal materials would seem *to the palaeontologist* to be little different from the fossil remains of other species.

Within the past 10,000-15,000 years, however, man has begun to have a profound and widespread effect on the biological and geological features of the earth. Since plants and animals were first domesticated some 10,000 years ago, man has not only spread his domestic species at the expense of wild ones, he has cleared forests, plowed grasslands, drained lakes and swamps, irrigated deserts, and greatly speeded up natural processes of erosion (some of these effects are discussed in Thomas 1956). These biological and geological effects are of sufficient magnitude that I would expect them to produce a marked disturbance or unconformity in the geological record, comparable to that which separates earlier geological stages. The unique thing about this unconformity is that, for the first time in the history of evolution, it has a cultural base. This disturbance may seem insignificant from our perspective, but I am sure it would not be if it were viewed by a geologist several million years hence, and in the context of the adjacent geological strata.

Emiliani himself goes so far as to recognize a faunal change, noting that many animal species have become extinct in the past 15,000 years as a consequence of human expansion. Thus I fail to understand his objection to "Holocene." If I understand him correctly, he would replace this with a mere subdivision of the Pleistocene, which he calls the "Flandrian"; i.e., he would lump recent geological history with the preceding epochs of the Cenozoic. I hope I have shown that there are good, indeed compelling, reasons for classifying the last 10,000 (or perhaps 15,000) years as marking a distinct geological epoch—one which, however, is defined *by completely different criteria* than those which have preceded it.

Returning to the question of periods versus eras, if we are to agree with Simpson (1960:286-87) that with culture "a fundamentally new sort of evolution has also appeared," then it seems to me appropriate to recognize a new era, the Culturozoic (or Paideozoic, if the Greek form is preferred). Huxley argues (1956:6) that this had its beginning mid-Pliocene. Emiliani seems to be of the same mind. The only question is what date we are to assign the mid-Pliocene.

If we are to admit that the development of culture has indeed been so profoundly significant for the course of evolution that it requires recognition of a new geological era, then we must mark the beginning of that era where culture itself has its beginning (which would also provide a terminal date for the Pliocene). Even though culture had little effect geologically during the Plio-Pleistocene (as currently defined), we might view this epoch as analogous to the Palaeocene, which served as a sort of preliminary developmental stage before the marked expansion of most mammalian orders in the Eocene and later epochs of the Cenozoic. My principal differences with Emiliani, then, concern what factors are to be considered in determining boundaries and how much significance is to be ascribed to these factors.

by Günter Smolla☆

Frankfurt Germany. 20 III 67

Everyone will agree that "some of the confusion existing in Pleistocene literature appears to result from semantics alone"; but—alas—I cannot see that Emiliani gives us any helpful suggestions for solving this problem. Terms like "postglacial" or "Holocene" for the past 10,000 years are indeed out of scale compared with the absolute length of the "glacial" and the whole Pleistocene. For the archaeologist, however, the scale is quite different. The past 10,000 years are for him full of events. He has to work not only with geological, but also—and more and more—with historical time scales. The term "Flandrian" proposed by Emiliani may be more "logical" and meaningful in his special field, but for workers in the various fields of biological and cultural history of mankind it can only produce more confusion.

Emiliani accepts the old myth that "man" is less capable for the "struggle of life" than other animals. But what enemies specialize in eating men? What animals of man's size can walk *and* run *and* climb *and* swim as well as he? Even more problematic is the "very important role" of cannibalism in the early stages of human evolution. Every cultural anthropologist knows that cannibalism is absent in known hunting and food-gathering populations, but nearly typical for primitive food-producing communities. There are some hints that cannibalism may have been practiced by early man (Choukoutien Loc. 1), but there is no known analogy by which we can interpret them. And where is the unquestionable evidence that *Australopithecus* killed and ate members of his own or related species?

