The Origin of Man

The following series of articles present many of the data relevant to the discussion of the new finds in Olduvai Gorge. Evernden and Curtis' 1st major publication of the Potassium-Argon method of dating, submitted to CURRENT ANTHROPOLOGY 19 ii 64, was sent for CA☆ Comment to 22 scholars of whom the following responded with written comments: William Bishop, Loring Brace, J. Desmond Clark, Paul E. Damon, R. L. Hay, D. M. Hopkins, F. Clark Howell, Adolph Knopf, Miklós Kretzoi, Harold E. Malde, J. R. Richards, Donald E. Savage, and H. E. Wright. The comments written for publication are printed in full after the authors' text and are followed by a reply by the authors.

While the Evernden and Curtis contribution was being circulated among commentators, it was suggested that Hay's *Science* article on the geology of Olduvai Gorge also be circulated among the same commentators for combined CA☆ treatment; therefore, Hay's article is republished here, and his reply to the comments follows that of Evernden and Curtis.

These comments and replies had already been prepared for publication when it seemed useful also to reprint from *Science* the new description of fission track dating which bears upon the finds at Olduvai Gorge.

Meanwhile, discussion of the new discoveries in Olduvai Gorge was proceeding in a rapidly growing series of articles in *Nature*. Correspondence with Dr. Tobias indicated that he had, in process, a major article which if published together with the series in *Nature* could initiate a significant discussion among the authors and Drs. F. C. Howell and J. T. Robinson. Tobias' article was, therefore, sent to all of the authors of the articles as well as to Howell and Robinson, with a request to comment on the entire discussion. The following responded with written comments: M. H. Day, F. C. Howell, G. H. R. von Koenigswald, and J. T. Robinson. The comments written for publication are printed in full after Tobias' text and are followed by a reply by Tobias.

1

The Potassium-Argon Dating of Late Cenozoic Rocks in East Africa and Italy

by J. F. Evernden and G. H. Curtis

INTRODUCTION

WE REPORT HERE a group of potassium-argon dates that are distributed in the time-range 0 to 15,000,000 years.[1] We show that:

1. potassium-argon ages of very high precision are obtainable on sanidines as young as 30,000 years. As the attainment of high precision is largely a matter of argon extraction technique, we include an extended discussion of our procedures;
2. our earlier estimates of the age of *Zinjanthropus* are now substantiated by an extensive series of dates from Bed I, Olduvai Gorge. As these ages have been questioned, not as to technical precision, but as to accuracy in the geological and anthropological sense, we discuss the possible causes of invalidation of these potassium-argon ages and conclude that available data to which we attach significance are reliable in the geological and anthropological senses;
3. Bed I and Bed II, Olduvai, including Olduwan and Chellean cultural remains, represent a time interval of at least 1,350,000 years, extending from approximately 1,850,000 years ago to something less than 500,000 years ago, each of these cultural traditions encompassing nearly the same length of time;
4. the time-scale of Pleistocene glaciations is greater than 1,000,000 years but the available dates of Pleistocene events place the tool-making hominids earlier than any glaciations as yet dated;
5. the Early Villafranchian Age of Europe is correlative with Late Blancan of North America and Late Villafranchian is probably correlative with Irvingtonian or Early Irvingtonian of North America;
6. the so-called Kafuan (pre-Olduwan) tool-making tradition was probably initiated more than 2,000,000 years ago;
7. the Pseudo-Stillbay and Kenya Stillbay cultures were initiated much earlier than previously imagined (greater than 400,000 years ago for Pseudo-Stillbay and greater than 200,000 years ago for Kenya Stillbay);
8. presently suggested correlations between the African "Pluvials" and European Glaciations are at least in part invalid;
9. the *Kenyapithecus*-bearing beds of Fort Ternan are approximately 14,000,000 years old, and the *Proconsul*-bearing beds of Rusinga Island are no more than 2,000,000 or 3,000,000 years older;
10. the age relationships of the Italian Pliocene and Pleistocene volcanoes can be clearly defined by potassium-argon dating, and we present data related to several of the volcanoes; and
11. rift faulting in the Eastern Rift Valley of Kenya was initiated in the time interval equivalent to the Late Hemphillian Age of North America or the Astian/Plaisancian Age of Europe (i.e., between 2,000,000 and 5,000,000 years ago) with faulting continuing until very recent times.

The data to be presented were obtained on a variety of materials, i.e., biotite, anorthoclase, sanidine, plagioclase, leucite, nepheline, obsidian, and basalt. It appears that the only requirements for usefulness in the time range here considered are: lack of alteration,

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Both Evernden and Curtis held Fellowships in the Miller Institute for Basic Research in Science at the University of California during the period 1958–1962. They have done extensive research in the development and application of the potassium-argon dating method to geological and anthropological problems. Research on the time-scale of the Pleistocene is progressing as rapidly as samples become available. Besides their cooperative work in age dating, Evernden has published papers in seismology, and Curtis has published studies in volcanology.

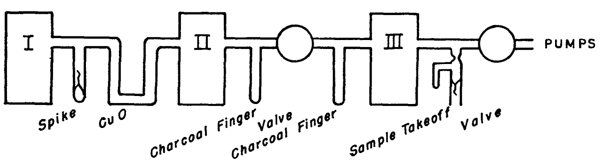
1 This work was supported by the National Science Foundation and the Mary Sprague and Adolph C. Miller Institute for Basic Research. In addition, numerous individuals contributed freely of their time and facilities to assist in the collection of samples. In East Africa, L. S. B. Leakey generously contributed his vehicles and many days of his time to aid in collecting samples at Olduvai Gorge and in the Rift Valley. A. Thompson, B. Baker, and W. Murchison of the Kenya Division of Mines made available the mineral concentration facilities of their laboratory and accompanied us in the field. In Italy, A. C. Blanc toured the regions around Rome with us, augmenting both our sample collection and our understanding of the history of that glorious terrain. G. H. R. von Koenigswald contributed the sample from Java. C. Downie provided the samples from Kilimanjaro. Kenneth Oakley first interested us in the extension of the potassium-argon technique to very young ages in order to date human cultures, and it was he who arranged the first meetings with Blanc and Leakey. To all of these people and many others who have assisted in various ways we are grateful.

grain-size and shape adequate to insure argon retention, and adequate ratios of atmospheric and radiogenic argon to allow precise measurement of the latter.

All ages presented in this paper are based upon $\lambda\beta = 4.72 \times 10^{-10}$ year^{-1} and $\lambda_k = 0.584 \times 10^{-10}$ year^{-1}.

ARGON EXTRACTION PROCEDURE

During the past several years, our extraction procedures have been progressively modified to achieve results of higher and higher precision at ages of less than 1,000,000 years. The argon extraction line used is a high vacuum line equipped with modified Alpert-type metal valves. Use of a sylphon element for the diaphragm has given a valve that survives hundreds of bakeouts and thousands of flexings. The major changes in the extraction procedure have been in mode of (1) attaining fusion of the sample, (2) water and carbon dioxide removal from the gas sample, and (3) gas purification.



PROCEDURE	I (Argon release)	II H_2, H_2O, & CO_2 removal		III N_2,etc., removal
B	fusion in Ni crucible electric heater	liquid air on cold-trap		Ca-vapour
C	fusion in Mo crucible RF heater	"		vacuum-distilled Ca
D	"	"		Titanium foil
		H_2,H_2O	CO_2	
E	"	zeolite dessicant	—	"
F	" (vac-fired crucible)	"	—	"
G	"	"	Ca metal	"
H	"	"	Cu	"

LINE H

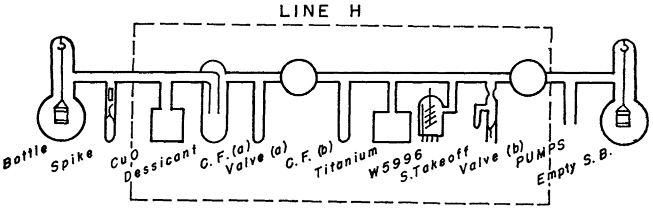


FIG. 1. ARGON EXTRACTION LINE SCHEMATIC

Figure 1 is a schematic diagram of the argon extraction line with details of the various procedures used in the past few years. The lower schematic is the diagram upon which the discussion of run procedure is based. In some of the earliest runs, fusion was achieved by heating a large nickel crucible with a Kanthal external element (Procedure B). The nickel crucible was attached to the high vacuum pyrex line by a stainless steel flange as described by Lipson (1958:137-49). All later procedures included fusion by induction heating of a small molybdenum crucible suspended in a pyrex bottle. The maximum sample size attainable in our crucibles is 15-20 grams of crystal concentrate or 30-35 grams of solid rock. An air blower is used to cool the pyrex bottle. Maximum temperatures reached are 1650° C.

Procedures B, C, and D included water removal by freezing with liquid air. In water-free samples (sanidine, etc.) and in small samples of water-bearing minerals (biotite, hornblende, etc.), available evidence indicates that this procedure has very little or no effect upon the argon measurement. In highly aqueous materials (obsidian, pumice, etc.), the indications are somewhat uncertain. We have no clear demonstrations of the deleterious effect when liquid air has trapped water but do have enough anomalous data to suggest that a change in procedure is desirable. The water is now removed by use of Linde artificial zeolite with 5 angstrom holes (Procedure E, etc.). This procedure has proven to be highly efficient with no known deleterious side effects.

Removal of carbon dioxide by freezing with liquid air creates problems, the major one appearing to be entrapment of argon in the frozen CO_2 with the result that more than 90% of the argon can be so lost. Contrary to a statement in Hay (1963:829-33), isotopic fractionation was not positively detected, though there was a suggestion of its presence. Our primary gas-scavenging technique did not handle CO_2 effectively, so other techniques have been employed which would work into our regular routine most easily. We have found that a clean calcium mirror will effectively scavenge appreciable quantities of CO_2 but the routine use of this method is cumbersome. A simple solution is to put copper metal into the copper oxide furnace (Fig. 1) and to operate this furnace at 850° C. if CO_2 is present in the gas sample resulting from fusion of the sample. This step, in combination with titanium gettering, gives very clean argon samples.

In Procedures B and C, general gas scavenging was achieved by use of calcium metal vapor. Commercially available calcium turnings have a high content of atmospheric argon. In order to eliminate this undesirable contaminant, the calcium was vacuum-distilled onto the pyrex walls of the calcium furnace while the entire line was open to the pumps. Previous flaming of the furnace walls insured that there would be no argon contribution from them when they were subsequently heated in order to move the calcium mirror during gas cleanup. This technique gave blank run values of atmospheric argon of 4×10^{-12} moles but often left the extraneous gas level at 10^{-9} moles. In addition, it was necessary to clean and refill the calcium furnace after every run. We have thus changed to titanium for gettering, using ⅛ of a pound of thin (.001 millimeter) titanium sheet in a stainless steel can. Cleanup is conducted at 850° C. There is no need to keep the titanium under vacuum while reassembling the line. One such can has been used more than 100 times with no loss of efficiency. Extraneous gas is reduced to 10^{-11} moles and blank runs as low as 1×10^{-12} moles of argon have been obtained. Hydrogen is handled by the CuO and dessicant.

Run procedure F (no CO_2 problem) is as follows:
A. Sample preparation, assuming feldspar: rock sample crushed, concentrated to high purity, washed on screens to remove fines (Evernden, *et al.* 1964) treated with HF (see below for discussion of HF treatment), dried, and immediately put into extraction line; sample is under vacuum within 25 minutes of final drying.

B. Line assembly: new spike, sample take-off, and sample bottle blown onto line; a previously vacuum-fired molybdenum crucible is used; line is at air pressure for approximately 20 minutes.
C. Bakeout: entire extraction line other than sample bottle baked out at 300° C. for 8 to 16 hours; sample bottle temperature determined by nature of sample; with coarse-grained feldspar concentrates, a bottle temperature of 450° C. is permissable; this procedure yields pressures of 1-4 $\times$ 10^{-9} millimeters of Hg within 1 hour after cooling of the line.
D. Argon extraction (Figure 1):

1. Charcoal fingers (C. F.), CuO furnace, and titanium furnace outgassed (titanium at 850° C., CuO at 850° C., charcoal fingers at 300° C.), empty molybdenum crucible for use on next run vacuumfired at 1500° C.;
2. Charcoal finger (a) cooled with liquid air, valve (a) closed, sample fused (see discussion below of heating schedule during fusion), spike broken during fusion;
3. Gas sample released from charcoal finger (a), H_2 and H_2O removed by CuO and desiccant;
4. Valve (b) closed, valve (a) opened, charcoal finger (b) chilled with liquid air in order to transfer gas sample through valve (a);
5. Valve (a) closed, gas sample released from charcoal finger (b), N_2, etc., removed by titanium;
6. Titanium cooled to room temperature, sample absorbed on charcoal of sample take-off by chilling with liquid air, sample take-off removed from line.

One man can operate 2 extraction lines, completing 2 argon extractions and 2 re-assemblies within 6-7 hours.

Our standard procedure is to keep the sample molten for 20-30 minutes. Biotite is held at approximately 1250° C. and feldspar at approximately 1550° C. Such heating achieves quantitative argon extraction for small samples of biotite or feldspar, as has been repeatedly confirmed by various cross-checks. Upon heating of larger feldspar samples (15-20 grams) in small crucibles, the highly vesicular nature of K-feldspar slightly above its fusion temperature raised fears of argon retention in the vesicles or insufficiently heated molten sample. Therefore, on all runs after KA 1030, crucible temperature was raised to the maximum attainable with our induction heater (1650° C.) and there held for 15-25 minutes after complete loss of volatiles, such volatiles forming a metallic mirror on the cooler upper portions of the sample bottle. The fused residual in the crucible cools to a dense clear glass. We have no data that suggest greater argon recovery by this new heating schedule. In fact, samples run by both schedules (i.e., heating terminated at highly vesicular state, and heating carried to complete loss of volatiles at 1650° C.) yield essentially the same age. Low potassium feldspars (labradorite, etc.) are fluxed with vacuum-fused pyrex.

EFFICACY OF TREATMENT WITH HYDROFLUORIC ACID (HF)

The main problem in dating very young feldspars is elimination of atmospheric argon from the crystal concentrate used for the fusion run. As we have previously stated (Evernden, Curtis *et al.* 1960), diffusion data show clearly that the atmospheric argon obtained by fusion of a crushed sample of concentrated mineral is not uniformly distributed throughout the crystals, but is concentrated at near-surface sites, thus implying that this argon is entering the crystal from the atmosphere. The implication for sample treatment is thus obvious: Remove the outer portions of the crystals and most of the atmospheric argon will be removed also. We use a 20-30 minute treatment in 7-10% HF (50° C.) as our standard procedure. The efficacy of such a procedure in the reduction of atmospheric argon content of a crystal concentrate is obvious from the data of Table 1. Note that Ar^{at}_{40}(%) in all tables of this paper is computed according to the formula $Ar^{at}_{40}\% = [Ar^{at}_{40} / (Ar^{at}_{40} + Ar^{r}_{40})] \times 100$, Ar^{at}_{40} referring to atmospheric argon 40 and Ar^{r}_{40} referring to radiogenic argon.

The reduction of atmospheric argon is noticeably greater if the treated sample is immediately put into the extraction line rather than being stored for days or weeks.

Another important role of treatment with HF is the removal of adhering grains of the tuff matrix from crystals of a tuff. This adhering material is generally glass or devitrified glass. In the tuffs of Olduvai Gorge, all such material has been devitrified and transformed to clay minerals of various sorts with consequent inability to retain argon generated by potassium decay. A sample of feldspar so coated might be expected to yield too young a computed age. Whether the argon loss was entirely prior to sample collection or whether a significant fraction of the argon of the clay minerals was lost during bakeout procedures is unknown. In any case, quantitative removal of such material is desirable. We believe that runs KA 846 and KA 851 (Leakey, *et al.* 1961: 478) are examples of the errors arising from failure to remove all of such contamination. The tuffs from which these samples were obtained underlie hominid-bearing levels at MK and FLK I sites, Olduvai Gorge. Both tuffs are strongly weathered. Runs KA 846 and 851 were made on crystalline material untreated with HF and with appreciable adhering devitrified glass. The indicated ages were 1,570,000 and 1,640,000 years, respectively, and were anomalous in that they appeared younger than tuffs overlying the hominid sites. HF-treated crystal concentrates from these 2 tuffs have now been run as KA 1082 and KA 1042, both runs yielding indicated ages of 1,920,000 years. Whether these 2 tuffs are slightly contaminated by older debris or whether this is an accurate estimate of the age of the tuffs is another question (see Olduvai discussion p. 351-55). The important point is that by removal of such adhering devitrified glass matrix, anomalously young "ages" were shown to be the result of improper sample preparation, not lack of precision of argon extraction technique and not unknown geological process. Tuffs essentially immediately underlying and overlying the hominid sites thus yield the following corrected ages:

Overlying hominids at sites FLKMM I (KA 1051) and MK (KA 1043) 1,760,000 years.

TABLE 1

Reduction of Atmospheric Argon by Treatment of Feldspars with Hydrofluoric Acid
(Each sample group is composed of untreated [NO] and treated [YES] portions of the same feldspar concentrate).

Sample Number	HF	Weight (grams)	Ar^{40}_{at} (%)	Age (years)	Remarks
Olduvai Gorge					
A–KA 850	NO	8.16	41	1.78×10^6	Anorthoclase from explosive tuff.
KA 1051	YES	9.02	6	1.76	
B–KA 851	NO	7.79	61	1.64	Difference in age explained by removal of devitrified glass by treatment with HF. See text. Anorthoclase from explosive tuff.
KA 1042	YES	9.61	5	1.92	
C–KA 847	NO	5.90	73	1.85	Anorthoclase from explosive tuff.
KA 1043	YES	9.67	7	1.76	
D–KA 924	NO	4.96	85	1.60	Difference in age explained by removal of devitrified glass by treatment with HF. See text. Anorthoclase from explosive tuff.
KA 1179	YES	4.41	17	1.70	
Torre in Pietra					
E–KA 334	NO	20.8	66	0.434	
KA 345	NO	15.2	88	0.438	Sanidine from scoriaceous obsidian bombs.
KA 1185	YES	9.9	17	0.431	
F–KA 408	NO	11.8	47	0.432	Sanidine from scoriaceous obsidian bombs.
KA 1175	YES	2.5	19	0.422	
Malawa Gorge, Kenya					
G–KA 963	NO	12.48	81	0.230	
KA 1086	YES	11.11	43	0.244	Sanidine from explosive tuff.
Epomeo Tuff, Ischia					
H–KA 464	NO	14.3	94	0.072	
KA 1137	YES	12.6	54	0.083	Anorthoclase from pumice ejecta.
Mono Craters, California					
I–KA 269	NO	250	92	0.056	Sanidine from rhyolite flow. These data show clearly the potential of the potassium-argon method when applied to young volcanic feldspars. KA 269 was by Procedure B, all others were by Procedure F.
KA 1197	YES	15	13	0.049	
KA 1197B	YES	1.5	75	0.069	
KA 1197C	YES	0.96	78	0.063	

TABLE 2

Reproducibility of Potassium Determinations

Sample	K (%)	Sample	K (%)	Sample	K (%)
Blank[1]	(.0027)	KA 943	.871	NBS 91	2.713
"0.5 gram"	(.00057)	basalt	.865	opal glass	2.722
	(.00064)	powder	.862	powder	2.721
	(.0013)		.870	dried	2.715
	(.0021)		.868	110° C/24 hours	
	(.0024)		.870	Pyne	6.920
	(.0019)		.867	biotite	6.915
	(.0019)		.868	28/60 mesh	6.921
Quartz	.0037	KA 939	1.119		6.917
16/28 mesh	.0039	basalt	1.121		6.921
	.0032	powder	1.117	B 3203	7.54
	.0045		1.126	biotite	7.57
	.0025		1.111		7.58
	.0025		1.113		7.56
NBS 99	.332		1.113	KA 853	15.80
Na-feldspar	.331		1.120	leucite	15.79
powder	.329		1.118	28/60 mesh	15.81
dried	.325	NBS 98	2.674		15.82
105° C/24 hours	.329	plastic clay	2.658		15.82
		powder	2.669		15.81
		dried	2.664		15.79
		125° C/24 hours	2.662		15.82
					15.81

[1] "K(%)" on blank run data computed as if 0.5 gram sample had been used.

TABLE 3

Samples for Which 2 or More Age Determinations Were Made

Run Number	Sample	Age (years)	Comments
KA 269 KA 1197	Sanidine	56,000 49,000	Satisfactory agreement.
KA 334 KA 345 KA 1185	Sanidine	434,000 438,000 431,000	Satisfactory agreement.
KA 408 KA 1175	Sanidine	432,000 422,000	Satisfactory agreement.
KA 405 KA 405A	Anorthoclase	360,000-520,000 502,000	KA 405 was a very poor run due to spectrometer malfunctioning. KA 405A excellent run by F procedure
KA 412 KA 436 KA 437	Anorthoclase	1.63×10^6 $1.89 \pm .13 \times 10^6$ 1.79×10^6	KA 412 on incompletely cleaned sample (devitrified glass still adhering to feldspars).
KA 413 KA 435	Anorthoclase	0.486×10^6 2.9×10^6	Discordance of results implies presence of contamination.
KA 417 KA 458 KA 459	Anorthoclase	276,000 273,000 97,500	KA 458 and KA 459 disagreement while KA 417 and KA 458 agree is most perplexing. Primary nature of rock argues against contamination and general agreement of KA 417 and KA 458 with KA 1086 suggests that KA 459 is probably the run to be ignored. See appendix. However, proper interpretation of these data is uncertain.
KA 475I KA 475IV	Anorthoclase	3.02×10^6 3.13×10^6	Satisfactory agreement.
KA 664 KA 664R	Biotite	1.03×10^6 1.13×10^6	Satisfactory agreement.
KA 846 KA 919 KA 1082R	Anorthoclase	1.57×10^6 2.36 1.92	KA 846 was sample with adhering devitrified glass. Spread between KA 919 and 1082R supports field evidence of contamination.
KA 851 KA 1042	Anorthoclase	1.64×10^6 1.92	KA 851 was sample with adhering devitrified glass, KA 1042 was sample which had undergone HF leaching. See text.
KA 918 KA 1090	Anorthoclase	2.55×10^6 4.4	Spread in ages evidence of strong contamination.
KA 850 KA 1051	Anorthoclase	1.78×10^6 1.76	Satisfactory agreement.
KA 464 KA 1137	Sanidine	72,000 83,000	Satisfactory agreement.
KA 924A KA 966 KA 1179	Anorthoclase	1.60×10^6 1.60 1.70	No HF treatment of KA 924 and KA 966. KA 1179 treated strongly.
KA 927ABD KA 927E	Basalt w/r	$4.0\text{-}4.4 \times 10^6$ 1.60×10^6	KA 927A, B, and D are invalid due to faulty run procedure. See text. Also, note in text that KA 927E is not an accurate estimate of the age of the basalt, though it is a precise measure of the present argon content of the rock.
KA 933 KA 933A	Basalt w/r	1.76×10^6 1.76	Satisfactory agreement.
KA 963 KA 1086	Anorthoclase	230,000 244,000	Satisfactory agreement.
KA 1055 KA 1079	Anorthoclase	1.66×10^6 1.75×10^6	Samples from tuff horizon interbedded with bentonitic clay lake deposits. Spread in determined ages may be due to contamination.
KA 925 KA 1059	Anorthoclase	1.45×10^6 1.25×10^6	Field evidence indicates contamination in sample. Spread in ages confirms the presence of such contamination.
KA 847 KA 1043	Anorthoclase	1.85×10^6 1.76	Satisfactory agreement. Improved procedures on KA 1043.
KA 849 KA 1036	Anorthoclase	1.89×10^6 2.03×10^6	Field evidence indicates contamination.
KA 1039 KA 1180		1.74×10^6 1.85×10^6	Re-run by improved procedures (HF) to remove adhering devitrified glass.