Without question, changes in ecological conditions during the Pleistocene should have selective influence on human evolution; but it will be very difficult to produce evidence for this speculative supposition. Perhaps "modern man," i.e., *homo sapiens sapiens*, was better able to withstand cold weather than "the Neanderthals," but he replaced the latter not only in cold regions but also in temperate and even warm ones.

The one factor in the evolution of man during the Pleistocene which we must know about above all others is time. We are indebted to Emiliani for his very stimulating work on dating and correlating Pleistocene events, and we look forward eagerly to hearing more from him on those subjects.

by L. Van Valen☆

Chicago, Ill., U.S.A. 3 III 67

Emiliani's stratigraphic and palaeotemperature discussions are quite welcome. The agreement between his results and varieties of Milankovich's isolation curves (Van Woerkom 1953) is rather good and provides presumptive support for both.

Most of the section on biology would, however, have benefited by omission. "Modern views on speciation and evolution" do not require geographic isolation for phyletic evolution, and as far as any kind of evolution at the subspecific level (e.g., Neanderthals) is concerned, geographic isolation, as opposed to occupancy of different areas, would be evoked only in special cases. One species can change into another by evolution in the same continuously occupied area. (See Simpson [1961] for a discussion of the principles of phyletic evolution and Van Valen [1966] for a more detailed theory of subspecific evolution possibly applicable to man.)

Determination of the place of origin of *Homo* is a problem in sampling, even in the unlikely event that this transition occurred in a restricted region. The degree of knowledge of mammalian faunas of appropriate ages must be weighed in each region against the presence or absence of human fossils. The fragmentary evidence from Indonesia and China in fact suggests transitional early *Homo* in these regions also. It is also questionable (Darlington 1957) whether the severity of glacial climate contributed importantly to human or other evolution except for direct adaptations to cold.

Are there any data on the relative frequency of arthritis in Neanderthals and more recent man? I thought the sample size of even all Neanderthals was still too small for age-specific statistics such as this. The suggestion of a relation between the size of air sinuses and the occurrence or severity of upper respiratory disease is interesting and apparently original, even if perhaps unlikely, and could be tested in recent populations.

Reply

by CESARE EMILIANI☆

I am gratified to see that my paper has been honored by such a large number of CA☆ comments. Because different comments often cover the same points, I will provide a general reply, followed by a few specific remarks. Each statement or group of statements is followed by the name(s) in brackets of the author(s) who prompted the statement(s).

According to the principles followed by stratigraphers since "strata Smith," and later internationally agreed upon, the stratigraphic subdivision of the entire geologic column since the Cambrian is based upon *marine* deposits. Each term is defined on the basis of a type section. The term "Pleistocene" is no exception: it is defined on the basis of the type section at Le Castella, Calabria, southern Italy, following the unanimous decision of the 7th INQUA Congress (Richmond and Emiliani 1967) [Butzer].

It is true that the decision of the 18th International Geological Congress to place the Plio-Pleistocene boundary at the base of the Calabrian stage also included a statement to the effect that this boundary be placed "at the horizon of the first indication of climate deterioration" in the continuous late Cenozoic marine sections of Italy. Subsequent research has shown, however, that the first indication of climatic deterioration dates from many million years ago, and the Pleistocene, thus defined would probably include most of the Pliocene. Substantial glaciation in Antarctica problably began 5,000,000 years ago (Emiliani 1957), and evidence for glaciation elsewhere dates from at least 3,000,000 years (Curry 1966). Thus, the Pleistocene cannot be defined in terms of cooling alone [Cooke]. As a matter of fact, isotopic analysis of the Le Castella section has shown that the Plio-Pleistocene boundary, as defined today, is not accompanied by any substantial temperature decrease (Emiliani, Mayeda, and Selli 1961).[1] Thus the boundary, as presently defined, appears to rest on an ecological event of rather limited significance. Within the next few years, absolute dating of marine epicontinental sections and the intensive study of thick oceanic sections from the open seas may provide new evidence advising a redefinition of the Plio-Pleistocene boundary. For the time being, the term Pleistocene should be restricted to its formal meaning: any deviation therefrom will only create confusion [Butzer, Schwerin].

I am quite sympathetic to the problems which scholars dealing with continental deposits face because of the difficulty (or impossibility) of correlating these deposits with the marine sections. While these problems will not be solved by applying indiscriminately (i.e., without the support of absolute dating) the marine terminology to the continental deposits, they can be quite easily solved by establishing an independent palaeontological nomenclature based upon the fossil evidence of the continents, a road which the Soviet students have already adopted with their usage of the term "Anthropogene." I agree with Anati that CA should coordinate this meritorious effort. The continental terminology would then be related to the marine one only at the points where absolute dates are available. Only a few such points exist at the present time beyond the range of C^{14}: these, together with the points provided by C^{14} dating, indicate a general agreement, within the limits of error of the various analytical techniques, between oceanic, marine epicontinental, and continental events [Butzer, Cooke].[2]

The notion that there were four glaciations has such great classic fascination that it will not die easily. However, the evidence from both marine and continental deposits, indicating that there were considerably more than four glaciations, is both compelling and mounting. For instance, Kukla, in his comment, mentions eight loesses; Scherf (1936) found eleven weathering horizons in the loess section at Paks, Hungary; and Lona (1963) illustrates eight climatic oscillations within the Donau-Mindel interval, all having *full* glacial/interglacial amplitude. I would venture to say that, whenever the stratigraphic record is analyzed in *sufficient detail,* strong climatic oscillations are noticed; and whenever a time scale (or even a time estimate) exists, the wave length appears to be in the neighborhood of 50,000 years [Gigout, Ree]. These analyses, of course, are most easily done on oceanic sediments. Sea-level changes also appear to follow the time scale provided by the deep-sea cores. Thus, the high stands dated at 125,000 and 85,000 years ago correspond to dated high isotopic temperatures, separated by a temperature minium at about 110,000 years [Kukla].

The relationship between semi-annual insolation variations at different latitudes and climatic oscillations requires closer analysis. Köppen and Wegener (1924) assigned predominant significance to the summer insolation variations at 65°N. Kukla, in his comment, speaks in favor of the winter insolation variations at 55°N. I believe that these are only first-order approximations, and that the effect of insolation variations at different critical la-

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[1] It is curious that, of the many hundred molluscan and foraminiferal species which occur in the North Atlantic, only two dozen species entered the Mediterranean at the beginning of the Calabrian and nearly all of these species are common to both the North Atlantic and the Baltic. Species living in the Baltic must necessarily be adjusted to abnormal salinity conditions. I wonder whether the northern guests may not have entered the Mediterranean through a temporary passage in southwestern France, acting as a salinity ecologic filter. The maximum required subsidence would be about 186 m. along a small area between Toulouse and Carcassonne. The scanty marine deposits in the area may have been entirely eroded away since, thus eliminating any evidence of such a marine transgression.

[2] I do not accept as definite any C^{14} date greater than about 45,000 years [Butzer].

titudes and in different seasons should be evaluated. For instance, it may be argued (Emiliani 1966) that summer-insolation minima in the accumulation latitudes coupled with insolation maxima in the evaporation latitudes will tend to produce strong glaciations; summer-insolation maxima in the accumulation areas coupled with insolation minima in the evaporation areas will tend to produce warm interglacials; and intermediate conditions may produce milder glacials and interglacials [Kukla].