Underlying hominids at sites FLK I (KA 1042) and MK (KA 1082) 1,920,000 years.

See discussion of samples from Olduvai Gorge for evaluation and relationships of all samples.

REPRODUCIBILITY OF K DETERMINATIONS

When determining the age of very young rocks, the precision of the potassium determination is comparatively unimportant, as the greatest uncertainty lies in the argon determination. However, if the Ar^{at}_{40} % can be reduced to 75% or less, the potassium determination may have the lower precision. The tabulation below indicates the degree of reproducibility which we routinely attain, using a Perkin-Elmer flame-photometer with propane flame, accurate buffering of standards, and interpolation between close standards. Sample preparation is most important in the obtaining of such reproducibility, uniformity of sample being necessary. Crystal concentrates are used at the same size range for both potassium and argon determinations. Powdering of the splits used for potassium determination is avoided, as an erroneous idea of sample uniformity is thus developed. For samples which are run on a whole-rock basis, slices from either end of the piece used for argon determination are crushed and run separately for potassium.

The degree of interlaboratory agreement attainable can be gauged by the value for B 3203, our value of 7.56 ± .01% for 4 determinations being in essential agreement with the accepted value for this biotite. All of the K values for standards of the National Bureau of Standards are within the accepted ranges, though the ranges given in the literature are quite wide. Other interlaboratory checks which we have conducted indicate that competently done potassium analyses will agree to within less than 2%, often to within less than 1%.

REPRODUCIBILITY OF Ar DETERMINATIONS

There is little need to document further the reproducibility and internal consistency of argon determinations. Data published by ourselves and others have indicated the high precision of such measurements; a few data are presented here for completeness.

1. On 5 recent runs on biotite B 3203 (used as relative checks on our last spike set), the argon determination had a standard deviation of 0.27% and a standard error of 0.13%.

2. Sized fractions of a biotite sample (from the Cathedral Peak quartz monzonite of Tuolomne Meadows, Yosemite National Park) yielded the following data:

Run Number	Tyler Mesh Size	% K	Age (10^6 years)
KA 476 I	— 28 + 48	5.76	82.9
KA 476 II	— 48 + 65	5.42	83.2
KA 476 III	— 65 + 100	5.07	82.5
KA 476 IV	— 100 + 150	4.37	82.9

The computed ages (an overall check of both argon and potassium determinations) have a standard deviation of 0.35%.

3. Sanidine, biotite, and glass from the same tuff (Norwood Tuff, Morgan County, Utah):

Run Number	Mineral	Age (10^6 years)
KA 825	sanidine	37.4
KA 826	biotite	37.5
KA 827	glass	36.0

The lower age on the glass may be due to small argon loss from the glass.

4. "Tuff with black pumices," sanidine crystals in obsidian matrix. Single volcanic bed dated at several sites 1 or more times:

	Procedure	Weight (grams)	Ar^{at}_{40}(%)	Age
Site A				
KA 334	B	20.8	66	434,000 years
KA 345	B	15.2	88	438,000
KA 1185	F	9.9	17	431,000
Site B				
KA 408	B	11.8	47	432,000
KA 1175	F	2.5	19	422,000
Site C				
KA 407	B	11.0	56	417,000
Site D				
KA 304	B	24.5	32	431,000

These data show clearly that the correction for atmospheric argon is properly done in terms of the isotopic composition of atmospheric argon of today.

5. Single volcanic bed dated at several sites. Anorthoclase crystals in *nueé ardente:*

KA 1058	1.76 × 10^6 years
KA 1057	1.75
KA 1053	1.76
KA 1043	1.76
KA 1179	1.70
KA 437	1.79

6. The tabulation given as Table 4 of Evernden, *et al.*, (1964) indicates the essential correspondence between the relative geochronology of fossil vertebrates and the radiometric geochronology of potassium-argon dates throughout the entire Tertiary Period. Figure 1 of the above-mentioned paper is included in this paper as Figure 2; the time-scale of the Tertiary there presented is relevant to later discussions.

POTENTIAL LIMITS OF K-Ar DATING

With the introduction of treatment of feldspar with HF, the attainment of high precision measurements of radiogenic argon on small samples of young crystals is now possible. There is now real hope that the precision and accuracy of potassium-argon ages on high potassium feldspars in the 30,000-year age range will be superior to that of radio-carbon in the same age range. If we use a mean value of atmospheric argon per gram attainable by optimum leaching procedures of 5×10^{-13} moles per gram [see Table 1 and note that in sample I (KA 1197), Ar^{at}_{40} was reduced to 1×10^{-13} moles per gram], 20 grams of a sanidine of 10% potassium and 30,000 years age would give

0.75 × 10^{-11} moles of radiogenic argon and 1 × 10^{-11} moles of atmospheric argon. With careful mass spectrometer operation and proper spike material, the precision of the radiogenic argon determination under these conditions would have a standard deviation of less than 5%. Therefore, assuming the availability of datable material, the potassium-argon technique can now yield high percision ages throughout the entire Pleistocene and can meet radio-carbon ages. No portion of geological time is now outside the reach of one or more radioactive dating techniques.

The data of sample I of Table 1 are of particular interest, as the KA 1197 series is one in which we attempted to optimize the sample preparation and argon extraction procedures for a very young sample. KA 269 was run by procedure B on a 250-gram sample, the KA 1197 series was done by procedure F on 15, 1.4, and 0.96 grams. The gross differences in sample sizes and isotope ratios in the several runs show that the internal consistency of the computed ages is the result of a reproducible analytical technique. The last 2 runs push our present calibration data to the limit. Improvements are still possible and are in process of being made. As they stand, however, these data show that, on 20 grams of high potassium feldspar, our present preparation and extraction procedures will date 2500-year old samples with a precision of 25%.

REPEATED RUNS

Table 3 presents the data on samples of this paper which, in 1 condition or another, were run 2 or more times. Assuming proper run procedures, factors leading to discordant results are presence of contamination and improper sample preparation, both factors being of significance only in loose tuffs. Incomplete removal of devitrified glass explains KA 412 versus KA 437, KA 846 versus KA 919 and KA 1082R, KA 918 versus KA 1090, KA 924A versus KA 1179, and KA 1039 versus KA 1180. Presence of contaminating older anorthoclase explains KA 413 versus KA 435, KA 919 versus KA 1082R, KA 918 versus KA 1090, KA 925 versus KA 1059, and KA 849 versus KA 1036, virtually all of these discordances being predictable by field or microscopic criteria listed below (see discussion of Olduvai Gorge samples). Except for KA 459 and KA 927 series, all repeat runs on properly prepared, uncontaminated samples gave satisfactory agreement. We discuss below the explanation of the gross discordance in the KA 927 series.

TIME OF RIFT FAULTING IN EAST AFRICA

Historical accident has resulted in varying definitions of the boundaries of Tertiary Epochs and thus in divergent usages of Epoch terms. Such divergent usage has contributed greatly to misunderstandings and miscorrelations. We suggest that such terms should not be used in intercontinental correlations if it is possible to express the correlations in terms of faunal Stage-Ages. Fortunately, the faunally recognized Stage-Ages are still reasonably objective units and thus have the potential of being more precisely correlated throughout the world than do the classic Epochs. In addition, they permit a more refined subdivision of the Tertiary Period. Of course, it remains uncertain that valid palaeontologic criteria exist for extending European Stage-Ages to tropical Africa; however, the

precision and internal consistency of radiometric dates obtained during the past few years make it certain that, where suitable material is available, problems of intracontinental and intercontinental palaeontologic correlation can be resolved by such dates. Radiometric ages provide secure correlation data if they have been obtained on satisfactory material. Finally, note that it is invalid to speak of the Sarmatian or Barstovian of East Africa, because Stage-Ages should be defined on the basis of local sequences, but the phrase "equivalent in time to the Barstovian Age of North America" can and should be used. It is more cumbersome but it is more meaningful and more precise than "Upper Miocene."

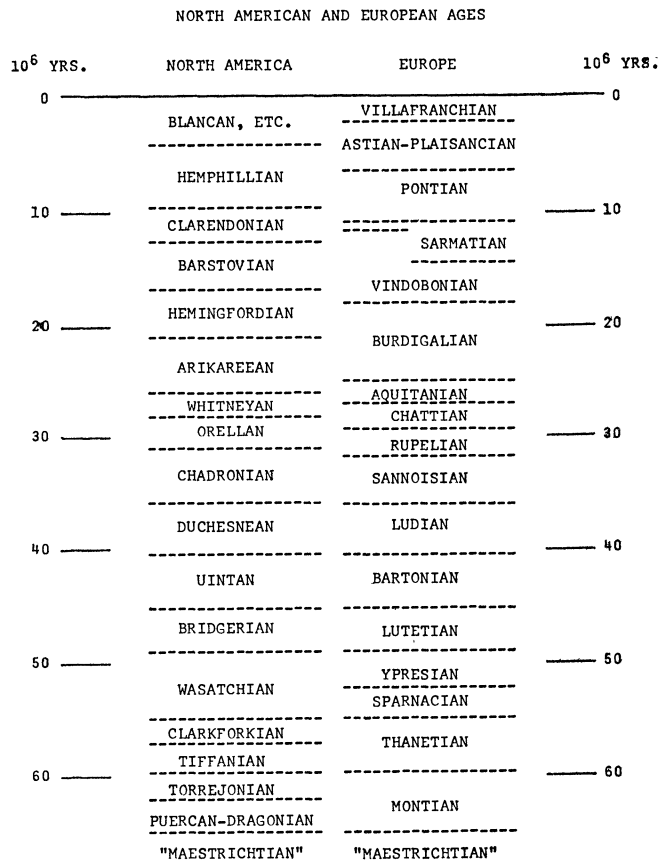


FIG. 2. CORRELATION AND TIME-PLACEMENT DIAGRAM

Figure 2 gives present estimates of the Stage-Age correlations between the indicated European Ages and the North American Land-Mammal Ages, the figure being based upon palaeontologic and geochronometric controls (Evernden, *et al.* 1964).

With the framework of reference established, we can discuss the potassium-argon ages obtained on African volcanic rocks related to the history of rift faulting.

Field relationships, as described by Binge (1962), Shackleton (1945, 1946, 1951:345-89), and Murchison (personal communication) indicate the following relationships between early phases of the Late Cenozoic volcanism of Kenya at the indicated localities:

The Yatta phonolite initiates the volcanic episode near Garrish, Kenya, and is thought to be a time equivalent of the Kericho-Kapiti phonolites. The tuffs

Locality	Garrish	Western Nyanza	Kericho Area	Nairobi-Magadi
Stratigraphy	Yatta Phonolite		Kericho Phonolites (Fort Ternan Tuffs)	Ol Orgesailie Phonolite Athi Phonolite Kapiti Phonolite
		Rusinga Island Agglomerates and Tuffaceous Sediments	Coarse Agglomerates Koru Beds	
		Precambrian	Precambrian	Precambrian

of Fort Ternan are interbedded with the Kericho phonolites. All of these rocks, including the Athi and Ol Orgesailie Phonolites (the latter rock having nothing genetically to do with the volcano of the same name), are thought to be pre-rift faulting, Binge (1962) having expressed the opinion that rifting was probably initiated shortly after eruption of the Kericho phonolites. It is customary to classify the Rusinga Beds as "Lower Miocene" based largely upon the extensive vertebrate fauna there found (Clark and Leakey 1951: 1-117; see Evernden, *et al.* 1964 for Rusinga bibliography). Such an age assignment has been disputed, a discussion of this point having been presented by us in the reference cited. The Fort Ternan tuffs (KA 429) have yielded an extensive mammalian fauna which Leakey (personal communication) assigns "a latest Miocene or Early Pontian" age.

Potassium-argon dates obtained on these rocks are as follows:

Run Number	Stratal Unit	K-Ar Age
KA 663	Athi Phonolite	5.2×10^6 years
KA 653	Ol Orgesailie Phonolite	5.8×10^6 years
KA 1054	Yatta Phonolite	13.2×10^6 years
KA 651	Kapiti Phonolite	13.4×10^6 years
KA 427	Fort Ternan Tuffs (=Kericho Phonolite)	14.0×10^6 years
KA 336	Kiahera Series, Rusinga Island	$15.2 \pm 1.5 \times 10^6$ years

The K-Ar dates are in the proper order as closely as known, and so satisfy the demonstrable critical criteria of relative age. The K-Ar age for the tuff of Fort Ternan (equivalent in time to the Sarmatian Age of Europe or Late Barstovian Age of North America) is in excellent agreement with Leakey's suggested placement. However, agreement with previous time-placement of some events is poor. As noted above, the fauna of Rusinga Island is such as to have caused wide disagreement about its proper time-placement. As we have discussed this point previously, we shall limit ourselves to stating that critical evaluation of the Rusinga fauna indicates that it may well be equivalent in time to the Vindobonian of Europe or Early Barstovian of North America. An assignment to the "Lower Miocene" is not required by the fossil data, the confusion about proper placement probably being due to late survivals (relict fauna) in equatorial Africa. Owing to the high standard deviation of the KA 336, the Rusinga fauna may be approximately 3,000,000 years older than the Fort Ternan fauna, i.e., about the time equivalent to an average Land-Mammal Age of North America, but placement in time equivalent to the Burdigalian Age of Europe seems out of the question.

Another point of some surprise is the extended interval of time between extrusion of the Kapiti Phonolite and the Athi-Ol Orgesailie Phonolites. Binge (1962) has suggested that extrusion of the Kericho Phonolites may have only slightly preceded the initiation of rift faulting, the 2 events being genetically related. The K-Ar dates suggest, however, that rift faulting was not initiated, at least in the Nairobi area, until millions of years after the Kapiti-Kericho phonolites. In fact, KA 653 and KA 663 indicate that rift faulting in the Eastern Rift was initiated less than 5,000,000 years ago (i.e., at a time equivalent to the late Hemphillian of North America, or Astian/Plaisancian of Europe).

The Limuru trachyte (KA 654) post-dates initiation of rift faulting as does the Plv_2 (KA 650 and KA 661) series (Baker 1958). After the Plv_3 series (KA 652), there occurred 1 of the major periods of rift faulting, subsequently succeeded by the accumulation of the Ol Orgesailie lake beds and further minor faulting (Baker and Murchison, personal communications). The Limuru trachyte is thought to be older than the Plv_2 series though field relationships do not demonstrate superposition. Mount Kenya (KA 475 and KA 657) is older than the Limuru trachyte (Shackleton 1945).

The K-Ar ages, in stratigraphic order where established, are as follows:

Run Number	Stratal Unit	K-Ar Age
KA 652	Plv_3 series	2.4×10^6 years
KA 661	Plv_2 series, younger	0.802
KA 650	Plv_2 series, older	1.74
KA 654	Limuru trachyte	1.72
KA 657	Syenite plug of Mount Kenya	2.64
KA 475	Kenyte from high on Mount Kenya	3.1

The Plv_3 date is grossly divergent from described field relationships (Baker 1958). Otherwise, there is satisfactory agreement between K-Ar ages and field data. Acheulian cultures in the Ol Orgesailie lake bed sequence which overlies Plv_2 and Plv_3 suggest an age of approximately 400,000 years for portions of these strata (see discussion of "Time-Scale of Human Cultural Evolution").

It is concluded that rift faulting was initiated in the Eastern Rift Valley between 2,000,000 and 5,000,000 years ago, occurred on a major scale between 800,000 and 400,000 years ago, and has continued on a minor scale until the present. A further clarification and refinement of the history of rift faulting in Africa must await additional sampling, dating, and mapping.

K-Ar DATES FROM OLDUVAI GORGE

An extensive program of dating of anorthoclase and plagioclase-bearing tuffs at Olduvai Gorge, Tanganyika has now been completed. While little new has been found since our first published notes, the importance of the date of 1,750,000 years for the age of *Zinjanthropus* and associated mammalian fauna to the fields of anthropology, palaeontology and geology demanded that it be checked as thoroughly as possible.

There are several primary tuff deposits in the Bed I sequence at Olduvai, only 1 or 2 in Bed II, and none in higher beds. Our program of dating has included all primary tuffs recognized plus several units of obviously contaminated deposits and several of questionable character. These were done to assist in an evaluation of factors that might invalidate determined potassium-argon ratios as valid data for estimating times of deposition. The results obtained have suggested the following set of criteria for selecting meaningful ages from a sequence of samples:

Accept dates if —

1. Two different crystal types from the same tuff yield essentially the same age;
2. sample is a volcanic flow (basalt, trachyte, etc.) and the problem of argon loss is not insurmountable (Evernden, *et al.* 1964; Dalrymple 1963). At the worst, a minimum age is obtained;
3. several samples from the same horizon at scattered localities yield the same age;
4. the unequivocally primary nature of a tuff can be established. Such a nature is to be established on both field and laboratory evaluation with criteria used to establish primary nature of a tuff being:

a. throughout its thickness, the tuff is composed of a heterogeneous mixture of various sizes of particles of eruptive type (the heterogeneous mixture of crystals and pumice may show a progressive gradation to finer sizes towards the top of the tuff),

b. a total absence of mineral grains derived from pre-eruptive rocks of the area (at Olduvai, this would mean total absence of such minerals as quartz, combined quartz-feldspar fragments, hornblende),

c. absence of cross-bedding,

d. a thickening of tuff units towards their volcanic source (at Olduvai such thickening should be towards Ngorongoro),

e. uniform increase in particle size towards the source.

Reject dates if —

1. any possibility of deposit being reworked;
2. any possibility of admixed detrital component of different age;
3. different concentrates of the same mineral from the same tuff (assuming proper sample preparation procedures) yield markedly different ages. Under 1 and 2 above, criteria establishing unsatisfactory nature of the tuff are:

a. presence of non-euhedral quartz grains,

b. abrasive rounding of tuff components,

c. well-developed stratification of tuff unit,

d. detrital components of any type (tools, teeth and bones, lapilli, etc.).

Laboratory criteria are simply more careful evaluation of quartz content, other detrital elements, and rounding of grains.

With these criteria as guide, the Olduvai samples dated are grouped as follows:

Rejected—KA 846 (KA 919, KA 1082), KA 848, KA 849 (KA 1036), KA 851 (KA 1042), KA 1033, KA 1038, KA 1041, KA 1044, KA 1048, KA 1052, KA 1055, KA 1085 (KA 1295), and KA 1087.

TABLE 4

AFRICAN VOLCANIC ROCKS NOT RELATED TO HUMAN OR TOOL-BEARING HORIZONS

Sample Number	Mineral	Procedure	Weight (grams)	K (%)	Ar^{40}_{at} (%)	Age (10^6 years)	Remarks
KA 336	Biotite	C	1.73	2.72 ± 0.26	88	14.6 ± 1.4	Rusinga Island, Kiahera Series
KA 336R	Biotite	C	1.43	2.72 ± 0.26	62	15.9 ± 1.5	Rusinga Island, Kiahera Series
KA 427	Biotite	C	1.36	7.67	52	14.0	Fort Ternan Tuff
KA 475	Anorthoclase	C	6.75	4.04	23	3.13	Mount Kenya, flow
KA 650	Anorthoclase	C	10.34	5.26	61	1.74	Plv_2 Series of Baker (1958)
KA 651	Nepheline	C	7.87	4.98	28	13.4	Kapiti Phonolite
KA 652	Anorthoclase	C	8.99	2.85	55	2.4	Plv_3 Series of Baker (1958)
KA 653	Nepheline	C	4.35	3.25	62	5.8	Ol Orgesailie Phonolite
KA 654	Anorthoclase	C	8.13	5.27	37	1.72	Limuru Trachyte
KA 657	Anorthoclase	C	7.92	4.53	36	2.64	Mount Kenya, plug
KA 661	Anorthoclase	C	8.20	5.16	93	0.802	Plv_2 Series of Baker (1958)
KA 662	Anorthoclase	C	4.05	4.92	76	2.7	Ol Orgesailie Trachyte
KA 663	Anorthoclase	C	3.39	4.75	41	5.2 ± 0.3	Athi Phonolite
KA 937	Obsidian	E	15.71	4.86	91	0.365	Kilimanjaro, Main Rhombporphyry Series
KA 940R	Basalt w/r	F	21.62	1.65	89	0.514	Kilimanjaro, Mawenzi Trachy-basalt
KA 947R	Basalt w/r	F	20.46	4.03	84	0.463	Kilimanjaro, Lavaturm Phonolite Series
KA 1054	Anorthoclase	F	8.12	5.31	1	13.2	Yatta Phonolite
KA 1060	Trachyte w/r	F	16.7	3.41	89	6.7	Ol Esayeiti Phonolite

Questionable—KA 1084 (Anomalous age but did not detect evidence of contamination).

Accepted—all others.

Note that there are several runs in Table 5 which are indicated to be invalid but which do not appear in the list above. These are runs in which improved procedures resulted in internally consistent data on samples for which all rejection criteria were negative. Such runs are KA 412 (replaced by KA 437), KA 924, etc. (replaced by KA 1179), KA 405 (replaced by KA 405A), KA 847 (replaced by KA 1043), KA 1039 (replaced by KA 1180), and KA 850 (replaced by KA 1051).

The bases of rejection of the several samples listed above are as follows:

KA 846	Detrital material in sample (bone fragments, quartz, etc.).
KA 848	Detrital material in sample (bone fragments, etc.).
KA 849	Stratification indicating reworking
KA 851	Reworked nature of deposit.
KA 1033 KA 1038 KA 1055 KA 1087	Disagreement of dates obtained on this series of samples and interbedding with lake deposits, suggesting redeposited nature of these thin tuff horizons.
KA 1042	Detrital material in sample (bone fragments, etc.).
KA 1044	Lapilli in tuff.
KA 1048	Rounded quartz grains in crystal concentrate.
KA 1052	Rounded nature of pumice fragments.
KA 1085 KA 1295	Stratification indicating reworking.

The accepted samples, arranged in stratigraphic order from youngest to oldest are presented as Table 6.

Some discussion of the basalt date is in order in light of our earlier estimates of 4,000,000-4,400,000 years for the age of KA 927 (Curtis and Evernden 1962:610-12). It must be admitted that on occasions problems not previously met arise to confound the experimenter. An example in point is KA 927. This basalt sample has appreciable disseminated carbonate of primary origin. When the first runs were made on bulk samples on KA 927, our only technique for handling CO_2 effectively was by freezing with liquid air. As much of the argon becomes entrapped in the frozen CO_2, fears of possible argon fractionation arise. On 2 of the samples previously reported, 90-95% of the argon was lost in that manner, thus resulting in an abnormally small argon sample for isotopic analysis. In the third sample, a major portion of the argon sample was introduced into the mass spectrometer, but it was strongly contaminated with CO_2. We realized that all runs were not of the highest quality, but the apparent uniformity of results suggested that a reasonably good estimate of the age of the basalt had been obtained. Note that the isotopic ratios in these runs were such that slight increases in the normal uncertainties of these ratios sufficed to more than double the computed age. Later results suggest that our previously published estimate of the age of the basalt is in gross error. KA 927E and KA 1100 have been done by Procedures E and F, KA 927E on the whole rock but by a technique capable of removing CO_2 chemically (cold calcium mirror), and KA 1100 on crushed and lightly leached basalt (acetic acid to remove carbonate and 7% HF for 25 minutes to remove chloritic fine-grained material). This process yields a virtually pure mixture of pyroxene and plagioclase. KA 927E yielded an indicated age of 1.60×10^6 years, while KA 1100 gave 1,920,000 years.

To quote from our previous note relative to the description of KA 927: "Most of the margins of the titanaugite are chloritized, and chlorite or a serpentine mineral replaces the margins and cleavage cracks in most of the olivine. Even some plagioclase grains are extensively chloritized." We would thus expect incomplete argon retention, and the difference in computed age of KA 927E and KA 1100 we interpret to signify argon loss from the whole rock, while unaltered crystals still retain virtually all, if not all, of the argon generated within them. Our earlier note also commented on the extremely fine-grain size of KA 933, this fineness of grain being thought to again explain the computed age differential between KA 933 (1.76×10^6 years) and KA 1100 (i.e., argon loss from KA 933 due to very small radius of diffusion for effective argon escape from rock). KA 1100 is thus thought to provide the best estimate of the age of the basalt at Olduvai Gorge.

KA 664 was reported previously as being from a tuff unit in the top of Bed I. Later fieldwork by R. Hay (personal communication) clearly shows that there are no biotite—laden tuffs in Bed I and that the only tuffs carrying appreciable biotite are in Lower to Middle Bed II. Inadequate knowledge of the stratigraphy led to faulty stratigraphic placement of KA 664. This example illustrates that impatience to obtain dates should not lead to dating of inadequately or uncertainly controlled samples.

Stratigraphic terms used in this discussion are those of R. Hay as presented in *Science* (Hay 1963: 829-33). Conclusions drawn by Hay are pertinent to discussion of potassium-argon dates at Olduvai. Previously expressed opinions of the depositional history at Olduvai were based upon minimum fieldwork with no careful effort to trace key horizons in the field. Hay has shown that Bed I is an accumulation of tuffs from one volcano (Ngorongoro), that these tuffs were probably accumulated over a geologically short interval of time, that Bed II is largely composed of alluvial or fluvial detrital deposits at the margin of a lake, that these deposits might well have accumulated over an appreciable interval of time, and that there is probably no disconformity between Beds I and II, simply a cessation of accumulation of tuffs due to cessation of activity of Ngorongoro.

The potassium-argon dates indicate that the portions of Bed I between the basalt and Marker Bed A accumulated during approximately 350,000 years, that the upper half or so of Bed II took 600,000 years to accumulate, and that only 400,000 years elapsed during the interval represented by the upper part of Bed I and the lower portion of Bed II. Thus the potassium-argon dates support Hay's conclusions (1963) relative to the long time covered by the deposits of Bed II and the absence of an appreciable time-break between Bed I and Bed II. It is important to remember that the original definitions of Bed I and Bed II were in the eastern part of the Gorge (i.e., site FLK — Second Fault area). As all of the dates here presented are in this area, the discussion above applies strictly only to the strata there exposed. Application of the stratigraphic terms and potassium-argon dates

TABLE 5

OLDUVAI GORGE

(All ages enclosed in brackets are invalid estimates of the absolute age of the sample for reasons discussed in the text.)

SAMPLE	MINERAL	PROCEDURE	WEIGHT (GRAMS)	K %	Ar$^{at}_{40}$ (%)	AGE (10^6 YEARS)	HF	REPEAT RUNS
KA 405	Anorthoclase	B (1120° C/5 hours)	21.26	5.20	90	(0.45 ± 0.08)	NO	KA 405A
KA 405A	Anorthoclase	F	10.56	5.01	70	0.502	NO	KA 405
KA 412	Anorthoclase	B (1170° C/10 hours)	29.6	3.29	67	(1.63)	NO	KA 436
KA 436	Anorthoclase	C	10.53	3.21	76	(1.89 ± 0.13)	NO	KA 412
KA 437	Anorthoclase	C	6.67	3.06	70	1.79	NO	KA 412
KA 664	Biotite	C	1.39	6.96	93	1.03	NO	KA 664R
KA 664R	Biotite	C	5.98	6.96	82	1.13	NO	KA 664
KA 846	Anorthoclase	C	7.91	2.52	52	(1.57)	NO	KA 919, 1082
KA 847	Anorthoclase	C	5.90	3.18	73	(1.85)	NO	KA 1043
KA 848	Anorthoclase	C	9.96	2.85	57	(5.4)	NO	
KA 849	Anorthoclase	C	6.50	2.04	77	(1.89)	NO	KA 1036
KA 850	Anorthoclase	C	8.16	3.94	41	1.78	NO	KA 1051
KA 851	Anorthoclase	C	7.79	3.69	61	(1.64)	NO	KA 1042
KA 919	Anorthoclase	C	15.04	2.82	36	(2.36)	NO	KA 846, 1082
KA 924A	Anorthoclase	C	4.96	3.17	85	(1.60)	NO	KA 966, 1081, 1179
KA 927E	Basalt w/r	F	4.11	0.876	93	(1.60)	NO	
KA 933	Basalt w/r	C	21.16	1.47	90	(1.76)	NO	
KA 966	Anorthoclase	D	13.78	3.47	28	(1.60)	YES	KA 924, 1081, 1179
KA 1033	Anorth/Plag	E	10.06	1.43	64	(2.00)	NO	
KA 1036	Anorthoclase	E	8.56	2.17	14	(2.03)	YES	
KA 1037	Anorth/Plag	F	9.85	1.41	17	1.79	YES	
KA 1038	Plagioclase	F	8.91	0.814	26	(2.18)	YES	
KA 1039	Anorthoclase	F	11.05	3.37	10	(1.74)	YES	KA 1180
KA 1040	Anorthoclase	F	7.76	2.56	9	1.65	YES	
KA 1041	Anorthoclase	F	10.18	3.25	17	(1.85)	YES	
KA 1042	Anorthoclase	F	9.61	4.00	5	(1.92)	YES	KA 851
KA 1043	Anorthoclase	F	9.67	3.43	7	1.76	YES	
KA 1044	Anorthoclase	F	8.09	2.57	38	(1.84)	YES	
KA 1045	Plagioclase	F	9.33	0.717	26	1.64 ± 0.06	YES	
KA 1046	Plagioclase	F	8.54	0.682	56	(1.86 ± 0.06)	YES	
KA 1047	Plagioclase	F	9.16	0.670	36	1.86 ± 0.06	YES	
KA 1048	Anorthoclase	F	8.38	1.99	18	(2.77)	YES	
KA 1050	Anorthoclase	F	10.39	0.924	37	1.66	YES	
KA 1051	Anorthoclase	F	9.02	4.20	6	1.76	YES	KA 850
KA 1052	Anorthoclase	F	9.03	4.30	4	(1.89)	YES	
KA 1053	Anorthoclase	F	8.07	3.24	23	1.76	YES	
KA 1055	Anorthoclase	F	9.17	3.63	46	(1.66)	YES	
KA 1057	Anorthoclase	F	8.71	3.28	16	1.75	YES	
KA 1058	Anorthoclase	F	9.63	3.56	11	1.76	YES	
KA 1062	Plagioclase	F	9.16	0.837	49	1.57 ± 0.06	YES	
KA 1079	Anorthoclase	F	4.48	3.35	24	(1.75)	YES	KA 1055
KA 1080	Anorthoclase	F	2.50	3.96	28	1.85	YES	
KA 1082	Anorthoclase	F	11.46	3.05	8	(1.92)	YES	
KA 1084	Anorthoclase	F	6.98	2.23	30	(2.69)	YES	KA 846, 919
KA 1085	Anorthoclase	F	10.06	2.26	21	(1.75)	YES	
KA 1087	Anorthoclase	F	6.46	1.46	58	(1.55)	YES	
KA 1088	Plagioclase	F	3.95	2.51	60	1.91	YES	
KA 1100	Plag/Augite	F	9.37	0.967	70	1.92	YES	
KA 1170	Anorthoclase	F	7.70	5.02	4	5.01	YES	
KA 1179	Anorthoclase	F	4.41	3.30	17	1.70	YES	KA 924
KA 1180	Anorthoclase	F	7.86	3.25	9	1.85	YES	KA 1039, stronger HF treatment

of the east to the western portions of the Gorge depends upon probable but not positive stratigraphic correlations.

We must consider the geological factors that might invalidate the potassium-argon ages for the tuffs of Olduvai Gorge.

1. POSSIBILITY OF BASEMENT CONTAMINATION

As these tuffs were erupted through and lie upon a Precambrian terrain, the possibility of old mineral grains in the concentrates must be considered. Derived tuffs are present in the Olduvai section and were avoided. Primary tuffs were recognized by the criteria listed above. Several tuffs bury living sites and hence were deposited directly from the air on to dry ground. The presence of soil horizons on the tops of many of the tuffs in the area of the buried living sites supports this statement. Samples were not only examined for contamination at the time of collection but also during the various stages of mineral concentration. The uniformity of the age determinations also argues against basement contamination. Basement rocks at Olduvai lie within a belt of metamorphic rocks which have been dated as older than 2,250,000,000 years. Simple calculations indicate that, at the grain sizes used, a 1-grain difference in contamination in the several mineral concentrates, if the contaminant be considered to be high K feldspar, would cause an age scatter of approximately 400,000 years, i.e., greater than observed. Such uniformity of contaminant would seem

TABLE 6

AGES FOR SAMPLES FROM OLDUVAI GORGE WHICH SATISFY GEOLOGIC CRITERIA

RUN NUMBER	STRATIGRAPHIC POSITION	AGE (10^6 YEARS)
KA 405	Overlies Chellean II, underlies Chellean-Acheulean	0.50
KA 664	Middle or Lower Bed II	1.1
KA 1062	Between marker beds of "Marker Bed A"	1.57 ± .05
KA 1050	Approximately same stratigraphic level as "Marker Bed A"	1.66 ± .05
KA 1046	Just beneath base of "Marker Bed A"	1.86 ± .06
KA 1045	15 inches below base of "Marker Bed A"	1.64 ± .06
KA 1040	65 inches above *Zinjanthropus* floor	1.65
KA 1037	50 inches above *Zinjanthropus* floor, no correlation with KA 1058	1.79
KA 1058	*Nueé ardente* overlying hominid site at DK site. Samples scattered throughout mile of outcrop	1.76
1057		1.75
1053		1.76
1043		1.76
1179		1.70
KA 1047	12 inches above *Zinjanthropus* floor	1.86 ± .06
KA 850	1 inches above *Zinjanthropus* floor	1.78
KA 1180	81 inches below *Zinjanthropus* floor	1.85
KA 1100	Basalt	1.92 ± .06
KA 1080	Few feet below basalt — Anorthoclase	1.85
KA 1088	Plagioclase	1.91 ± .04

highly unlikely. High K feldspar is the only type of basement mineral that would be possible in the final anorthoclase concentrates. Such lack of minerals derived from the Precambrian rocks also disposes of the possibility of contamination of the tuffs by material torn from the walls of the explosion conduit.

2. POSSIBILITY OF PRE-ERUPTION ARGON IN VOLCANIC ANORTHOCLASE

Is it possible to retain argon within a feldspar lattice at the temperatures of extrusion of such tuff deposits? If this is possible, then a complicated history of anorthoclase genesis within the melt at times long preceding the moment of eruption could be proposed to cast doubt on the significance of the tuff ages. If the partial pressure of argon in the magma were essentially zero during this time of proposed anorthoclase growth, available data on the diffusion of argon in feldspar lattices are adequate to deny the possibility of argon retention within the lattice for geologically significant periods. If the temperature in the magma were no higher than 800° C., the maximum period of retention of an argon atom within a feldspar crystal would be 4-6 months. Higher temperatures would greatly shorten this time (Evernden, Curtis, *et al.* 1960:583-604). This time estimate presumes that anorthoclase has diffusion characteristics similar to sanidine, whereas available data suggest greater argon loss rates for anorthoclase-like feldspar than for high-K sanidines (Evernden and Richards 1962:1-50). If, however, a pre-eruption environment of high partial pressure of argon is proposed, argon retention within the feldspar lattice becomes theoretically possible. That this happens rarely, and that it is extremely unlikely as an explanation for the tuff ages at Olduvai, can be easily argued. We have dated several geologically young samples whose extreme youth we affirmed in the field. All of these have yielded very young ages [young lava flow damming Lake Naivasha, Kenya, 28,000 years (KA 920); post-last glacial phase eruption of Mono Craters, California, 5,000 years (KA 329); A.D. 1304 lava flow on Ischia, Bay of Naples, Italy, 0 years (KA 403); 1912 eruption at Mount Katmai of highly gas-charged tuff, 0 years; 1955 eruption on New Guinea, 0 years (KA 1104)]. Demonstration of close agreement between potassium-argon dates and the stratigraphy of Olduvai Gorge (Table 6) argues very strongly against a high argon pressure in the pre-eruption environment. Such concordance of data would not seem a likely result of such a proposed history.

We have dated only 1 sample that has yielded a potassium-argon age far in excess of its assigned geological age. A sanidine concentrate sent to us from Germany is purported to come from a sanidine-bearing phonolite of Alleröd age, i.e., approximately 10,000 years old. Repeat runs on this material yielded ages of several hundred thousand years which varied by as much as 200%. Such results almost certainly indicate the presence of contaminating old material.

3. TUFF AGES AND AGE OF UNDERLYING BASALT

It has been suggested that a potassium-argon age of 1.3×10^6 years obtained by Lippolt on a basalt sample from underneath Bed I indicates that Bed I must be younger than 1,300,000 years (Koenigswald, *et al.* 1961:720-21). The basalt sample used by Gentner and Lippolt was in large part composed of what had once been a glassy ground mass but which had been thoroughly devitrified and altered to pelagonite (Gentner, personal communication). Numerous basalts and glasses have been dated at our laboratory. It is clear that devitrification of glass develops a micro-crystalline aggregate that, at surface temperatures, is incapable of quantitatively retaining argon generated subsequent to the devitrification. In order for a basalt to yield the age of its extrusion, it is essential that the potassium-bearing phases of the basalt be unaltered. In other words, any age figure obtained on such a sample as that used by Gentner and Lippolt must be considered a minimum estimate of the age of the basalt under Bed I. We have dated 2 samples of basalt from beneath Bed I, neither of them being ideal for age-dating purposes, and have obtained ages of 1,700,000 years and 1,900,000 years (see above). Thus there is no contradiction between the best estimate of the age of the basalt under the tuffs and the indicated age of the tuffs.

4. COMPARISON OF OLDUVAI AGES WITH AGES OF CONTEMPORANEOUS ROCKS ELSEWHERE

Finally—is an age of 1,750,000 years for Villafranchian mammals in disagreement with other physical age data? We have 1 date on a Villafranchian horizon at Valros, France (Kloosterman 1960) at 1,600,000 years. We have determined that Late Blancan mammals of North America which are considered to have lived contemporaneously with the Villafranchian or Early Villafranchian mammals of Europe are 2,400,000 years old (Evernden, *et al.* 1964). The age of 1,750,000 years for the Villafranchian mammals of Olduvai is therefore consistent with all other available data.

We believe that the above listed arguments indicate that there are no grounds for rejecting the Bed I dates on geological grounds and that they must be accepted as valid estimates of the age of *Zinjanthropus*.

ITALIAN SAMPLES

The applicability of criteria other than absolute dating to the clarification of the sequence of volcanic events in Italy during the last few million years is limited, as one does not have critical stratigraphic control on the relative ages of many volcanic flows. Within a local area, superpositional relationships can be demonstrated, but inter-volcano or even intra-volcano correlations are often such that there are no available checks on the geological validity of the results here published as Table 7.

Where superpositional or other stratigraphic control is available, the potassium-argon dates are in satisfactory agreement with such control. Thus, on the Island of Ischia, 3 samples of known relative age have been dated:

Sample Number	Stratigraphy	Age (years)
KA 403	Erupted in A.D. 1306	0 years
KA 1137	Epomeo Tuff	83,000 years
KA 410	Tuff older than Epomeo Tuff	3.97×10^6 years
From Volcano Albano:		
KA 409	1 of youngest flows of volcano	277,000 years
KA 855	Older than KA 409, younger than KA 348	268,000 years
KA 348	Old phase of Albano. Older than KA 855	706,000 years

Under the section of reproducibility, the data for the "tuff with black pumices" of Torre in Pietra and surrounding localities have already been noted.

General relationships between the several volcanoes for which we have obtained 1 or more dates have been considered to be as follows:

Volcano Albano—activity was largely after the Flaminian-Nomentanan interglacial and probably extended to Würm III;

Volcano Bracciano—only horizon dated is placed in Flaminian-Glaciation or Flaminian-Nomentanan Interglacial (Blanc 1957: 95-109);

Volcano Bolsena—activity was initiated during the Sicilian but time of termination is very uncertain, probably largely overlapping the period of activity of Volcano Bracciano (i.e., well post-Sicilian);

Volcano Tolfa—volcano probably of Pliocene age as evidenced by state of erosion;

Volcano Amiata—considered to be 1 of the older volcanoes due to its northern position in the string of Plio/Pleistocene volcanoes of Italy.

The potassium-argon ages obtained suggest the following conclusions:

1. The "old phase of Albano" may well be associated with a distinctly different volcanic episode than the "young phase". We will accept this interpretation and will consider the young phase to be that activity which is younger than the Flaminian-Nomentanan Interglacial.
2. The "tuff with black pumices" (Volcano Bracciano) yields an excellent control point for the Italian sequence. See discussions of this date under sections on Glaciations and on Time-scale of Human Cultures, pp. 355-60.
3. Activity at Volcano Bolsena extended to times more recent than previously thought and we failed to sample the older events in its history.
4. Tolfa is correctly placed as being Late Pliocene or Early Pleistocene.
5. Rather than being very old, Volcano Amiata was active at the same time as Bracciano and Bolsena.
6. Monte Cimino is greater than 1,000,000 years in age, its present topographic form being not inconsistent with such an age.
7. The flow sampled at Punta della Madonna, Ischia is the oldest volcanic rock dated.
8. Volcanic activity appears to have been particularly widespread 250,000-450,000 years ago (Amiata, Bracciano, Bolsena, Albano, Rocca Monfina).

K-Ar DATES RELATED TO GLACIATIONS

Data presently available which relate to glacially influenced strata or to Villafranchian strata are presented in Table 8.