The glacial/interglacial amplitude of the climatic oscillations remained essentially constant at an estimated 60°C during the past 425,000 years, with the single execption of deep-sea core stage 3. This temperature range is essentially the same as today's winter/summer range for the open North Atlantic, and it is on the basis of this similarity that I stated that glacial summers were essentially similar to intergracial winters I now believe that I did overstate the case, and that what is true for the open North Atlantic need not be true for inland seas (North Sea, Baltic, Mediterranean) or for the continents at middle and high latitudes. There, the glacial/interglacial range appears to have been smaller than today's winter/summer range. As an example, the glacial/interglacial range as measured by isotopic analysis of fossil marine shells (food refuse) from the Haua Fteah Cave of Cyrenaica appears to have been about 8°C (Emiliani *et al.* 1964: Fig. 5) while today's winter/summer range is 11°C. A more appropriate generalization would be that, for Europe and the Mediterranean basin, glacial summers were similar to today's late autumns [Frisch, Hester, Holloway]. In the low latitudes, on the other hand, the glacial/interglacial range was much greater than the modern winter/summer range. My estimate of a 6°C temperature decrease for the Caribbean-equatorial Atlantic area was found by van der Hammen and Gonzales (1960) to coincide with estimates obtained by them from the fossil floras.

We know, from direct palaeontological and palaeobotanical evidence, that the climatic oscillations of the Pleistocene had a profound effect on both plants and animals, not only on the continents but also in the sea. There is little doubt that, when floras migrate, plant-eaters also migrate, and meat-eaters follow. Thus, the Pleistocene was a time of great *and repeated* migrations, a phenomenon per se conductive to the splitting of large populations into smaller groups and to genetic isolation of some of these: It is not necessary, for isolation, to have intervening glaciers: an intervening hostile ecology may be sufficient [Holloway, Kukla, Reed, Van Valen]. Conditions for rapid evolution were established for the entire biosphere, and not for man alone [Brace]. I do not believe that, as a rule, an entire species speciates into a different one. My reason for this belief is that, a hundred years after Darwin, the missing links are still largely missing in the geological record [Brace].

It is an almost universally adopted view that the most important result of bipedal posture in early Hominidae was the freeing of the anterior limbs for toolmaking. My point however is that bipedal posture was not achieved through the necessity of making tools, but through the more primordial necessity of better perception for scouting. Mammals which have attained a certain body size (making hiding difficult), which are preyed upon by carnivores, and which have no adequate defense mechanism, tend to have their perception centers as high above the ground as permitted by their general body architecture (gazelles, zebras, etc.). This is not the case for other mammals having similar feeding habits but endowed with powerful offense and defense mechanisms (bisons, rhinoceros, etc.). Giraffes developed extra-long necks (and forelimbs) as a secondary effect related to tree-grazing. Early Hominidae, even though they may have been able to swim, run, climb, etc., would have been successful in these activities, as evasive practices, only if alerted in time. It would seem that they too would have benefited from increased perception and that the benefit might have resulted in vertical posture [Holloway, Longyear, Smolla, Reed].

Whether or not the genus *Homo* dates from the Pliocene depends on two things: (*a*) the definition of *Homo;* and (*b*) the absolute age of the appearance of *Hyalinea baltica at* Le Castella. If *habilis* is assigned to *Homo,* and if the appearance of *H. baltica* at Le Castella is as recent as some micropalaeontological evidence suggests (about 700,000 years, according to Hay and Boudreaux 1968), then *Homo* originated in the Pliocene [Holloway].

I will conclude with a few assorted remarks. I should have used *subspeciated* instead of *speciated* when discussing the origin of *Homo sapiens sapiens* [Howell]. The term *Flandrian* is not my idea, but was proposed by Dubois in 1924 [Schwerin, Smolla]. I do not equate evolution with changes in cranial capacity [Anati]. I wish to amplify my statement on cannibalism to emphasize that cannibalism includes not only nutritional cannibalism but also cultural cannibalism. The latter is likely to have been more important than the former, and I should have discussed this point [Anati, Lasker]. The age which I estimated for Swanscombe (Emiliani 1956) is 125,000 years and not 95,000 years. Also, I provided no time estimate for Steinheim [Coon]. The Neanderthals had apparently a very advanced culture: they not only invented religion but exhibited excellent judgment in choosing the right place (Monte Circeo, only 50 miles from Rome) [Longyear].

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