KA 409 and KA 855 are dates on flows from Volcano Albano, Italy, a volcano whose activity was largely during the Flaminian/Nomentanan Interglacial and later. KA 1185 is from the "tuff with black pumices" of Volcano Bracciano and is interbedded between deposits correlated with the Flaminian and Nomentanan Glaciations. KA 264 is on a phonolite-bearing volcanic rock from the Eifel whose debris is thought to appear in the Lower Main Terrace of the Rhine but not in the Upper Main Terrace (Evernden, Curtis, and Kistler 1957: 1-5). The dates on glaciation on Kilimanjaro are interesting but seem to be of little or no use for elucidation of the overall time-scale of Pleistocene glaciations. KA 305 is on the Bishop Tuff, California, a tuff which overlies a glacial till (Evernden, *et al.* 1964). This till has been assigned an Illinoisan age by Putnam (1960:270-74) but numerous investigators of the field relationships doubt this correlation. We must consider that this till

TABLE 7
ITALIAN SAMPLES

Number Sample	Mineral	Procedure	Weight (grams)	K (%)	Ar^{at}_{40} (%)	Age (10^6 years)	HF	Repeat Runs
KA 302	Sanidine	B (1000° C/10 hours)	24.5	10.57	13	2.3	NO	
KA 304	Sanidine	B (1040° C/12 hours)	35.1	12.06	32	0.431	NO	
KA 334	Sanidine	B (1150° C/1 hour)	20.8	12.16	66	0.434	NO	KA 345, 1185
KA 345	Sanidine	B (1110° C/20 hours)	15.2	12.16	88	0.438	NO	KA 334, 1185
KA 348	Leucite	B	32.5	16.92	53	0.706	NO	
KA 403	Sanidine	B (1120° C/12 hours)	120		100	0.00	NO	
KA 406	Sanidine	B (1180° C/12 hours)	24.5	9.83	69	0.328	NO	
KA 407	Sanidine	B (1180° C/12 hours)	11.0	11.9	56	0.417	NO	
KA 408	Sanidine	B (1140° C/12 hours)	11.8	11.53	47	0.432	NO	KA 1175
KA 409	Leucite	B (600° C/12 hours)	57.1	16.38	72	0.277	NO	
KA 410	Sanidine	B (1150° C/12 hours)	27.0	4.20	39	3.97	NO	
KA 441	Sanidine	C	10.0	6.94	85	0.430	NO	
KA 457	Leucite	C	10.5	15.85	88	0.275	NO	
KA 460	Leucite	C	14.0	16.81	85	0.368	NO	KA 1137
KA 464	Sanidine	C	14.3	9.18	94	0.072	NO	
KA 853	Leucite	C	9.53	15.62	97	0.095	NO	
KA 854	Leucite	C	11.7	15.92	82	0.431	NO	
KA 855	Leucite	C	11.4	16.07	88	0.268	NO	
KA 857	Biotite	C	2.23	7.36	45	6.4	NO	
KA 1137	Sanidine	F	12.6	9.48	54	0.083	YES	KA 464
KA 1162	Sanidine	F	10.1	10.97	13	1.14	YES	
KA 1175	Sanidine	F	2.47	11.45	19	0.422	YES	KA 408
KA 1181	Sanidine	F	11.2	10.59	5	1.18	YES	
KA 1185	Sanidine	F	9.95	12.07	17	0.431	YES	KA 334, 345

is of unknown affinities with any other Sierran till and so with any other till elsewhere in North America. KA 1189 may be of extreme significance. It is on a basalt from Idaho interbedded with sediments carrying remains of *Mammuthus* (Malde and Powers 1962:1197-1220). Vertebrate palaeontologists of North America assign *Mammuthus*-bearing strata to the Irvingtonian Age, and they, in addition, suggest a Kansan Glaciation age for such strata! The present controls on that correlation are minimal. In Nebraska, such a correlation is based upon assuming that a late Blancan fauna of western Nebraska is correlative with deposits of the Aftonian Interglacial of eastern Nebraska (i.e., thus implying that the Irvingtonian Age could not have begun by pre-Kansan time) (Condra, *et al.* 1947), and by assuming that Kansan strata of Nebraska which contain *Mammuthus* record the first occurence of *Mammuthus* in North America. In Meade County, Kansas, beds containing *Mammuthus* are assigned a late Kansas Age on stratigraphic and palaeontologic evidence (Hibbard 1953; Taylor 1960). It does seem apparent that one should conclude that *Mammuthus*-bearing beds are no older than Nebraskan Glaciation. For sake of discussion only, let us presume that the lake bed sequence of "Middle Pleistocene" Age in which the *Mammuthus* occur, the Bruneau Formation of Malde and Powers (1962), is correlative with Nebraskan Glaciation. There is no basis for such a correlation other than this attempt to create as little chaos as possible when interpreting available potassium-argon ages. A suggested time-scale of the glaciations might be as shown in Table 9.

The North American-Alpine correlations form 1 of 2 previously suggested schemes, and the Alpine-Rome area correlations are those proposed by A. C. Blanc (1957:95-109). Such a scheme, in conjunction with all of the available data, says:

1. Main Terraces of the Rhine are in some sense correlative with Mindel Glaciation;
2. evidence of glaciation on Kilimanjaro goes back only as far as Gunz to Mindel time;
3. the glacial till below the Bishop Tuff of California has nothing to do with Sangamon-time but is rather to be correlated with Nebraskan moraines of the central United States;
4. the time of transition from late Blancan fauna to Irvingtonian fauna was concurrent with the Nebraskan glaciation.
5. the Villafranchian faunas of Olduvai Gorge and Valros, France are 2 to 3 times older than the Gunz glaciation; and
6. the time-scale of the classic Alpine glaciations is essentially that proposed by Zeuner, *et al.*, on the basis of the astronomical theory.

The above suggested scheme appears to do the least damage to previously accepted concepts of 1 sort or another. Another possible scheme would be to place the beginning of the Irvingtonian Age at post-Kansan Glaciation with a consequent large extension of the time-scale of the glaciations. Appropriate juggling could accommodate much of the available data within a reasonable relative-time-scale scheme. The Main Terrace date would either be ignored or used to imply that these terraces were correlative with Riss Glaciation; the Olduvai Villafranchian dates could be brought within the range of the Pleistocene glaciations; the "tuff with black pumices" date would be placed in the Flaminian-Nomentanan Interglacial; the Albano dates would probably be post-Nomentanan Glaciation; and the time-scale of the glaciations would depart widely from that suggested by Zeuner and others on the basis of astronomical theory. Presently available data do not permit a choice between the above schemes.

TABLE 8

DATES RELATED TO GLACIAL EVENTS

SAMPLE NUMBER	RELATIONSHIP TO GLACIAL EVENTS	AGE (YEARS)	DATA
KA 409 KA 855	Flaminian (= Mindel ?)/Nomentanan (= Riss ?) Interglacial or younger, Italy	270,000	See Table 8
KA 937	Early "Second Glacial", Kilimanjaro	365,000	See Table 5
KA 264	Lower Main Terrace of Rhine ? (= Elster ? = Gunz ? = Mindel ?)	370,000	See Evernden, *et al.* 1957:1–5
KA 1185, etc.	Flaminian (= Mindel ?) Glaciation or Flaminian/ Nomentanan (= Riss ?) Interglacial	430,000	See Table 8
KA 947	Early "Post-First Glacial", Kilimanjaro	463,000	See Table 5
KA 940	Prior to "First Glacial", Kilimanjaro	510,000	See Table 5
KA 305, etc.	After a glaciation in California	980,000	See Evernden, *et al.* 1964:145–98
KA 1188	Irvingtonian fauna, post-Blancan, glacial ?	1,360,000	Wt = 28.2 gr, K = 0.62%, Ar^{at}_{40} = 56%
KA 1184	Villafranchian, Valros, France	1,610,000	Wt = 20.8 gr, K = 0.80%, Ar^{at}_{40} = 95%
KA 1058, etc.	Villafranchian fauna, Olduvai (Upper Villafranchian according to L. S. B. Leakey)	1,750,000	See Table 6

TABLE 9

SUGGESTED TIME-SCALE AND CORRELATIONS OF LATE PLEISTOCENE GLACIATIONS

NORTH AMERICA	ALPS	ITALY	AGE
Nebraskan	Donau	Aquatraversan	1,000,000–1,200,000 years
Kansan	Gunz	Cassian	approximately 600,000 years
Illinoisan	Mindel	Flaminian	approximately 400,000 years
Wisconsin	Riss and Wurm		10,000 to approximately 250,000 years

TABLE 10

POST-OLDUWAN CULTURAL SITES IN EAST AFRICA

(All ages enclosed in brackets are invalid estimates of the absolute ages of relevant samples for reasons discussed in the text.)

SAMPLE NUMBER	MINERAL	PROCEDURE	WEIGHT (GRAMS)	K (%)	Ar^{at}_{40} (%)	AGE (10^6 YEARS)	HF	REPEAT RUNS
KA 405A	Anorthoclase	F	10.56	5.01	70	0.502	YES	
KA 411	Anorthoclase	B (1150° C/12 hours)	51.27	5.18	70	3.34	NO	
KA 413	Anorthoclase	B (1150° C/12 hours)	31.39	4.78	62	(0.486)	NO	
KA 415	Anorthoclase	B (1270° C/3 hours)	26.16	4.79	26	(3.1)	NO	
KA 417	Anorthoclase	B	29.85	5.60	85	0.276?	NO	
KA 435	Anorthoclase	C	8.32	4.66	57	(2.9)	NO	KA 413
KA 458	Anorthoclase	C	14.26	5.69	90	0.273?	NO	KA 417
KA 459	Anorthoclase	C	14.65	5.47	95	0.098?	NO	KA 417
KA 918	Anorthoclase	C	20.24	4.27	73	(2.55)	NO	KA 1090
KA 920	Anorthoclase	C	25.14	3.62	98	0.029	NO	
KA 921	Anorthoclase	C	20.32	5.00	61	0.557	NO	
KA 923	Anorthoclase	C	25.04	5.27	13	(1.64)	NO	KA 413
KA 963	Anorthoclase	D	12.48	5.12	81	0.230	NO	KA 1086
KA 965	Anorthoclase	D	4.87	4.91	22	(0.928)	NO	
KA 1035	Anorthoclase	E	9.27	5.39	19	0.946	YES	
KA 1061	Anorthoclase	F	9.12	5.41	11	(1.11)	YES	
KA 1089	Anorthoclase	F	12.47	5.29	37	0.439	YES	
KA 1086	Anorthoclase	F	11.11	5.00	43	0.244	YES	
KA 1090	Anorthoclase	F	9.86	4.96	7	(4.4)	YES	KA 918

TIME-SCALE OF HUMAN CULTURAL EVOLUTION

The absolute age data which we have that directly determine ages of cultural levels are as follows:

1. Olduwan Culture. Tools of Olduwan culture are associated with *Zinjanthropus* skeletal remains in Bed I, Olduvai Gorge and thus have at that level an age of 1,750,000-1,800,000 years. Pure Olduwan artifacts are found to the top of Bed I and even into the lower portions of Bed II. Thus a pure Olduwan culture persisted to a time much younger than Marker Bed A (1,550,000 years) and possibly to nearly the time of deposition of the highly micaceous beds of Bed II (1,110,000).

2. Late Chellean. At Olduvai, a Chellean II or III culture is overlain by a tuff whose age is 500,000 years, which is in turn overlain by beds containing tools

classed as transitional from Chellean to Acheulian (Cole 1954).

3. Choukouteinian Culture. Accepting the essential equivalence of *Sinanthropus* and *Pithecanthropus* (Boule and Vallois 1957), and that they probably fashioned similar tools, a date of 500,000 obtained on a leucitic lava of Java which post-dates all *Pithecanthropus*-bearing levels indicates that these men and their culture existed prior to 500,000 years ago.

4. Abbevillian-Acheulian Culture. At Torre in Pietra, fragments of the "tuff with black pumices" occur in the deposits burying an Abbevillian-Acheulian culture (Blanc 1957:95-109). Field evidence suggests a close correlation in time between the occupation of the living site and eruption of the pumice. Thus this cultural transition took place in Italy around 430,000 years ago.

5. Pseudo-Stillbay Culture. A crystal vitric tuff containing some lapilli buries a Pseudo-Stillbay culture at Cartwright's site, Kenya. Field evidence suggests that the tuff is primary and that it buried an occupied living site. The potassium-argon age obtained on a feldspar concentrate from this tuff is 440,000 years. A date that tends to confirm this estimate is an age of 557,000 years on a 3-foot flow or welded tuff which occurs 4-5 feet stratigraphically below a Pseudo-Stillbay culture at Wetherill's site, Kenya. The quality of this sample is unequivocally excellent, uncertainty about the significance of the age arising from lack of specific knowledge of the time interval between extrusion of the flow and accumulation of the Pseudo-Stillbay artifacts.

6. Kenya Stillbay Culture. At Malawa Gorge, Kenya, a 3-inch pumice lapilli and crystal layer overlies Kenya Stillbay artifacts. Coarse-grained euhedral crystals were collected individually from this tuff. Contamination by crystals of an older tuff seems extremely unlikely. The 2 runs on this material are in essential agreement, indicating that Kenya Stillbay artifacts are as old as 240,000 years.

Is this last date contradicted by or supported by other field and stratigraphic relationships? The generally accepted estimate of the age of Kenya Stillbay cultures is approximately 30,000 years, based upon correlation of the Gamblian "pluvial" with the Würm glaciation of Europe. The field evidence in Kenya seems plainly to contradict such an estimate. At Malawa Gorge, the Kenya Stillbay artifacts are buried under more than 30 feet of silts, sands, and fine gravels which occur more than 120 feet above the present level of the lake. Erosion subsequent to this period of deposition produced a submature topography on the deposits. It is virtually certain that all these events took much longer than 30,000 years to occur. In confirmation of this view, we can mention another potassium-argon date we obtained in the same general area. In very late Pleistocene time, long after the above-described events took place, Lake Naivasha was dammed by an eruption of tuff and lava. The lake rose, overflowed the lava, and cut a channel through the lava and tuff. The channel has been dry since the last high lake level. The age of 28,000 years obtained for the tuff is in accord with geology and supports our contention that geological evidence in the Rift Valley requires an age for Kenya Stillbay cultures much greater than previously imagined.

A similar type of argument can be given in support of the Pseudo-Stillbay date. Cartwright's site (the Pseudo-Stillbay site we dated) lies at the top of a series of fault scarps which have had hundreds of feet of displacement since the site was occupied. The rocks exposed in the scarps are lava flows, highly resistant to erosion under the climatic conditions of the area. Yet a canyon has been cut into these lavas to over a depth of 100 feet by a very small intermittent stream. With many times the water to do the work, canyons of similar depth in the Sierra Nevada of California have required 100,000 years to be cut (determined by potassium-argon dating in our laboratory). Thus it is geologically reasonable to presume that several 100 thousand years might have been required to cut the canyons of 100-foot depth in the Rift Valley.

It is true that cultures assumed to be time correlates of these 2 cultures have been dated by radio-carbon in Africa. An age of 57,300 years was obtained on wood from a final Acheulian (Pseudo-Stillbay correlate) site at Kalambo falls, Northern Rhodesia (Clark 1960:307-24; Leakey 1960). A Stillbay variant, called Pietersburg at Olieboompoort Cave, Transvaal, was dated at 25,000 years (Flint and Deevey 1960) while the Rhodesian Lupemban culture at Kalambo falls was dated at 25,000-28,500 years (Clark 1959). The significance of this disagreement will only be clarified by further dating.

Finally—is such an estimate of the age of the Pseudo-Stillbay culture inconsistent with other anthropological evidence? To be sure, it is inconsistent with the following scheme of argument which we believe is a fair presentation of that now employed for estimating the absolute age of this culture:

1. Pseudo-Stillbay is contemporaneous with early Levalloisian in East Africa;
2. Early Levalloisian appeared at the end of the "Kanjeran Pluvial";
3. The "Kanjeran Pluvial" is contemporaneous with the Riss Glaciation and the "Gamblian Pluvial" with Würm Glaciation;
4. The time-scale of glaciations is that suggested by Zeuner's interpretation of the astronomical theory; and
5. Therefore, Pseudo-Stillbay cultures would have appeared approximately 200,000 years ago.

Statements in refutation of this scheme are easily composed. The fact that flake-tool cultures existed contemporaneously with core-tool cultures from late Chellean onwards in Europe suggests that such may well have happened in East Africa. In fact, the co-existence of early Levalloisian and early and Middle Acheulian in North Africa may be considered as more than suggestive of such a probable relationship in East Africa. Thus, an age for Pseudo-Stillbay in the range of lower Acheulian is to be expected, i.e., exactly the result obtained.

Secondly, the involved argument based upon pluvial-glaciation correlations is subject to doubt on several counts. The demonstration that Beds I and II of Olduvai Gorge, purportedly deposited during the "Kamasian Pluvial" and thus during the Mindel Glaciation (Leakey 1960), span more than 1,350,000 years, indicates the lack of correlation of this so-called pluvial with any single European glaciation. The tenuousness of the glaciation-"pluvial" correlation has been noted by others (McBurney 1960). Thus, the whole scheme of pluvial-glaciation correlation must be considered as suspect. It may, of course, happen that the Gamblian Pluvial is correlative with a specific European glaciation, but there is no secure argument for rejecting a correlation with the Riss Glaciation. Finally, Kenya Stillbay may not be synchronous with numerous Stillbay variants elsewhere.

Therefore, it seems obvious that there is no compelling evidence against the ages obtained for the Pseudo-Stillbay and Kenya Stillbay cultures. The basic fact to be grasped is that early estimates of the ages of nearly all stone-age cultures were in gross error, all estimates being far too young.

We have attempted to obtain age data on 3 other East African sites without success.

A. Dighton's Cliff Extension, Magosian Culture. At this site, a Magosian industry is buried by what appears to be a primary crystal vitric tuff. We obtained an age of approximately 270,000 years on a split of the original mineral concentrate (KA 417). The remaining sample was then split on the magnetic separator (iron staining of some crystals allowed such a splitting), and the 2 fractions were run as KA 458 and KA 459. An age of 270,000 years was obtained on the former in agreement with KA 417, but an age of 100,000 years was obtained on KA 459. The disagreement would generally be construed as meaning contamination, but the apparently primary nature of the deposit would tend to deny this. The only way of resolving the problem is to again sample and inspect the site.

B. Ol Orgesailie, Acheulian Culture. The only bed found that seemed to present any hope of being useful for dating is a tuffaceous layer between land surfaces 5 and 6. However, from the moment of collection, we expected the sample to prove to be contaminated. The results obtained, on KA 413, KA 435, and KA 923 confirmed our expectations. In addition, we analysed a crystal concentrate from an obviously reworked tuff from just above land-surface 11 at Ol Orgesailie and again proved the presence of contamination. All results obtained at Ol Orgesailie are useless for establishing close chronologic control of the site. We can state only that the site is younger than 486,000 years.

C. Kariandusi, Acheulian Culture. Again, the discovery of primary tuffs at the level of the industry proved impossible. In desperation, and in order to apply the criterion of relative age, we dated several samples. In stratigraphic order, they are as follows:

KA 415	water-laid tuffaceous sediment overlying main site	3.1×10^6 years
KA 1061	tuff (rounded pumice fragments in tuff) with axe	1.1
KA 965	possibly reworked tuff under hand axe	0.93
KA 1035	pumice fragments from diatomite 50 feet below site level	0.95

The last date is probably correct but it does very little towards determining the absolute age of the Acheulian industry at Kariandusi.

The data on ages of human cultures permit the following conclusions:

1. Olduwan and Chellean cultures persisted at Olduvai Gorge for approximately 1,350,000 years, or approximately 700,000 years each. The earlier so-called Kafuan cultural tradition of East Africa (see for example, Leakey 1960) probably had a comparable life-span, and thus there is little doubt that hominoids

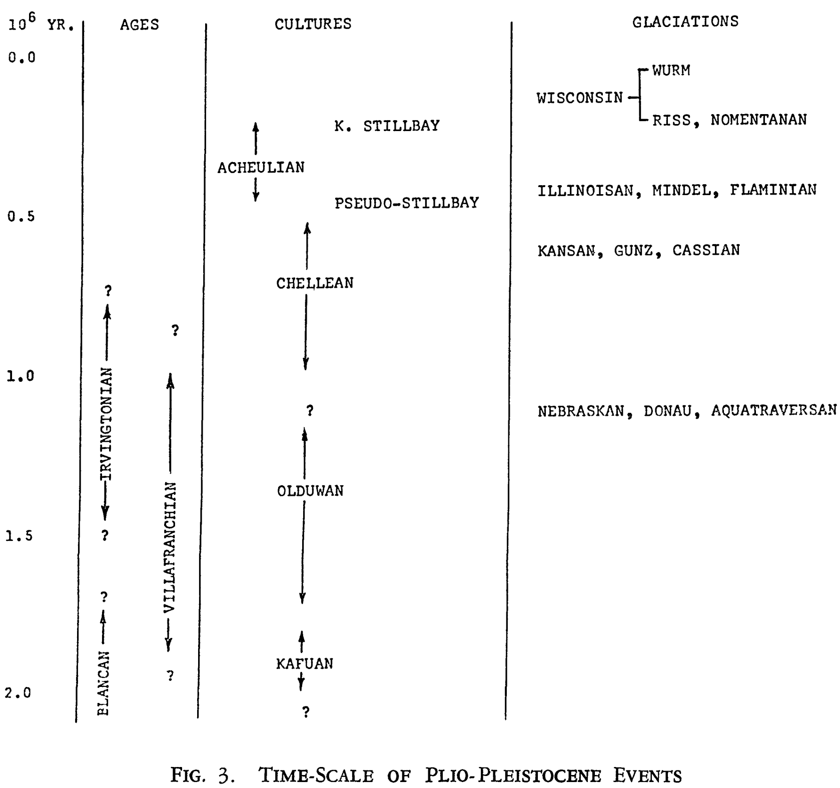


FIG. 3. TIME-SCALE OF PLIO-PLEISTOCENE EVENTS

capable of fashioning stone tools were extant well over 2,000,000 years ago.

2. Late Chellean-Early Acheulian cultures were extant in Europe and East Africa 450,000-500,000 years ago.

3. Pseudo-Stillbay cultures were extant in East Africa 430,000 years ago, i.e., only 50,000 or so years later than the late Chellean of Olduvai.

4. Kenya Stillbay cultures were extant 250,000 years ago in East Africa.

It follows that:

a. the so-called Kamasian Pluvial during which Beds I and II at Olduvai are supposed to have accumulated covered a time-span of well over 1,250,000 years is thus not to be considered correlative with any of the European glaciations. The lithologic characteristics of the sediments of Bed I and Bed II are not to be explained on the basis of a "pluvial" related to a more northerly glaciation. See Hay (1963:829-33) for statement that Bed I accumulated during period of desiccation rather than during pluvial; and

b. if the Gamblian Pluvial is correlative with any European Glaciation, it is with Riss, certainly not with Würm.

Finally, we present Figure 3 as a *suggested* interpretation of available data. The most encouraging thing about this figure is that it shows that we are actually accumulating data in this time interval. Five years from now, few questions about the general time-scales of Pleistocene glaciations and of human cultural evolution will remain to be answered.

Abstract

A technique for the potassium-argon dating of high potassium feldspars of less than 50,000 years age is described. The technique is applied to the obtaining of high precision ages in the time-range 60,000-2,000,000 years. Sufficient data are presented to show that the time-scale of Plio-Pleistocene glaciations is greater than 10^6 years and that the time-scale of hominoids capable of fashioning tools by the working of stone is at least 1.75×10^6 years. Several other points on the time-scale of human evolution are presented.

The time-scale of rift faulting in Kenya is established and the ages of several Italian volcanoes are presented.

Appendix

All ages enclosed in parentheses ( ) are invalid estimates of the absolute age of the rock for reasons discussed in the text.

KA 264 — Sanidine — 370,000 years
Location: Laacher See, Germany.
Stratigraphy: See reference.
Reference: Evernden, Curtis and Kistler, 1957.

KA 269 — Sanidine — 56,000 years
Location: Hill 8044, Mono Craters, California.
Stratigraphy: Oldest of sanidine-rich plugs of Mono Craters series.
Remarks: See KA 1197 series for additional runs on this sample.

KA 302 — Sanidine — 2.3×10^6 years
Location: ½ kilometer east of Tolfa, Italy, on highway.
Stratigraphy: Deeply eroded, "Old Quaternary" volcano.

KA 304 — Sanidine — 431,000 years
Location: Rome area, Italy.
Stratigraphy: "Tuff with black pumices" from Volcano Bracciano. Same horizon as KA 334, etc.
Reference: Blanc, 1957.

KA 329 — Sanidine — 5,000 years
Location: Craters Mt., Mono Craters, California.
Stratigraphy: Younger than last high lake level of Mono Lake and thus younger than last ice advance in Sierra Nevada.

KA 334 — Sanidine — 434,000 years
Location: Torre in Pietra area, W. of Rome, Italy.
Stratigraphy: "Tuff with black pumices" of Volcano Bracciano. This tuff is younger than a peat seam related to the Flaminian (= Mindel ?) Glaciation and older than deposits related to the Nomentanan (= Riss ?) Glaciation. See text.
References: Blanc, 1957; Evernden, Savage, Curtis, James, 1963.
Remarks: Same horizon as KA 345, 1185, 407, 408, 304, 1175.

KA 336, 336R — Biotite — $15.2 \pm 1.5 \times 10^6$ years
Location: Rusinga Island, Lake Victoria. Site R107, Kiahera Series.
Stratigraphy: Below Proconsul-bearing strata.
Reference: Evernden, Savage, Curtis and James, 1963 for discussion of fauna and absolute age data and for bibliography on Rusinga Island fauna.

KA 345 — Sanidine — 438,000 years
Same sample as KA 334. See text.

KA 348 — Leucite — 706,000 years
Location: Cava di lava dell' Acquacetosa, between kilometer 8 and 9 on Via Laurentina, Rome, Italy.
Stratigraphy: Old phase of Monte Albano, older than KA 409.

KA 403 — Sanidine — 0 years
Location: Island of Ischia, Bay of Naples, Italy.
Stratigraphy: Flow extruded in 1302 A.D. (= Lava dell' Arso).

KA 405, 405A — Anorthoclase — 502,000 years
Location: Olduvai Gorge, Tanganyika. Site SHK.
Stratigraphy: Crystal vitric tuff overlying Chellean II or III industry, underlying Chellean/Acheulian industry.

KA 406 — Sanidine — 328,000 years
Location: At southwest limits of San Lorenzo Nuovo, Volcano Bolsena, Italy.
Stratigraphy: Sample from rim of crater. Pre-collapse.

KA 407 — Sanidine — 417,000 years
Location: Cava del Cecio, Volcano Bracciano, NW. of Rome, Italy.
Stratigraphy: "Tuff with black pumices", same horizon as KA 334, etc. See text.

KA 408 — Sanidine — 432,000 years
Location: Cava Nera Molinario, Volcano Bracciano, Grottarossa-Rome, Italy.
Stratigraphy: "Tuff with black pumices", same horizon as KA 334, etc. See text.

KA 409 — Leucite — 277,000 years
Location: Road from Rocca di Papa to Monte Cavo, 200 meters southwest of Monte Cavo, Volcano Laziale, Italy.
Stratigraphy: One of youngest flows of Latial complex. Scoriaceous agglomerate with much leucite.

KA 410 — Sanidine — 3.97×10^6 years
Location: Island of Ischia, Bay of Naples, Italy. Punta della Madonna, south side near the Roman tunnel.
Stratigraphy: Pre-Epomeo Tuff. Pliocene?

KA 411 — Anorthoclase — 3.34×10^6 years
Location: Durie's Site, Kinancop. Kenya.
Stratigraphy: Lava below archaeological site. Contact between lava and tuffs above is an unconformity.
Remarks: Lava age obviously of little use in dating site.

KA 412 — Anorthoclase — (1.63×10^6 years)
Location: Olduvai Gorge, Tanganyika. Immediately west of third fault on south side of gorge at knife-edge descent.
Stratigraphy: Crystal vitric tuff correlative with or just below tuff of KA 1058, etc. See text.

KA 413 Anorthoclase (486,000 years)
Location: Ol Orgesailie, Rift Valley, Kenya.
Stratigraphy: Crystal pumice lying between land surfaces 5 and 6. Redeposited and contaminated nature obvious in the field.

KA 415 Anorthoclase (3.1×10^6 years)
Location: Kariandusi, Kenya.
Stratigraphy: Water-laid tuffaceous bed overlying sites at Kariandusi. Redeposited and contaminated nature obvious in the field.

KA 417 Anorthoclase 276,000 years?
Location: Dighton's cliff Extension, Rift Valley, Kenya.
Stratigraphy: Crystal-bearing black glass or pumice overlying Magosian industry (i.e., equivalent in age to end of Gamblian Pluvial).

KA 427 Biotite 14.0×10^6 years
Location: Fort Ternan, Kenya.
Stratigraphy: Interbedded with Kericho Phonolites and overlies mammals of latest Miocene or early Pontian age (Leakey, personal communication). See reference for more details.
Reference: Evernden, Savage, Curtis and James, 1963.

KA 433, 433R Leucite 500,000 years
Location: Central Java. Muriah Volcano, near Tempur, Djapara district.
Stratigraphy: Approximately equal in age to Trinil Beds and thus to *Pithecanthropus.* "Middle Pleistocene".

KA 435 Anorthoclase (2.9×10^6 years)
Rerun of KA 413.

KA 436 Anorthoclase ($1.89 \pm .13 \times 10^6$ years)
Remainder of KA 412 sample split on Frantz isodynamic magnetic separator at 1.6 ampere and 5° tilt. Low-magnetic fraction run as KA 436, high-magnetic fraction run as KA 437.

KA 437 Anorthoclase 1.79×10^6 years

KA 441 Sanidine 430,000 years
Location: In Piancastagnaio. Volcano Amiata, Italy.
Stratigraphy: Comparatively late flow of Volcano Amiata, a volcano presumed to be one of the oldest of Italian volcanoes.

KA 457 Leucite 275,000 years
Location: Just north of Aquapendente, Volcano Bolsena, Italy.
Stratigraphy: Flank flow of Volcano Bolsena. Leucite-basalt.

KA 458 Anorthoclase 273,000 years?
Remainder of KA 417 sample split on Frantz isodynamic magnetic separator at 1.6 ampere and 5° tilt. Low-magnetic fraction run as KA 459, high-magnetic fraction run as KA 458.

KA 459 Anorthoclase 97,500 years?
See KA 458.

KA 460 Leucite 368,000 years
Location: Roccamonfina, Italy.
Stratigraphy: Volcano Roccamonfina.

KA 464 Sanidine 72,000 years
Location: Forio, Ischia, Italy.
Stratigraphy: Epomeo Tuff.
Remarks: See KA 1137 for an improved run on this sample.

KA 475I, 475IV Anorthoclase 3.1×10^6 years
Location: High on Mount Kenya, Africa.
Stratigraphy: Anorthoclase-bearing kenyte flow.

KA 650 Anorthoclase 1.74×10^6 years
Location: Approximately 1 mile west of Kampi ya bibi, on Nairobi-Magadi road. Grid reference in East Africa Grid System HZH 9129.
Stratigraphy: Plateau Trachyte Series, Plv_2 of Baker. In the field, it appears that this sample is low in the Plv_2 series. These flows pre-date the Plv_3 series, both of them predating the Ol Orgesailie lake-beds. One of the major periods of rift faulting took place between the time of outpouring of the plateau trachytes and the time of accumulation of the Ol Orgesailie lake-beds.
Reference: Baker, 1958.

KA 651 Nepheline 13.4×10^6 years
Locality: At Stoney Athi River on Nairobi-Mombasa highway, Kenya. Grid reference in East African Grid System HZJ 4538.
Stratigraphy: Kapiti phonolite, lies on pre-Cambrian basement. This flow pre-dates any rift faulting.
References: Shackleton, 1945: Binge, 1962.

KA 652 Anorthoclase 2.4×10^6 years
Locality: On east edge of Legemunde Plain, ¼–½ mile north of high-way, near Ol Orgesailie, Kenya. Grid reference in East Africa Grid System HZH 8328.
Stratigraphy: Plv_3 series of Baker. Orthophyric Trachyte. According to Baker, this is the youngest pre-lake bed flow, i.e., younger than KA 650 and KA 661.
Reference: Baker, 1958.

KA 653 Nepheline 5.8×10^6 years
Locality: Approximately 2 miles northeast of Loitigoshi, several miles east of rim of Rift Valley. Grid reference in East Africa Grid System HZJ 2129.
Stratigraphy: Ol Orgesailie Phonolite. In spite of the name of this rock, it has no genetic relationship to the volcano of the same name. That volcano is within the Rift Valley at a distance of about 25 miles. The Ol Orgesailie Phonolite was extruded prior to any rift faulting.

KA 654 Anorthoclase 1.72×10^6 years
Locality: On top scarp of Rift Valley at north end of Ngong Hills, near Nairobi, Kenya. Grid reference in East Africa Grid System HZJ 0352.
Stratigraphy: Limuru Trachyte. This flow is younger than initial rift faulting but older than much of the faulting. Thought to be older than Plv_2 series though this is not demonstrable on superpositional relationships. Limuru Trachyte is reported to be younger than Mount Kenya and its Kenyte lavas.
Reference: Shackleton, 1945.

KA 656 Biotite (21.7×10^6 years)
Locality: Rusinga Island, Lake Victoria.
Stratigraphy: See reference.
Reference: Evernden, Savage, Curtis and James, 1963.

KA 657 Anorthoclase 2.64×10^6 years
Locality: Near top of Mount Kenya, Africa.
Stratigraphy: Mount Kenya Syenite. This syenite forms the plug of Mount Kenya.

KA 661 Anorthoclase 802,000 years
Locality: Same as KA 650.
Stratigraphy: Plateau Trachytes, Plv_2 series of Baker. Different flow from KA 650. Thought to pre-date Plv_3 series.
Reference: Baker, 1958.

KA 662 Anorthoclase 2.7×10^6 years
Locality: Approximately 4½ miles South 60° West of top of Ol Orgesailie, on Nairobi-Magadi road. Grid reference in East Africa Grid System HZH 1075.
Stratigraphy: Ol Orgesailie Trachyte. Flow from volcano of Ol Orgesailie, pre-dates Plv_2 series.
Reference: Baker, 1958.

KA 663 Anorthoclase 5.2×10^6 years
Locality: Quarry on Beacon Ranch, ¼ mile southeast of STIGANDS Tri. Point. Grid reference in East Africa Grid System HZJ 3845.
Stratigraphy: Athi Phonolite. Overlies Kapiti Phonolite. Pre-dates any rift faulting.

KA 664, 664R Biotite 1.1×10^6 years
Locality: Olduvai Gorge, Tanganyika. See reference.
Stratigraphy: Described previously as being in upper part of Bed I (see reference). That placement was in error. R. Hay has shown that the biotite-laden tuff horizon that was thought to be in Bed I is actually well up in Bed II (R. L. Hay, personal communication).
Reference: Leakey, Evernden, and Curtis, 1961.

KA 800 Biotite (42.0×10^6 years)
Locality: Rusinga Island, Lake Victoria. See reference.
Stratigraphy: See reference.
Reference: Evernden, Savage, Curtis and James, 1963.

KA 801 Biotite (163×10^6 years)
Locality: Rusinga Island, Lake Victoria. See reference.
Stratigraphy: See reference.
Reference: Evernden, Savage, Curtis and James, 1963.

KA 802 Biotite (104×10^6 years)
Locality: Rusinga Island, Lake Victoria. See reference.
Stratigraphy: See reference.
Reference: Evernden, Savage, Curtis and James, 1963.

KA 846 Anorthoclase (1.57×10^6 years)
Locality: Olduvai Gorge, Tanganyika. MK Site.
Stratigraphy: 13–14 inch coarse-grained buff tuff, 18 inches below hominid remains. Tuff obviously contaminated.
Remarks: See text for discussion of this sample and KA 919 and KA 1082. This sample was not treated with HF.
Reference: Leakey, Evernden, Curtis, 1961.

KA 847 Anorthoclase (1.85×10^6 years)
Locality: Olduvai Gorge, Tanganyika. MK Site.
Stratigraphy: Light gray tuff approximately 20 feet thick just above hominid layer. Appears to be Pelean-type ash.
Remarks: Re-run by improved procedures as KA 1043.
Reference: Leakey, Evernden, Curtis, 1961.

KA 848 Anorthoclase (5.4×10^6 years)
Locality: Olduvai Gorge, Tanganyika. Pre-*Zinjanthropus* Site.
Stratigraphy: Approximately 3-foot light buff tuff forming living floor of pre-*Zinjanthropus* Site. Bone fragments in separated sample. Sample contaminated.

KA 849 Anorthoclase (1.89×10^6 years)
Locality: Olduvai Gorge. FLK I Site.
Stratigraphy: 11-inch crystal tuff overlying *Zinjanthropus* layer. May be largely derived.
Remarks: Re-run as KA 1036.

KA 850 Anorthoclase 1.78×10^6 years
Locality: Olduvai Gorge. Pre-*Zinjanthropus* Site.
Stratigraphy: 2-inch ash above very fine-grained 1-inch layer resting on pre-*Zinjanthropus* floor. Possibly contaminated (bone in layer) but looks fine in the field.
Remarks: Re-run as KA 1051.
Reference: Leakey, Evernden, Curtis, 1961.

KA 851 Anorthoclase (1.64×10^6 years)
Locality: Olduvai Gorge, Tanganyika. FLK I Site.
Stratigraphy: 12-inch light cream color tuff immediately under *Zinjanthropus* floor. Small bone fragments in separated samples suggesting reworking and possible contamination. Sample strongly weathered with *Zinjanthropus*-time root holes.
Remarks: Re-run by improved procedures as KA 1042. KA 851 not leached with HF. See text for discussion.
Reference: Leakey, Evernden, Curtis, 1961.

KA 853 Leucite 95,000 years
Locality: Vetralla, Volcano Vico, Italy.
Stratigraphy: Leucitic lava from volcano.

KA 854 Leucite 431,000 years
Locality: On route 71 west of Orvieto at turnoff to Porano, Italy.
Stratigraphy: Leucitic lava of Volcano Bolsena.

KA 855 Leucite 268,000 years
Locality: Colata di lava del Divino Amore, Volcano Laziale, Italy.
Stratigraphy: Leucitic lava, older than KA 409, younger than KA 348.

KA 857 Biotite 6.4×10^6 years
Locality: Island of Elba.
Stratigraphy: Granite described as "Miocene" age. By time-scale presented in Evernden, Savage, Curtis and James, 1964, this granite is older than the Astian/Plaisancian Stage-Age and is thus "Miocene" on one definition. It must be remembered that a potassium-argon age on a granite is a measure of the time since this granite cooled below 250°–300° C. and can be several million years less than time of original intrusion and emplacement. The length of the time interval between intrusion and cooling to 250° C. depends upon depth of emplacement of the granite and its rate of rise to a near-surface environment.

KA 918 Anorthoclase (2.55×10^6 years)
Locality: Wetherill's Site, Rift Valley, Kenya.
Stratigraphy: Tuff above laterite containing Pseudo-Stillbay industry. Field and laboratory criteria indicate presence of contamination.

KA 919 Anorthoclase (2.36×10^6 years)
Locality: See KA 846.
Stratigraphy: Same sample as KA 846. See text for discussion.

KA 920 Anorthoclase 29,000 years
Locality: Hell's Gate, Lake Naivasha, Kenya.
Stratigraphy: Pumice in which Hell's Gate channel is cut.

KA 921 Anorthoclase 557,000 years
Locality: Wetherill's Site, Kinancop: northeast of Lake Elmeinteita, Kenya.
Stratigraphy: 3-foot flow or welded tuff. Occurs across 200 feet wide gulch from Pseudo-Stillbay Site, but flow occurs 4–5 feet stratigraphically below level of industry.

KA 923 Anorthoclase (1.64×10^6 years)
Locality: Ol Orgesailie, Rift Valley, Kenya.
Stratigraphy: 42 feet above land surface 5, just below land surface 6. Same horizon as KA 413. On field evidence, this bed was classified as redeposited and probably contaminated.

KA 924A Anorthoclase (1.60×10^6 years)
Locality: Olduvai Gorge, Tanganyika. 100 feet upstream from Fault 3 on south side of gorge.
Stratigraphy: Coarse scoria or pumice fragments from 44 inches bed, base of which is 4 feet above level from which KA 412 came.
Remarks: Age invalid due to adhering surficial material. See KA 966, 1081, and 1179.

KA 925 Anorthoclase (1.45×10^6 years)
Locality: Ol Orgesailie, Rift Valley, Kenya.
Stratigraphy: Pumice crystal tuff just above land surface 11. Redeposited. See text. See KA 1059.

KA 927 Basalt (whole rock) (1.60×10^6 years)
Locality: Olduvai Gorge, Tanganyika, FLK Site.
Stratigraphy: Basalt flow interbedded in tuffs of Bed. I. See reference.
Remarks: See text for discussion of ages presented previously and the new data of this paper. New data are KA 927E and KA 1100. KA 927E invalid due to altered character of rock. See text.
Reference: Curtis and Evernden, 1962.

KA 933 Basalt (whole rock) (1.76×10^6 years)
Locality: Olduvai Gorge, Tanganyika. See reference.
Stratigraphy: Basalt interbedded in tuffs of Bed I. Rock very fine-grained. See text and reference for discussion.
Remarks: Result considered invalid in light of KA 1100 and because of very fine grain size. See text.
Reference: Curtis and Evernden, 1962.

KA 937 Obsidian 365,000 years
Locality: Kilimanjaro, Tanganyika.
Stratigraphy: Main Rhombporphyry Series. Early Second Glacial.

KA 940 Basalt (whole rock) 514,000 years
Locality: Kilimanjaro, Tanganyika.
Stratigraphy: Mawenzi trachy-basalt. Pre-First Glaciation.

KA 947 Basalt (whole rock) 463,000 years
Locality: Kilimanjaro, Tanganyika.
Stratigraphy: Lavaturm phonolite series. Early post-First Glacial.

KA 963 Anorthoclase 230,000 years
Locality: Malawa Gorge, northwest of Cartwright's Site, Rift Valley, Kenya.
Stratigraphy: 3-inch pumice lapilli and crystal layer about 7 feet above red zone in Phase III. Overlies Phase II which contains Stillbay artifacts. Could be contaminated, but would have to be so contaminated at coarse crystal size (1 to 3 millimeters). Such contamination seems highly unlikely.

KA 965 Anorthoclase (928,000 years)
Locality: Kariandusi, Kenya.
Stratigraphy: 15 inches pumice crystal tuff below hand-axe site on highway. May be reworked. Stratigraphic relationships of the several Kariandusi samples are as follows (Acheulian IV):

Water-laid tuffaceous sediment overlying main site	KA 415
Tuff containing hand-axe	KA 1061
Tuff just under hand-axe	KA 965
Approximately 50 feet stratigraphically lower than KA 965	KA 1035

See text for discussion.

KA 966 Anorthoclase (1.60×10^6 years)
Locality: See KA 924.
Stratigraphy: Same rock as KA 924, different concentrate. See text for discussion of several runs on material from this sample.

KA 1033 40% Plagioclase, 60% Anorthoclase (2.00×10^6 years)
Locality: Olduvai Gorge, Fifth Fault area.
Stratigraphy: ½-inch crystal vitric tuff, 5 feet above white marker bed in bentonitic lake beds of Bed II. Stratigraphic relationships of the four samples from this lake bed sequence are as follows:

Youngest KA 1038
KA 1087
KA 1055
Oldest KA 1033

See text for discussion.

KA 1035 Anorthoclase 946,000 years
Locality: Kariandusi, Kenya.
Stratigraphy: Crystals from pumice fragments in diatomite approximately 50 feet below hand-axe level (Acheulian IV).
Remarks: Age probably correct but does not date artifacts. Proves contaminated nature of all stratigraphically higher samples collected. See text.

KA 1036 Anorthoclase (2.03×10^6 years)
Locality: Olduvai Gorge, FLK I Site.
Stratigraphy: 11-inch crystal tuff overlying *Zinjanthropus* layer. May be largely derived on field evidence.

KA 1037 65% Plagioclase, 35% Anorthoclase 1.79×10^6 years
Locality: Olduvai Gorge. Near G' Site.
Stratigraphy: 30-inch crystal vitric lapilli tuff. Top of bed is 50 inches above hominid remains. Bed shows reverse graded bedding to middle of layer and normal graded bedding from there to top. Suggests primary ash fall with no redeposition.

KA 1038 Plagioclase (2.18×10^6 years)
Locality: Olduvai Gorge, Fifth Fault area.
Stratigraphy: 6-inch crystal vitric tuff, 33 feet 4 inches above white marker bed in green bentonitic lake beds of Bed II. See text and KA 1033.

KA 1039 Anorthoclase (1.74×10^6 years)
Locality: Olduvai Gorge, FLK I Site.
Stratigraphy: 8–10 inch crystal vitric lapilli tuff. Top of tuff is approximately 82 inches below *Zinjanthropus* floor.
Remarks: Re-run by improved procedure (leached with HF) as KA 1180.

KA 1040 Anorthoclase 1.65×10^6 years
Locality: Olduvai Gorge, FLK I Site.
Stratigraphy: 5-inch crystal vitric tuff. Base is 65 inches above *Zinjanthropus* floor.

KA 1041 Anorthoclase (1.85×10^6 years)
Locality: Olduvai Gorge, MK Site.
Stratigraphy: 44-inch unsorted lapilli and crytal vitric reworked nueé-type ash, 18 feet 5 inches above hominid floor.

KA 1042 Anorthoclase (1.92×10^6 years)
Locality: Olduvai Gorge, FLK I Site.
Stratigraphy: 12-inch cream colored tuff immediately under *Zinjanthropus* floor. Sample, is weathered, soft, contains bone fragments and numerous *Zinjanthropus*-time root holes.
Remarks: Same sample as KA 851, KA 1042 having been treated with HF.

KA 1043 Anorthoclase 1.76×10^6 years
Locality: Olduvai Gorge, MK Site.
Stratigraphy: 129-inch *nueé-ardente* ash immediately overlying hominid remains. Very fresh.

KA 1044 Anorthoclase (1.84×10^6 years)
Locality: Olduvai Gorge, MK Site.
Stratigraphy: 13-inch buff unsorted lapilli and crystal vitric tuff. Top of bed is 20 inches below bottom of the first marker bed of "Marker Bed A" (R. L. Hay, 1963). Presence of lapilli indicate contamination of older debris.

KA 1045 Plagioclase 1.64×10^6 years
Locality: Olduvai Gorge, MK Site.
Stratigraphy: 5-inch gray crystal vitric tuff. Top is 15 inches below first marker bed of "Marker Bed A" (R. L. Hay, 1963).

KA 1046 Plagioclase (1.86×10^6 years)
Locality: Olduvai Gorge, MK Site.
Stratigraphy: 1½-inch fine lapilli crystal vitric tuff just beneath first marker bed of "Marker Bed A" (R. L. Hay, 1963). Presence of lapilli suggests contamination.

KA 1047 Plagioclase 1.86×10^6 years
Locality: Olduvai Gorge, FLK I Site.
Stratigraphy: 3-inch crystal vitric tuff 12 inches above *Zinjanthropus* floor.

KA 1048 Anorthoclase (2.77×10^6 years)
Locality: Olduvai Gorge, pre-*Zinjanthropus* Site.
Stratigraphy: 8-inch light buff tuff whose base is 20 inches above basalt. Rounded quartz grains present in concentrate.

KA 1050 Plagioclase 1.66×10^6 years
Locality: Olduvai Gorge, Second Fault area.
Stratigraphy: 58-inch lapilli tuff. This tuff is probably redeposited in water. It shows crude bedding but no rounding of particles. Probably very close to level of "Marker Bed A" (R. L. Hay, 1963) and is probably equivalent to those marker beds.

KA 1051 Anorthoclase 1.76×10^6 years
Locality: Olduvai Gorge, pre-*Zinjanthropus* Site.
Stratigraphy: 2-inch crystal vitric ash resting on 1-inch layer just above pre-*Zinjanthropus* floor. This appears to be tuff just below limestone of *Zinjanthropus* floor. Same sample as KA 850.

KA 1052 Anorthoclase (1.89×10^6 years)
Locality: Olduvai Gorge, Second Fault area.
Stratigraphy: Rounded pumice lapilli tuff, below marker bed of "Marker Bed A" (R. L. Hay, 1963). Rounding of pumice and presence of lapilli indicate derived and contaminated nature of this tuff.

KA 1053 Anorthoclase 1.76×10^6 years
Locality: Olduvai Gorge, Second Fault area.
Stratigraphy: Lowest *nueé ardente* appearing on both sides of gorge. Sample is pumice bombs picked from tuff mass. Base of nueé is either 20 feet or 32 feet above basalt, depending upon definition of limits of this *nueé ardente.*

KA 1054 Anorthoclase 13.2×10^6 years
Locality: On Theka-Garrish road, elevation 4100 feet, 183 miles from Garrish, Kenya.
Stratigraphy: Vatta Phonolite. Basal flow of late Cenozoic volcanic series. Considered to be equivalent of Kapiti phonolite, KA 651.

KA 1055 Anorthoclase (1.66×10^6 years)
Locality: Olduvai Gorge, Fifth Fault area.
Stratigraphy: 5-inch crystal vitric tuff 23 feet 11 inches above white marker bed in green bentonitic lake beds of Bed II. See KA 1038 and discussion in text.

KA 1057 Anorthoclase 1.75×10^6 years
Locality: Olduvai Gorge, DK Site.
Stratigraphy: Pumice bed near bottom of section exposed at DK Site. This bed immediately overlies the lowest living floor at DK I Site.

KA 1058 Anorthoclase 1.76×10^6 years
Locality: Olduvai Gorge, MK I Site.
Stratigraphy: Very hard tuff covering thin clay containing hominid teeth. Same stratigraphic level as KA 1057.

KA 1059 Anorthoclase (1.25×10^6 years)
Locality: Ol Orgesailie, Kenya.
Stratigraphy: Redeposited pumice crystal tuff just above land surface 11.

KA 1060 Phonolite (whole rock) 6.7×10^6 years
Locality: Leakey's Hill, Rift Valley, on Nairobi-Magadi road, Kenya.
Stratigraphy: Ol Esayeiti Phonolite. Pre-dates initiation of rift faulting.

KA 1061 Anorthoclase (1.11×10^6 years)
Locality: Kariandusi, Kenya.
Stratigraphy: 18-inch unsorted layer of lapilli and fine ash at level of hand-axe. Contains rounded pumice fragments.

KA 1062 Plagioclase 1.57×10^6 years
Locality: Olduvai Gorge, MK Site.
Stratigraphy: 19-inch soft lapilli ash. Sample from lower 10 inches. This bed lies between the two marker beds of "Marker Bed A" (R. L. Hay, 1963).

KA 1079 Anorthoclase (1.75×10^6 years)
Locality: Same as KA 1055.
Stratigraphy: Same sample as KA 1055, re-concentration. See KA 1038 and discussion in text.

KA 1080 Anorthoclase 1.85×10^6 years
Locality: Olduvai Gorge, ¼ mile west of Third Fault.
Stratigraphy: Tuff 8 feet below basalt. Same tuff as KA 1088, different crystal type.

KA 1082 Anorthoclase (1.92×10^6 years)
Locality: Same as KA 919.
Stratigraphy: Same sample as KA 919. Reserve portion of KA 919 treated with HF and re-screened. See text for discussion.

KA 1084 Anorthoclase (2.69×10^6 years)
Locality: Olduvai Gorge, MK Site.
Stratigraphy: 18½-inch buff bentonitic tuff, 6 inches above basalt. Sample from upper 9 inches. Redeposited.

KA 1085 Anorthoclase (1.75×10^6 years)
Locality: Olduvai Gorge, at Second Fault.
Stratigraphy: 18-foot tuff, *nueé ardente* or lahar. Several feet above "Marker Bed A" (R. L. Hay, 1963). Very prominent and can be seen on both sides of gorge. Field evidence suggests redeposition possible.

KA 1086 Anorthoclase 244,000 years
Locality: Same as KA 963.
Stratigraphy: Same sample as KA 963 but KA 1086 treated with HF.

KA 1087 Anorthoclase (1.55×10^6 years)
Locality: Olduvai Gorge, Fifth Fault area.
Stratigraphy: 5-inch crystal vitric lithic pumice tuff. 28 feet above white marker bed in bentonitic lake beds of Bed II. See KA 1033 and text.

KA 1088 Plagioclase 1.91×10^6 years
Locality: See KA 1080.
Stratigraphy: Same tuff sample as KA 1080, different type of crystal.

KA 1089 Anorthoclase 439,000 years
Locality: Cartwright Site, Rift Valley, Kenya.
Stratigraphy: 16-inch crystal vitric tuff with some lapilli. Contains numerous re-worked obsidian tools of Pseudo-Stillbay culture. Tuff appears to be of primary origin, though lapilli content creates degree of uncertainty. See text.

KA 1090 Anorthoclase (4.4×10^6 years)
Locality: Same as KA 918.
Stratigraphy: Same rock sample as KA 918, different concentrate.
Remarks: Gross disagreement of KA 918 and KA 1090 indicate presence of contamination. See text.

KA 1100 Plagioclase and Augite 1.92×10^6 years
Locality: Same as KA 927.
Stratigraphy: Same basalt sample as KA 927. Rock sample crushed, treated with HCl for ½ hour and HF for 5 minutes. Essentially all chlorite and glass removed.
Remarks: Best estimate of age of Olduvai basalt. See text.

KA 1104 Anorthoclase 0 years
Locality: New Guinea.
Stratigraphy: Erupted in 1956 A.D.

KA 1137 Sanidine 83,000 years
Locality: Same as KA 464.
Stratigraphy: Same sample as KA 464, but treated with HF.

KA 1162 Sanidine 1.14×10^6 years
Locality: Fagianello (Viterbo). Mte. Cimino, Italy.
Stratigraphy: Sanidinite, product of Volcano Cimino.

KA 1170 Anorthoclase 5.01×10^6 years
Locality: Olduvai Gorge.
Stratigraphy: In area of its occurrence, this welded tuff lies upon pre-Cambrian basement forming base of Plio-Pleistocene tuff series. If this tuff occurs in the region of the hominid sites, it is at an undetermined depth.

KA 1175 Sanidine 422,000 years
Locality: Same as KA 408.
Stratigraphy: Same sample as KA 408. "Tuff with black pumices" KA 1175 by improved procedures. See text.

KA 1179 Anorthoclase 1.70×10^6 years
Locality: Same as KA 924.
Stratigraphy: Same sample as KA 924, but KA 1179 run by improved procedures.

KA 1180 Anorthoclase 1.85×10^6 years
Locality: Same as KA 1039.
Stratigraphy: Same sample as KA 1039 but stronger treatment with HF in order to completely remove glass from crystal surfaces. See text.

KA 1181 Sanidine 1.18×10^6 years
Locality: Northern slopes of Mte. Cimino.
Stratigraphy: Sanidinite, product of Volcano Cimino.

KA 1184 Basalt (whole rock) 1.61×10^6 years
Locality: Valros, France.
Stratigraphy: Overlies Astian/Plaisancian, underlies Villafranchian.
Reference: Kloosterman, 1960.

KA 1185 Sanidine 431,000 years
Locality: Same as KA 345.
Stratigraphy: Same sample as KA 345 ("tuff with black pumices") but run by improved procedures. See text.

KA 1188 Basalt (whole rock) 1.3×10^6 years
Locality: 3,500 feet north, 4,100 feet west of southeast corner section 27, T 4 S, R 10 E. Bennett Mountain Quadrangle, Elmore County, Idaho.
Stratigraphy: Bruneau Basalts, interbedded with *Mammuthus*-bearing sediments.
Reference: Malde and Powers, 1962.

KA 1197 Sanidine 49,000 years
Locality: Same as KA 269.
Stratigraphy: Same sample as KA 269, but run by improved procedures. See text.

Comments

By William Bishop☆

Kampala, Uganda. 21 xii 64

One cannot but welcome this contribution by Evernden and Curtis, which presents a detailed description of their irreproachable laboratory techniques coupled with an appendix of 127 dated specimens ranging from later Tertiary to late Quaternary age.

However, when they stray from the actual process of dating and discussion of probable sources of error in raw material into the field of stratigraphy and thence into vertebrate palaeontology, they are on less sure ground. Although I appreciate that it may be deliberate devil's advocacy on their part, I find the attitude that all potassium-argon dates be accepted as correct unless they have been proved incorrect rather premature. Particularly is this odd as the validity of K-Ar dates for rocks of less than 30×10^6 years still remains unproven in the view of many geologists.

It is interesting that of the 51 dates from Olduvai Gorge (Table 5) only 19 (Table 6) satisfy geological criteria for meaningful ages. Among these acceptable dates there is a remarkable consistency in the ages obtained for the 5 *nuée ardente* samples, suggesting that with ideal dating material excellent results are obtainable. All but 2 of the remaining acceptable dates are from within Bed I and suggest a range of 1.85×10^6 to 1.65×10^6 years for this bed. At present there seems insufficient dating evidence to suggest a time span for Bed II.

There is only 1 "acceptable" lava age in the Table 6 series (KA 1100, $1.92 \pm .06$), which was obtained after Hay had pointed out that stratigraphical evidence, in the form of the fresh lava tumuli and ropy surface of the pahoehoe lava beneath dated Bed I sediments, made an age as old as 4.4×10^6 years improbable. An interesting point is that although lavas and hypabyssal intrusive rocks are accepted as yielding reliable pre-Tertiary ages, these rocks seem singularly unreliable if dates reported for late Cainozoic tuffs within East Africa are accepted as a firm basis for comparison.

Despite the age range for the 6 Rusinga "Lower Miocene" dates (from 163×10^6 to 14.6×10^6 years), the writers accept an average age, of $15.2 \pm 1.5 \times 10^6$ years, obtained from KA 336 (14.6) and KA 336R (15.9), for the Kiahera Series.

In support of this they note, with others, that an assignment to the "Lower Miocene" is not required by the fossil data. This seems to be based upon a rather unusual view of the faunal assemblage. If in addition the age of the late Miocene Fort Ternan assemblage is accepted as being 14.0×10^6 years (KA 427) a complete faunal turnover in all groups of vertebrates represented at Fort Ternan and considerable evolutionary development is demanded within a maximum period of 3,000,000 years.

I find it preferable to continue to accept a broad age of Burdigalian for the Rusinga and other similar assemblages, based upon the vertebrate fauna. It is probable that the date of 21.7×10^6 years (Rusinga specimen KA 656) is close to the true radiometric age of the Kiahera Series. This would be in keeping with an age of $19 \pm 1.5 \times 10^6$ years obtained by Damon (Bishop 1964) for an identical Miocene assemblage at Napak, Uganda. The dates on samples KA 336 and 336R are as likely to be subject to some error, possibly argon leak, yielding too young an age, as are the dates on specimens KA 800, KA 801, and KA 802, as a result of contamination by basement biotite. It is important that other East African Burdigalian sites and other levels on Rusinga Island be investigated to provide comparative dates. At present it might be said that 14.6×10^6 years probably represents a *minimum* age for this assemblage.

The dates established in the Gregory Rift can only be reviewed when many more samples have been run. However, the lava-dating problem is seen again in comparing dates for KA 650, 652, and 661. A flow dated at 2.4 $\times 10^6$ years apparently overlies one of 0.8×10^6 years. Similarly the necessity of obtaining suitable, uncontaminated subaerial crystal tuffs is underlined by the range of "too old" dates obtained for the Olorgesailie and Kariandusi lake deposits. The value of the Olduvai area as a laboratory in which to experiment with the consistency of Quaternary potassium-argon dates probably lies in the subaerial nature of many of the deposits. The conclusion that the Olorgesailie site is younger than 486,000 years seems to be derived more from archaeological than radiometric evidence.

With the above cautionary remarks concerning the post-Bed I Olduvai dates; the lavas versus the tuffs; the Rusinga-Fort Ternan evidence; and the Rift Valley lacustrine deposits; the following points seem called for at present in modification of the cultural sequence listed on pp. 357-60 and in the Introduction (p. 343):

1. It has been shown by a large number of dated specimens that Bed I Olduvai (which seems remarkably rich in raw material suitable for dating) and the Oldowan culture probably span a time interval of 1,850,000 to 1,650,000 years. Detailed evidence for Bed II and later horizons remains to be established.

2. Various authors have shown that the term Kafuan is no longer valid as referring to a tool-making tradition

and that pre-Oldowan should be used in preference.

3. It is to be hoped that further potassium-argon dating will be undertaken of samples already controlled by C-14 dates. This will enable the claims of the authors concerning the reliability of potassium-argon dating of rocks as young as 30,000 years B.P. to be assessed. At present the hope that the accuracy of potassium-argon dating "will be superior to that of radiocarbon in the same age range" lacks substance. The apparent inaccuracy of potassium-argon dating of Stillbay sites by comparison with typological or C-14 chronologies suggests the Kenya Eastern Rift as a good starting point for further potassium-argon investigations.

4. The phrase "presently suggested correlation" between African Pluvials and European Glacials should surely read "previously suggested correlations." On a basis of stratigraphical investigations in East Africa carried out during the last 10 years many workers and the Pan-African Congress on Prehistory have underlined the danger of making such correlations. The value of radiometric dating lies in the hope that in the near future tentative correlations may be established based upon radiocarbon and potassium-argon evidence.

5. The time equivalent in Europe of the deposits assigned in their type area to the Gamblian Pluvial has not yet been established. Radiocarbon dating seems capable of clarifying this, and potassium-argon dating may also contribute to the problem. If the potassium-argon dates prove to be reliable, it is possible that lacustrine deposits in the Elmenteita-Nakuru Basin may be shown to range back farther in time than has previously been acknowledged.

6. The *Proconsul* bearing beds at Rusinga *and other sites yielding identical faunas* urgently require further investigation. Particularly is this so in relation to the date of 14,000,000 years suggested for the Fort Ternan assemblage. At present the palaeontological chronology appears more valid than that based on potassium-argon dating.

7. A possible age for the initiation of rift faulting has been established for *1 section* of the Eastern Rift Valley.

Only brief reference need be made to Hay's excellent preliminary note. Further work has now been carried out, and a detailed monograph will shortly supersede the *Science* paper with some modification of earlier findings.

With Hay's work the stratigraphy of Olduvai Gorge rests for the 1st time on a sound geological basis which permits correlation between the major sites. Also it suggests realistic palaeoecological reconstructions. It is to be hoped that the palaeontological and archaeological data will be reviewed within this framework. With further careful collecting of samples for radiometric dating related to the stratigraphy, an absolute time-scale may emerge for the whole sequence exposed in the Gorge and indeed for many other volcanic areas within East Africa. As Evernden and Curtis suggest, it is possible that in 5 years' time few questions concerning the East African cultural and stratigraphical time-scales will remain.

However, their provocative paper reveals that many major problems exist at present.

By C. LORING BRACE☆

Santa Barbara, Calif., U.S.A. 6 xi 64

Hay's previously published 1st hand account of the stratigraphy of Olduvai Gorge is very important, but Evernden and Curtis' demonstration of a dating technique, reliable for the entire time span during which the major events of human evolution have taken place, is of the greater significance. Some of the information, particularly on the argon extraction procedure, is of a technical nature which is beyond the range of experience of most anthropologists and would seem somewhat out of place in a journal catering to general anthropological interest. Still, because of the crucial importance of this work, the material discussed is of great interest and raises some questions which anthropologists might like to see treated. The following is one such tentative list:

1. What laboratories are currently equipped to perform such procedures? Can one expect more of them in the near future?

2. How much does the analysis of a given sample cost?

3. What controls should the field archaeologist exert in collecting samples for analysis? Perhaps as a matter of routine a geologist should be required to collect the samples in order to insure that reliable uncontaminated specimens are acquired.

4. What kind of theoretical accuracy figures can be regularly quoted? Carbon-14 dates are regularly published with standard deviation figures appended, which give the users some kind of perspective relating to their application; hopefully, some similar sort of procedure is possible for K-Ar dates.

5. What plans have been made for the dating of strata in areas where crucial but controversial hominid fossils have been discovered—e.g., the Transvaal, the Koro Toro region of the Chad basin, the Lake Natron area, and the Djetis and Trinil sequences in Java? While 1 date is quoted for central Java (KA 433) which agrees with the date derived from tektites (Koenigswald 1962), this is only a tantalizing hint and gives no indication of the length of time which elapsed during the deposition of this and the equally important earlier layers.

These and many other questions of application remain, but in spite of this, it is apparent that a major technical obstacle, long a plague to students of the prehistoric, has been overcome. The authors are to be heartily congratulated.

By J. DESMOND CLARK☆

Berkeley, Calif., U.S.A. 21 xii 64

Evernden and Curtis have done prehistorians a signal service by their refinement of the potassium-argon method of dating and its adaptation to suitable deposits of Pleistocene age, and by providing a time-scale for the Neogene primate and earlier stages of hominid cultural evolution they have made infinitely more understandable the time depth that lies behind the complementary changes discernible in the fossil and cultural record.

The age of 1.85×10^6 years for the base of Bed I at Olduvai is well attested by the samples and evidence given, as is also the comparatively rapid rate of deposition of Bed I, from 1.78 to 1.65×10^6 years for samples above the *Zinjanthropus* floor. The height of the samples from the tuffs that overlie the DK I site is not given, but, if they are from the consolidated tuff that immediately seals the site, then they are surely lower stratigraphically than the *Zinjanthropus* floor horizon. The samples, however, give younger dates (e.g., 1.70-1.76 as against 1.86 and 1.78 for samples immediately above the *Zinjanthropus* floor). The date of the highest sample in Bed I, $1.57 \pm 0.5 \times 10^6$ years, shows that the time span involved for the deposition of this bed is 0.28×10^6 years, which is more consistent with Hay's geological evidence than the earlier estimates suggested.

It is unfortunate that uncertainty exists as to the stratigraphic position of the sample giving an age of 1.1×10^6 years, and it is a matter of considerable importance that this should be checked again in the field and, if possible, further samples collected for dating. If this sample is really from the lower part or base of Bed II and does not date derived biotite from Bed V, as Hay suggests, then it indicates that the *Homo habilis* form from lower Bed II (site MNK II) took approximately 600,000 years to evolve into *Homo erectus*, a form represented at Olduvai by the Chellean III skull from LLK II dated to 490,000 years. The very slow rate of cultural evolution as exemplified at Olduvai Gorge is also in keeping with the long time interval involved.

Incidentally, the authors refer to "the so-called Kafuan (pre-Oldowan) tool-making tradition." It has been conclusively shown by Bishop (1959) that the "Kafuan Culture" at the type sites in the Kafu and Kagera valleys in Uganda does not exist as such, but is composed of a mixture of naturally fractured specimens and intrusive, much later, artificially worked pebbles. The term "Kafuan Culture" or "Kafuan tradition" is now meaningless and has been generally discarded by African prehistorians, as has the notion that there was an initial stage of culture when the toolmakers flaked their artifacts only on 1 side and that it was much later that stone was worked from 2 directions. How the stone was worked can, of course, be shown to be dependant upon the *shape* of the raw material, and both unifacial and bifacial forms occur together on sites of the Oldowan and later cultures. This is not, of course, denying that any earlier stage of culture than that present in Bed I at Olduvai existed. Most surely it did, but it has not yet been found and identified and it should not be called "Kafuan."

I have serious doubts about the dates for the so-called Pseudo-Stillbay, for the Kenya Stillbay, and for the Magosian from Deighton's Cliff. The Pseudo-Stillbay is, so far as I am aware, represented at only 3 sites on the Kinangop (Leaky 1936:49; Cole 1964:197). 2 of these are Wetherill's and Cartwright's sites, a few miles distant from each other overlooking the Naivasha basin. Cartwright's has also yielded an industry at 1st described as "Early Aurignacian" (Leakey 1936: 54) and later known as Basal Kenya Capsian. Later Leakey informed me (personal communication) that this blade industry might be intrusive or in beds of later age. Geologists at the 1947 Pan-African Prehistory Congress expressed the view that all these beds, at both Wetherill's and Cartwright's sites, that lie in the edge of and overlook the Kinangop rift scarp, were not of the same age as the sediments truncated by the rifting.

The Cartwright's site "Basal Kenya Capsian" industry is based upon blades, the Pseudo-Stillbay on flakes and discoid cores; neither has been precisely described nor comprehensively illustrated. Personal examination confirms Leakey's differentiation of the Pseudo-Stillbay industry from the Stillbay on technical grounds, and typologically the tools include a number of bifacial and unifacial points and single (rare) and convergent scrapers. So far as the scrapers are concerned they would not be out of place in a late Acheulian assemblage similar to those from Isimila or Kalambo Falls in Africa or the Jabrudian of the Levant. Such an age might now also be justified for the "Basal Kenya Capsian" in view of the subsequent discovery of the "Amudian" and "Pre-Aurignacian" blade industries that antedate the Mousterian and Levallois-Mousterian in the Levant and Cyrenaica and are contemporary with Jabrudian finds in the former region. The K-Ar results of 440,000 and 557,000 from tuffs at Cartwright's and Wetherill's sites are still, however, much earlier than is consistent with a late Last Interglacial age such as is ascribed to the Jabrudian and Amudian. The Pseudo-Stillbay with its small discoid prepared cores would certainly be quite out of context in an older cultural horizon. As a prehistorian, therefore, I find these dates unacceptable until a geological examination of the stratigraphy and precise description of the industries clarify the position. Since, so far as I am aware, obsidian is used only at 1 Acheulian or older site in Kenya (Kariandusi) and most of the known obsidian deposits are of later Pleistocene age, it might be of interest to date the obsidian, which is of 2 different kinds, from which the artifacts are made. If the result is younger than the age obtained for the tuffs, then there is clearly something wrong with the latter sample.

So far as the date of 240,000 for the Kenya Stillbay is concerned, this runs contrary to the cultural data, to the known stratigraphic position of the Stillbay variant of Middle Stone Age culture in sub-Saharan Africa, as well as to several consistent C-14 dates for the Stillbay and contemporary stages of culture there. For example: Upper Aterian (Dar-es-Soltan) $>$ 27,000 and $<$ 30,000 years (U.C.L.A. 678B; (Flint and Deevey n.d.); Upper Lupemban (Kalambo Falls) 27-29,000 years (Ohlson and Broecker 1961); Middle Pietersburg (Olieboompoort) 25,000 years (Flint and Deevey 1960); Stillbay (Twin Rivers, Zambia) 22,800 $\pm$ 1000 (Fergusson and Libby 1964); Middle Mazelspoort Variant (Florisbad) 29,000 years; Upper Mazelspoort Variant, 19,600 $\pm$ 700 years (Meiring 1956); Pomongwe (Rhodesia) 42,000 $\pm$ 2300, 35,550 $\pm$ 780, and 21,700 $\pm$ 400 for developing stages Robins and Swart 1964); and one could cite others. If the Kenya Stillbay from the Malewa Gorge really is a Stillbay—and there is no reason to doubt this though, again, no precise analysis of the assemblages from any Kenya Stillbay sites has yet been published—then the sample from which the K-Ar date was obtained is clearly contaminated. However, it is possible that the tuff from which the sample came and the sediments beneath it might be older deposits than the sediments that yield the Stillbay and Lower Kenya Capsian industries as exposed lower down the gorge. If, however, unquestionable Stillbay tools have been recovered from the deposits immediately underlying the tuff, then there is no alternative but to consider the K-Ar result suspect.

At Deighton's Cliff the nature of the industry beneath the tuff quarried for building stone is absolutely clear—it is Magosian. The Magosian is stratigraphically later than the Stillbay, and on C-14 evidence it dates to the close of the Pleistocene: Kalambo Falls (Zambia) 9550 $\pm$ 210 years (Ohlson and Broecker 1961); Pomongwe (Rhodesia) 15,800 $\pm$ 200 years (Robins and Swart 1964). The 2 dates for contemporary, equivalent cultures in the Congo basin are Mufo (Angola), Lower Tshitolian, 11,189 $\pm$ 490 years; Calunda (Angola), Lower Tshitolian, 12,970 $\pm$ 250 years (Fergusson and Libby 1963). The K-Ar dates of 270,000 and 100,000 are unacceptable to prehistorians, therefore, and, indeed, the authors themselves do not consider these results to be satisfactory.

While there is consistency in the older dates obtained by the K-Ar method, the later ones referred to above are inconsistent with the cultural and stratigraphic evidence from Africa generally, as it is currently known. It now behoves the prehistorians concerned to re-examine the cultural and stratigraphic evidence and, by precise description and analysis, to confirm or amend the previous classification of the industries to which these K-Ar dates apply.

By Paul E. Damon

Tucson, Ariz., U.S.A. 20 xi 64

Evernden and Curtis are deserving of praise for having led the way toward the establishment of a meaningful Late Cenozoic potassium-argon chronology. There is hope now that the potassium-argon method will bridge the long-standing gap in Pleistocene geochronometry and successfully overlap chronologies established by other methods, possibly even the carbon-14 chronology for the last 45,000 years. The patience and skill required to determine K-Ar ages in this time range can best be appreciated by geochemists who are also working with the method. Perhaps the dangers may also be more apparent to other geochemists than to anthropologists.

In my opinion, the authors have not demonstrated the non-existence of pre-eruption argon in volcanic minerals. They have set limits on the amount of pre-eruption argon (excess argon) in certain cases, but they have obscured these limits by the treatment of the data. On the basis of K-Ar dating, it may be possible to objectively set a limit of < 10,000 years on the Ischia Bay flow (KA 403), but a more precise statement is unwarranted. Establishment of a "zero" K-Ar age is a technical impossibility, and the quoting of "zero" ages is philosophically questionable.

It is probably correct to say that all minerals contain excess argon, though the amount may be trivial in many cases. In some cases the amount is not trivial. I will ignore clathrate-like minerals which have been shown to contain between 1×10^{-5} Scc/g and 3.2×10^{-2} Scc/g of excess argon (Damon and Kulp 1958). Feldspars and micas are not clathrate-like minerals. However, osumilite (volcanic "cordierite"), a clathrate-like mineral, is a not uncommon constituent of volcanic ash. Pyroxenes from originally deep-seated metamorphic terrains may contain large amounts of excess argon, $< 2 \times 10^{-7}$ Scc/g to 1.5×10^{-4} Scc/g (Hart and Dodd 1962; Gerling *et al.* 1962; Mc Dougall and Green 1964). A pyroxene, augite, was present in sample KA 1100. Lippolt and Gentner (1962) have demonstrated the occlusion of small quantities of excess argon in fluorite (1×10^{-7}, 4×10^{-7} Scc/g). In this laboratory, plagioclase from a pegmatite in a hypabyssal quartz monzonite was shown to contain 5×10^{-7} Scc/g argon. This does not establish the level of excess argon in eruptive volcanics, but the fact should be noted that Cherdyntsev and Kozak (1949) have measured 2.6×10^{-5} Scc/g of excess helium in magnetite from a volcanic bomb associated with the Kliuchevskii volcano on Kamchatka.

Selection of datable samples by hindsight rather than foresight is a somewhat subjective procedure and may obscure phenomena which might otherwise be observed. For example, only 19 of 51 K-Ar dates are selected for the Olduvai Gorge chronology. I have no quarrel with the selection criteria, but they should also be applied to KA 1050 (redeposited), KA 1046, and KA 1047 (both contaminated). It is interesting that the discordant series presented below could be ordered and placed in the right time range by the assumption that these minerals contain 1/10 the excess argon observed in the above plagioclase, i.e., an amount equal to 2.3×10^{-12} moles/g Ar^{40} (5×10^{-8} Scc/g). (See Table 1.)

TABLE 1
DISCORDANT SERIES ORDERED AND PLACED

SAMPLE NUMBER	MINERAL	K %	K-Ar DATE (10^6 YEARS)	HYPOTHETICAL DATE (10^6 YEARS)
KA 1038	plagioclase	0.814	2.18	0.57
KA 1087	plagioclase	1.46	1.55	0.65
KA 1055	anorthoclase	3.63	1.66	1.30
KA 1033	anorthoclase-plagioclase	1.43	2.00	1.12

The range of time obtained on this assumption is not unreasonable for Bed II. Contamination with older materials, suggested by the authors, is probably correct, but the example does serve to demonstrate an alternative explanation which should not be glossed over. Large variations in the amount of excess inert gas have been demonstrated in the case of the clathrate-like minerals and in pyroxene. Excess helium varies by over a thousandfold in beryl (Damon and Kulp 1958). Until a range of variations of excess argon is established for minerals in eruptive volcanic rocks, the possibility of large errors remains for very young minerals or for minerals of very low potassium content. Certainly, the data should be handled in such a way as not to induce false optimism concerning the accuracy of the potassium-argon method for dating events during the Pleistocene glaciations. At present one may choose between the Pa^{231}/Th^{230} time scale of Rosholt *et al.* (1961), placing the Riss (Illinoian) at *ca.* 125,000 years, or Evernden and Curtis' K-Ar time scale, placing this glacial episode at *ca.* 400,000 years. The correctness of the K-Ar chronology in this time range is not yet self-evident.

Curtis and Evernden have provided the hope of a really precise time scale for the Pleistocene, but it is best to proceed cautiously without overconfidence. In the words of the Scottish bard, Robert Burns:

The best laid schemes o' mice an' men
Gang aft a-gley
An' lea'e us nought but grief an pain
For promised joy.

By R. L. HAY

Berkeley, Calif., U.S.A. 10 xi 64

My field stratigraphy in Olduvai Gorge during 1962 and 1964 provides a basis for commenting on a few of the Olduvai Gorge dates accepted as valid by Evernden and Curtis in Table 6.

Sample KA 664 (1.1×10^6 years) was first attributed to Bed I (Leakey, Evernden, and Curtis 1961) and then to Bed II (Hay 1963 and the present paper). Now it appears most likely to represent Bed V, of very late Pleistocene age. In 1964 I carefully examined the supposed collection site, pointed out to me then by Dr. and Mrs. Leakey. I could not find biotite in Bed II at this locality, but biotite is abundant in nephelinite tuff of Bed V, which here unconformably overlies Bed II. Additional search both up and down the gorge in this vicinity revealed no layer in Bed II which contains more than a small amount of sparsely disseminated biotite. If the sample locality was correctly identified, then KA 664 represents a sample of Bed V, and the 1.1×10^6 years date may represent contamination of young biotite with older biotite.

Within Bed I, an apparent conflict exists between the dates for KA 1180 and KA 1047 (1.85 and 1.86×10^6 years, respectively) and the date of the *nuée ardente* deposit. 5 dates from the latter deposit range from 1.70 to 1.76×10^6 years, averaging 1.75×10^6 years, and seem to provide 1 of the firmest dates in the Olduvai sequence. KA 1080 (1.85×10^6 years represents the ash-fall equivalent of the *nuée ardente* deposits, and KA 1047 is stratigraphically higher than KA 1080 and the *nuée ardente* deposit. Thus, if the age of 1.75×10^6 years for the *nuée ardente* deposit is accepted, then the 1.85-1.86 $\times 10^6$ years dates must be considered about 100,000 years too old. The *nuée ardente* deposit lies only 2-3 feet above the lowermost hominid remains and artifacts known from Bed I, at sites DK and MK; its ash-fall equivalent underlies the more complete hominid remains (*Zinjanthropus* and *Homo habilis*) from site FLK. Thus, the oldest recognized evidence of hominid occupation at Olduvai Gorge appears closer to 1,750,000 than 1,850,000 years old.

By D. M. HOPKINS☆

Menlo Park, Calif., U.S.A. 14 xii 64

Anthropologists and Quaternary geologists owe an enormous debt of gratitude to Evernden and Curtis for their pioneer work in adapting and refining the potassium-argon dating method for use in establishing time-scales for the evolution of mankind and for the Quaternary Period. The 2 papers reviewed here are of especial value because they remove all remaining doubt as to the great antiquity (*ca.* 1.75×10^6 years) of the earliest hominid remains at Olduvai Gorge. The new and more fully documented analytical data given by Evernden and Curtis, together with Hay's re-examination of the physical stratigraphy at Olduvai Gorge, should answer the questions raised by von Koenigswald, Gentner, and Lippolt (1961) and by Straus and Hunt (1962). Not only are the radiometric and stratigraphic data from Olduvai Gorge internally consistent, within the present limits of precision for potassium-argon determinations, but they are consistent with Evernden and Curtis' age determinations on early and middle Pleistocene rocks from other parts of the world.

Further confidence in the general validity of the age determinations for Olduvai Gorge results from the determination by Grommé and Hay (1963) that the 1,900,000-year-old basalt at Olduvai Gorge crystallized at a time when the earth's magnetic field was normal or oriented as at present. Cox, Doell, and Dalrymple (1964) base the Olduvai normal palaeomagnetic event within the Matuyama reversed palaeomagnetic epoch upon this lava and show that lava flows in North America that are approximately 1,900,000 years old also crystallized at a time when the earth's field was normal, but that slightly older and slightly younger lava flows crystallized at times when the earth's field was reversed.

My remaining remarks are concerned with Evernden and Curtis' paper. Their discussion of the problems and techniques of sample collection, preparation, and analysis is most useful. I should like, however, to know exactly what is meant by the standard deviation figure given after some of the age determinations, how it is calculated, and why standard deviations are given for some determinations but not for others (compare, for example, KA 412, KA 436, and KA 437, Table 3). Clarification is also needed for the statement (p. 349) that "... factors leading to discordant results are presence of contamination and improper sample preparation, both factors being of significance only in loose tuffs." Dalrymple (1964*a*) has shown that important amounts of contaminating older argon can be retained in xenocrysts and xenoliths (foreign crystals and inclusions of older rocks) in lava flows. A recent restudy of the Bishop Tuff, in which age determinations are based upon K-Ar analyses of pumice lumps that are free of xenoliths and xenocrysts, has shown the formation to be only about 0.7×10^6 years old (G. B. Dalrymple, A. Cox, and R. R. Doell, written communication, 1964). The 0.98×10^6 years age determination for the Bishop Tuff given by Evernden and Curtis (Table 8; also see Evernden *et al.* 1964) evidently is spuriously old because of the unrecognized presence of contaminating argon in the many inclusions of granite and microcline that are found in most specimens of welded-tuff from the Bishop Tuff.

Evernden and Curtis' other age determinations for specimens that can be related to local glacial events in Africa, Europe, and North America are useful, but their suggested correlations of glacial events cannot be taken seriously. For example, it has been recognized for several years that the Wisconsin Glaciation of North America is *not* equivalent to the combined Würm and Riss Glaciations of Europe and that the Wisconsin Glaciation did *not* begin 250,000 years ago. Radiocarbon dating in Europe and North America, reviewed by Flint and Brandtner (1961), indicates that the Wisconsin and Würm Glaciations are precisely equivalent to one another and that this complex climatic event began about 70,000 years ago and ended about 10,000 years ago. The Wisconsin = Würm Glaciation was preceded by a warm cycle, the Sangamon or Riss/Würm Interglaciation, which evidently is represented by interglacial lava flows, raised beaches, and warm-water foraminiferal faunas in deep-sea cores dated in many parts of the world as being approximately 90,000 to 110,000 years old (Blanchard 1963; Dalrymple 1964*b*; Emiliani 1964; Ericson, Ewing, and Wollin 1964; Hopkins 1964; Rosholt *et al.* 1961; Thurber, Broecker, and Kaufman 1964).

An earlier glaciation that took place between about 100,000 and 150,000 years ago, represented by dated glacial till in northwestern Alaska (Blanchard 1963; McCulloch, Taylor, and Rubin 1965) and by cold-climate foraminiferal faunas in deep-sea cores (Emiliani 1964; Ericson, Ewing, and Wollin 1964; Rosholt *et al.* 1961) represents at least part and perhaps all of the Illinoian Glaciation of North America and the Riss Glaciation of the Alps; it seems likely that the Illinoian = Riss Glaciation began considerably later than 250,000 years ago.

Attempts to correlate earlier components of the "standard" glacial sequences of north-central United States and the Alps and to relate early and middle Pleistocene events in other areas to 1 of these sequences are so hazardous and speculative that they are almost without value at the present time (and I speak with the fervor of a reformed sinner!). For example, when various investigators can suggest that the till beneath the Bishop Tuff may be as young as the Illinoian Glaciation of north-central United States (Putnam 1960) or, on the contrary, older than the Donau Glaciation of the Alps (Emiliani 1964:136), an age determination for the Bishop Tuff obviously contributes little or nothing to the establishment of a time-scale for either "standard" sequence.

Furthermore, it may be incorrect to assume that either the Alpine or the American "standard" glacial sequence is complete, containing a record of every significant Pleistocene glaciation or interglaciation. Primary among my several reasons for suspecting that the "standard" sequences are incomplete is the fact that a preliminary analysis of geologic, palaeomagnetic, and palaeontological studies carried on in Iceland last summer by Thorleifur Einarsson (geologist, University Research Institute of Iceland), R. R. Doell (geophysicist, U.S. Geological Survey), F. S. MacNeil (palaeontologist, U.S. Geological Survey) and me suggests the possibility that as many as 10 and perhaps 12 distinct glaciations, separated from one another by interglacial episodes, may have taken place during the last 3,000,000-3,500,000 years and that all of these may lie within the Quaternary Period as defined in the Italian type sections (Movius 1949).

Only when radiometric age determinations are available for deposits closely bracketing or unambiguously assignable to the Donau, Günz, Mindel, and Riss Glaciations of the Alps or to the Nebraskan, Kansan, and Illinoian Glaciations of north-central United States will these 2 sequences become useful standards of reference for glacial and interglacial events in other areas. In the meantime, radiometric age determinations such as those presented by Evernden and Curtis permit us to undertake the more important task of establishing the frequency and timing of Quaternary climatic, tectonic, volcanic, and evolutionary events without requiring that we try to warp individual events into possibly incomplete and still undated "standard" glacial sequences.

By F. Clark Howell☆

Berkeley, Calif., U.S.A. 21 xii 64

My comments will be restricted to some consideration of the relevance of age determinations by the K-Ar technique for certain aspects of palaeoanthropological studies and/or some matters of Pleistocene subdivision and correlation. Any evaluation of technical procedures is utterly beyond my own competences. However, the reliability of the technique has been fully demonstrated for Tertiary (and older) rocks; its refinement for age determinations on more recent rocks evidently can provide largely consistent results.

1. Some previous age determinations (Curtis and Evernden 1962; Leakey *et al.* 1961) on minerals in volcanic products (primary tuffs) from Bed

I, Olduvai Gorge, are surely confirmed by this newer, more extensive series of measurements. 5 determinations on samples from a *nuée ardente* yield 4 figures of 1.75-1.76 $\times$ 10^6 years, and 1 figure of 1.70 $\times$ 10^6 years. This deposit overlies, by rather less than a meter, the hominid occupation places at DK I. and MK I.; an ash fall equivalent [1] underlies the FLK I Main ("Zinj") and FLK NN I (ex-"pre-Zinj") sites (R. L. Hay, personal communication). Some 1.75 $\times$ 10^6 years seems to be the best approximation of an age for the earlier Bed I hominid occurrences and occupations.

There are few satisfactory age determinations for the middle/upper sediments of Bed I. Presumably the best estimates pertain to "Marker Bed A," which overlies by several meters the aforementioned FLK I Main occupation places. These are samples 1062, 1050, and 1045.[2] These values fall between 1.57 (within the Marker Bed sediments) and 1.64-1.66 $\times$ 10^6 years (equivalent to or below this horizon). There is, however, a very substantial accumulation of sediments between this horizon and the overlying "Marker Bed B" tuffs which delineate Bed I from Bed II.

If the basalt within Bed I is dated at some 1.91-1.92 $\times$ 10^6 years (samples KA 1100, 1088; but cf. KA 1080),[3] then the time elapsed between it and "Marker Bed A" is some 260,000-350,000 years (rather than simply 350,000 years). Sample KA 664 (1.1. $\times$ 10^6 years) seems to date 1 of the infrequent biotite occurrences in lower Bed II; and hence the authors' tentative assessment of 400,000 years for time elapsed between "Marker Bed A" and lower Bed II.

Aside from the problems which remain, the available K-Ar determinations have an important bearing on matters of hominid phylogeny. 1 australopith taxon, *Australopithecus (Paranthropus) robustus—Zinjanthropus, Paranthropus*, or *Australopithecus robustus* for other authors—is *at least* about 1,750,000 years of age. However, this taxon has a substantial temporal duration, since: (1) it occurs in the Peninj beds, northwest Lake Natron, with a mammalian fauna which includes elements of *upper* Bed II affinity (L. S. B. Leakey, personal communication) and where palaeomagnetic evidence indicates an age of $\geq$ 1,000,000 years (G. Isaac and R. L. Hay, personal communication); and (2) it is also represented, in my opinion (and also that of J. T. Robinson and A. A. Dahlberg) at the BK II site, now known to be a channel filling of late upper Bed II age, cut well down into lower Bed II. The morphological distinctiveness and long-term persistence of this taxon, surely *at least* specifically and probably subgenerically (even generically?) distinct from (*Australopithecus*), is now more than adequately documented.

This same taxon is represented in southern Africa at the Swartkrans and Kromdraai sites. No chronometric methods are appropriate for assessing the ages of these occurrences. On purely parsimonious grounds it is hard to avoid the conclusion that the Swartkrans-Kromdraai occurrences fall somewhere within the time span of this taxon at Olduvai Gorge. However, the known temporal duration is substantial—very probably well over 1,000,000 years.[4]

Whatever the "early Middle Pleistocene" may be—in northerly latitudes it is often regarded as the Cromerian + the Mindel complex [5]—there is no reason now why this taxon should not be much earlier not only at Olduvai, but also at Swartkrans. Kurtén (1957; 1960*a*; 1962) has forcefully argued that, at the latter site, this taxon *must be* of such "earlier Middle Pleistocene" age because of the occurrence there of the hyena genus *Crocuta*, a form of presumed Asian origin (ancestry recognizable in the Pinjor zone, Upper Siwaliks, in *Crocuta sivalensis*) which was dispersed over Europe and Africa in the course of the Cromerian faunal stage. However, perhaps not expectedly, *Crocuta* aff. *ultra* is now known to occur within the Bed I faunal assemblage, chronometrically assessed as greatly exceeding 1,000,000 years in age!

The occurrence of a hominid distinct from this taxon is now adequately demonstrated in Bed I, just as it has been previously documented at Swartkrans in southern Africa. Coexistence in the immediate habitat is demonstrated at the former locality, at the site of FLK I Main, with (*Paranthropus*) *robustus* and another different hominid which is represented by isolated (upper and lower) teeth and (probably) cranial vault fragments. The morphological transition from (*Australopithecus*) *africanus* [6] to *Homo erectus*, nicely documented (in my opinion) by the MK I → NN I → FLK I Main → MNK II series of hominid remains (i.e., so-called *Homo habilis*), is less well documented temporally in terms of chronometric determinations. The earlier occurrences are *africanus*-like and the subsequent occurrences are remarkably *erectus*-like. It will be most interesting to see where the remains from the new Maiko Gully site (above Marker Bed B, at the base of Bed II) fit in terms of this seeming transition. This presumed transition, perhaps occurring over 1,000,000 years, seemingly involved relatively slight changes in cranium and dentition, and perhaps only minor (recognizable) alterations in behavioral capabilities, at least as discernible in stone artifacts and, maybe, food debris at occupational situations.

Ages of this magnitude should not really be surprising in the light of either stratigraphic or palaeontologic evidence for earlier ranges of the Quaternary. The extensive continental and littoral marine sedimentation,

[1] There are some ambiguities in determinations from samples KA 1039 and KA 1180 on this crystal vitric lapilli tuff (from Pelean eruptions probably, according to R. L. Hay). The original determination (KA 1039) gave a figure of 1.74 $\times$ 10^6 years, quite comparable with the *nuée ardente* figures. However, with enhanced pretreatment (hydrofluoric acid) another determination (KA 1180) gave a figure of 1.85 $\times$ 10^6 years. Similarly KA 1047, about 30 centimeters (*ca.* 12 inches) above the FLK I Main ("Zinj") occupation surface gave an even greater figure, 1.86 $\times$ 10^6 years. Presumably both this latter sample and KA 850 (1.78 $\times$ 10^6 years) have given too high values (that is, even higher values than the lapilli tuff and *nuée ardente* which stratigraphically *underlie* both samples).

[2] KA 1046 gives a value (1.86 $\times$ 10^6 years) substantially exceeding that for the stratigraphically *lower* lapilli tuff and *nuée ardente*.

[3] It may be even older in view of palaeomagnetic studies. The remnant magnetism of the Olduvai basalts is normal (present polarity). This has been interpreted, however, as representing a peak of normal geomagnetic polarity (Grommé and Hay 1963) during a protracted epoch of reversed polarity (Cox *et al.* 1964; also 1963), termed the Matuyama reversed epoch, between *ca.* 1,000,000 and 2,500,000 years. Such a normal peak, about 1,700,000–1,900,000 years, is also now known from western North America. The basal Quarternary/end-Tertiary, between *ca.* 2,500,000 and 3,500,000 years, by contrast was a period of normal geomagnetic polarity (termed Gauss normal epoch). In France, as Roche (1963) has shown, volcanic rocks postdating the Villafranchian stage exhibit normal polarity (termed Brunhes normal epoch), whereas previously, throughout the earlier Villafranchian stage, polarity is reversed.

[4] If one accepts the age determination (*ca.* 500,000 years) of KA 405, and the probable occurrence of the taxon subsequent to this age (at the BK II site).

[5] But perhaps, as Kurtén (1960*b*) has suggested, best regarded as that range of Quaternary time and the "Günz Glacial."

[6] The occurrences of (*Australopithecus*) *africanus* at the Sterkfontein Main site and Taung(s) may well represent a still earlier phase of the (*Australopithecus*) lineage, at least on morphological grounds and, as I have previously maintained, on faunal associational grounds as well. It is interesting and provocative that both occurrences lack any positive artifactual evidence of capabilities (manifest in stone) for implement use or manufacture.

orogeny, tectonics, and volcanism of that range of time have been noted for many years, not only in the better-studied areas of Europe, but also in the Maghreb of Africa and in the southerly foothills of the Himalayas. Those investigators acquainted with this range of the Quaternary have long noted its very substantial duration by comparison with the so-called glacial Pleistocene.

Vertebrate palaeontologists in particular have recognized the complexity of mammalian faunal assemblages referred to the Villafranchian stage. Depéret (1885; 1893; 1909) long ago distinguished differences, presumably correlative with substages (but worthy of stage rank by comparison with other Pleistocene faunal "stages"), within the Villafranchian complex—thus, post-Plaisancian/Astian (originally substage A) and pre-St. Prestian (originally substage B). More recently the complexity of the European Villafranchian has been emphasized by several investigators. Dietrich (1953) has distinguished 2 main substages: (1) post-uppermost Pliocene, and (2) pre-Cromerian (Mosbachian). Azzarioli (1953) distinguished 4 principal substages within the European Villafranchian complex. Viret (1954) recognized 3 principal substages, the middle of which he regarded as further subdivisable. Kretzoi (1956), on evidence from both southwestern Europe and the Pannonian basin, distinguished 3 (maybe 4) substages. Bourdier (1961) recognized 4 main substages in southwestern Europe, principally France. Bout (1963; also 1960) has recognized 3 principal substages in the classic Villafranchian faunal localities in the middle/upper Allier and upper Loire basins. In the Oltenia/Moldavia and Transylvanian portions of Rumania, Samson and Radulesco (1963) have recognized 3-4 main subdivisions of the Villafranchian sedimentation sequence. And most recently Kurtén (1963; also 1960*b*), on the basis of varied evidence throughout Europe, has recognized 6 major substages of the European Villafranchian; and he suggests there are data to indicate a damp → drier → damp → drier → damp → drier succession within this range of time.

These conclusions of some highly competent vertebrate palaeontologists are offered here only as an indication of a sample of thought on this subject. It is even more appropriate in the light of very new K-Ar determinations (on tuffs) of over 3,000,000 years for a "Basal" Villafranchian occurrence (*Equus*, no *Elephas*) and *ca.* 2,500,000 years for a "Middle" Villafranchian occurrence, both in southern France (G. H. Curtis, personal communication). This is still in keeping with the previous determination (KA 1184), on basalt, from Valros (Agde) below Villafranchian faunal bearing sediments and above end-Pliocene (Plaisancian/Astian) marine beds. In fact if an "Upper" Villafranchian faunal zone[7] in eastern Africa, as represented in Bed I, Olduvai Gorge, might be thought to be broadly equivalent with analagous occurrences in Europe, which is possible though dubious, then all these determinations are internally consistent with what might now be expected from still earlier ranges of (palaeontologically-defined) Quaternary time.[8]

Varied palaeobotanical studies have already established climatic vegetational changes at several European Villafranchian faunal localities in eastern England (Norfolk), in the Netherlands (Tegelen), in northern Spain (Villaroyo), in Germany (Wölfersheim), in Poland (Mizerna), in Tuscany (Val d'Arno), and in Lombardy (Leffe). And something of vegetational conditions is also known elsewhere in France (Senèze, Perrier, Saint Vidal, Ceyssac, La Roche-Lambert, as well as, among others, Paulhaguet, Bannat, Vallone, du Riou, Joursac, Murat, Lac Chambon, Cladel, Gurp, and Meximieux).

These are usually incomplete segments of earlier Quaternary time. However, in the Netherlands samples (including the Tegelen section), derived from borings, 3 major cold phases (termed Praetiglian, Eburonian, and Menapian, from oldest to youngest), separated by substantially warmer phases (Tiglian and Waalian), occur above the end-Pliocene (Reuverian) and below the Cromerian Interglacial (Zagwijn 1960, 1963; Sluijs and Zagwijn 1962). A less complete succession from Suffolk and Norfolk, in which presumably the basalmost elements of the Quaternary (Red Crags mostly) are missing, indicates (reading upwards) temperate (Ludhamian) → cold (Thurnian) → temperate (Antian) → cold (Baventian) stages prior to the Cromerian Interglacial (West 1961, 1963; Funnell 1961; Funnell and West 1962). And the Leffe vegetational succession, now more fully known as a consequence of further work by Lona (1963; also 1950; Lona and Follieri 1957) reveals 4 principal cold stages preceding, and 4 following, what seemingly represents the Cromerian Interglacial stage (grouped 1-2/3/4-major interglacial [=? Tiglian]-5-6-7-8). The evidence for these and some other complexities of the basal Quaternary, and their possible pertinence for australopith ages, can only be examined in some detail elsewhere. Perhaps the important point is that general failure to recognize the climatic complexity of the Basal Quaternary has led some workers to make simplistic assumptions about Pleistocene climatic episodes and hence has produced too recent correlations for the australopiths from southern Africa.

2. *Homo erectus*, in upper Bed II (site LLK II) at Olduvai Gorge, appears to be about 500,000 years old. Such an age is of the same order of magnitude as 2 determinations on leucite basalt (samples KA 433 and KA 433R) from the Muriah volcano complex, northern central Java, which relates to tuffs (at Pati Ajam) yielding a Trinil type mammal fauna. These determinations, 500,000 years, along with those made on the same rocks in the Max-Planck-Institut für Kernphysik (Heidelberg), which range between 595,000 and 435,000 years (= 495,000 ± 100,000-60,000 years), provide an age for the latest Javan occurrence of *Homo erectus* (Koenigswald 1962).[9]

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[7] Only a small part of the very rich vertebrate fauna recovered from Olduvai Bed I has as yet been identified (cf. the forthcoming book by Leakey *et al.*). The available evidence to date would seem to indicate an assemblage *less archaic* than that from Kanam, and perhaps, also, that from the (middle) Kaiso series, Albertine Rift Valley, and *more archaic* than that from the immensely rich Omo beds in southern Ethiopia. The species identified and published so far include: Proboscidea (*Deinotherium bozasi, Elephas* cf. *africanavus*, primitive *Elephas? recki*); Equidae (*Stylohipparion albertense, Equus oldowayensis*); Suidae (Hippopotamus sp.; *Ectopotamochoerus dubius, Potamochoerus intermedius, Promesochoerus mukiri, Tapinochoerus* sp., *Phacochoerus altidens robustus*); Giraffidae (*Okapia* cf. *stillei, Giraffa* sp., *Libytherium olduvaiensis*); Rhinocerotidae (*Ceratotherium* cf. *efficax*); Chalicotheriidae (*Metaschizotherium* cf. *hennigi*); Canidae (*Canis africanus, Thos mesomelas, Otocyon recki*); Hyaenidae (*Crocuta* aff. *ultra*); Felidae (*Panthera* aff. *crassidens*; Machairodontinae indet.); Mustelidae (*Lutra* sp.); Cercopithecidae (*Simopithecus* indet., *Papio* sp., Cercopithecidae indet.); Bovidae (*Strepsiceros maryanus, Hippotragus gigas, Damaliscus antiquus, Gorgon olduvaiensis, Parmularius altidens, Beatragus antiquus, Gazella* cf. *wellsi*); Leporidae (*Lepus* sp., *Serengetilagus* sp.); Hystricidae (*Hystrix* sp.); plus other unidentified forms of some of these families, many rodents, insectivores, birds, reptiles, amphibians, and fish, all still under study.

[8] The Blancan Mammal-Age, the upper part at least of which is temporally equivalent with the European Villafranchian Stage-Age on comparative faunal grounds, yields ages between *ca.* 3,500,000 and 1,500,000 years (6 samples from California and 1 from Idaho) (Evernden *et al.* 1964). The Irvingtonian Mammal-Age, to be equated on similar grounds to the upper part of the (European) Villafranchian, yields an age of 1,360,000 years. Hence these are all consistent age assessments on sediments which are palaeontologically Quaternary, but which substantially antedate the Pleistocene as traditionally defined on climatic criteria.

[9] Studies of the radiogenic argon content

These age assessments should be compared with those K-Ar determinations from the lower Rhine Valley and the Latium (Rome region):

(*a*) phonolite tuff (KA 264), derived from the Laachersee (Eifel) region and stratigraphically related to the final depositional phases of the younger Main Terrace (Jüngere Hauptterrasse) of the lower Rhine, has yielded an age of 370,000 years (correction to *ca.* 390,000 years suggested by Frechen 1959). On grounds of stratigraphy and heavy-mineral analysis, this formation is equivalent with the *termination* of the Netherlands Weert zone (Sterksel Formation). Hence it postdates the Cromerian Interglacial stage (Zagwijn 1963; also Frechen 1959; review in Brelie 1959) and is equivalent with the initial phase(s) of the Elster Glacial stage.

(*b*) From the Latium, age assessments are available for a series of 7 samples relating to eruptive products (tuffs with black pumices) of the Bracciano volcano (Sabatino group). This volcano commenced its eruption after the Sicilian (marine) stage and its initial products (granular tuffs) overlie both Calabrian and, sometimes, brackish Sicilian sediments (Blanc 1957). Cold climatic conditions are indicated by palaeobotanical, palaeontological, and sedimentary evidence. The term Flaminian has been given to this stage. A subsequent renewed eruptive phase produced the distinctive "tuff with black pumices." Subsequently there was a time of colder climatic conditions, designated Nomentanan, prior to the Tyrrhenian (II) (= Monastirian) marine stage, i.e., the Last Interglacial. Consequently these distinctive eruptive products *predate* the Nomentanan (equated by Blanc with the Riss Glacial) and *postdate* the Flaminian (equated by the same investigator with the Mindel Glacial).

Now 5 of these determinations are surprisingly, and reassuringly, close in time—KA 1185 and KA 304 (Torre Pietra), 431,000 years; KA 334 (Torre in Pietra), 434,000 years;[10] KA 408 (Cava Nera Molinario), 432,000 years (however, the same sample rerun [KA 1175] gave 422,000 years); and KA 345 (same as KA 1185, Torre in Pietra), 438,000 years—that is between 431,000 and 438,000 years. Another sample, KA 407 (Cava del Cecio), gave a slightly lower value, 417,000 years.

The age range for the "tuff with black pumices" is relatively brief, about 20,000 years, and this occurrence represents a remarkably good stratigraphic marker in the Latium area. This age range exceeds somewhat that determination for the presumably earlier phases of the Elster (= Mindel) Glacial complex in the lower Rhine valley. Does this eruptive event in the history of the Sabatino group, referred to the Flaminian Glacial, antedate the Elster Glacial complex? Or does the Rhine valley age determination really represent not the earlier (or earliest) phases of the Elster Glacial complex, but a subsequent "middle" phase? Moreover, these figures from the Latium are approximately comparable with those relating to the Trinil faunal-bearing brecciated tuffs in central Java. Is it not possible, even likely, that the Trinil faunal zone extends, as von Koenigswald (1962:117, Abb. 1) suggests, from "Günz times" through "Mindel times"? Might not then the middle-upper sediments of Olduvai Gorge Bed II represent the "Günz + Mindel"? If such may be the case, what is the correlative position of the well-defined Cromerian Interglacial stage of the temperate latitudes? Obviously the matter cannot now be resolved, but it is important to recognize the existence of discrepancies between current stage/age concepts and correlations suggested by these age assessments.

3. The authors present several determinations which might be thought to provide very substantial ages for human lithic industries of demonstrable Upper Pleistocene age:

(*a*) Samples KA 417, 458, and 459 are from a crystal vitric tuff, presumed to be a primary deposit, which overlies a Magosian industry at Deighton's Cliff, Kenya Rift Valley. At this locality, exposed in a section of the Makalia River valley which drains into the Nakuru basin, occur lacustrine varved clays and shallower-water silts and sands, with Kenya Stillbay artifacts, of late Upper Pleistocene age. These are overlain by subaerial beds, including stony horizons and aeolian sand deposits, representing an old land surface; it is here that the Magosian occurs. The thick (up to *ca.* 6 meters) consolidated tuff which overlies the latter has been quarried for construction purposes.

The determinations on the tuff gave ages of 276,000-273,000 years and 97,500 years. As the authors state, these assessments are internally inconsistent and *do not* provide an age for the tuff. The Magosian is known of course in a number of parts of southern Africa, as well as elsewhere in eastern Africa, and occurs stratigraphically *above* Middle Stone Age industries established to be of late Upper Pleistocene age, i.e., within the past 25,000-30,000 years. There are at least 2 radiocarbon determinations on horizons which yield Magosian: 9,550 ± 210 years (B.P.) for Stoneline 1a at Kalambo Falls, Zambia (Lamont, L-395D), and 15,800 ± 200 years (B.P.) for this industry at Pomongwe cave, Matopos Hills, Southern Rhodesia (Salisbury, SR-11).

(*b*) 1 determination (KA 963, 2 runs) is on anorthoclase from a pumice lapilli/crystal tuff, superposed to sediments yielding Kenya Stillbay artifacts,[11] at Malewa Gorge, Naivasha basin. The sample is considered not to be contaminated by older tuff crystals.

Naivasha, now a fairly shallow semi-freshwater lake, occupies the southernmost of 3 small basins in an elongate trough in the central Kenya Rift Valley (cf. summaries in Cooke 1958; Flint 1959). These now dischar-

of tektites (12 specimens) from a widely dispersed field in southeast Asia (between Indo-China and Australia, and including Indonesia) have yielded K-Ar ages of *ca.* 710,000 years (Zähringer and Gentner 1963). In Java these occur in quantity in the upper part of the Kabuh beds, e.g., at Sangiran with typical Trinil type fauna (Koenigswald 1960). The leucite age determinations provide a more accurate assessment of the age of these beds, and hence (late) *Homo erectus,* than do the tektite figures which afford at least a *maximum* age. At any rate it is evidently a matter of an age of $\leq$ 700,000 years–very substantially less than the figures for the earlier Quaternary.

10 It should be noted that these determinations do *not* pertain directly to the age of the sediments in which the (Middle) Acheulian site of Torre in Pietra occurs. The age assessments relate to volcanic products *older* than the Acheulian horizon; the latter sediments, which indicate various signs of cold climatic conditions, do contain *derived* tuff with black pumices. Thus, the author's statement that "field evidence suggests a close correlation in time between the occupation of the living site and eruption of the pumice" is rather misleading and should not be taken literally.

11 Several Stillbay occupations in Pomongwe cave, Matopos Hills (Southern Rhodesia) have been determined by C-14. The age assessments are: 42,000 ± 2,300 years (B.P.) (SR-9) for earliest Stillbay; 35,530 ± 780 years (B.P.) (SR–39) for "typical" Stillbay; and 21,700 ± 400 years (B.P.) (SR-10) for "developed" Stillbay. A Middle Stone Age occupation at Holley Shelter, Wartburg (Natal) has a C-14 determination of 18,200 ± 500 years (B.P.) (BM-30). And another, referred to the Late Pietersburg, at the Cave of Hearths, Makapan Valley (Transvaal), yielded a C-14 determination of 15,100 ± 730 years (B.P.) (Chicago, C-925). Also, a travertine, related to Middle Stone Age occurrences at Twin Rivers Kopje (Zambia) afforded an age of 22,800 ± 1,000 years (B.P.) (UCLA-229). Hence the K-Ar determination of KA 920 is fully in agreement with what is known thus far of the "absolute age" of Stillbay and related Middle Stone Age industries in southern Africa.

geless basins are tectonic in origin, being perhaps of Middle Pleistocene age, and were obstructed by volcanic dams to the north (Menengai) and south (Suswa and Longonot). The volcano Ol Doinyo Eburru separates the northern pair of basins (Nakuru-Elmenteita) from the single, larger southern basin (Naivasha). The floors of the basins are filled with a variety of Pleistocene lacustrine sediments (diatomites, clays, silts), fluviatile gravels, and volcanic tuffs. These are visible in sections exposed by the erosion of tributary streams—for example, the gorge of the Malewa (sometimes called Murendat) River which drains the western slopes of the Aberdares and the lower-lying Kinangop Plateau. Fairly continuous strandlines, probably slightly warped, are represented by several sorts of shore features, quite high above both northern and southern basins. As few as 3 (Solomon 1931) to 6 (Nilsson 1932) or as many as 25 (in 7 groups) (Nilsson 1940; also 1963) strandlines have been distinguished. The upper lacustrine sediments, exposed for example along another tributary, the Little Gilgil River, and along the lower Malewa River, especially opposite Government Farm, contain (Developed) Stillbay artifacts, sometimes in stony horizons representative of old land surfaces.

The southerly outflow of the Naivasha basin, cut into Middle/Upper Pleistocene sediments, was once into the Kedong Valley via Njorowa Gorge (or Hell's Gate), an irregular erosional valley over 200 meters deep. 2 phases, at least, of outflow are recorded, with attendant erosional activity. Now the age of some of the cemented tuffs, forming a portion of the dam which seems to have blocked this basin, has been ascertained (KA 920) as 29,000 years. However, an age determination of a tuff (KA 963) which *overlies* sediments presumably retained (in part) by this dam is 230,000 years! Are the stratigraphic relations here other than what they have been thought to be? Or might this presumably primary tuff contain mineral contaminants from Middle Pleistocene volcanics?

(*c*) 2 determinations have been made on minerals from volcanic deposits related to occurrences of the so-called Pseudo-Stillbay industry. These occurrences, Cartwright's site (KA 1089) and Wetherall's site (KA 921) are both situated along the Kinangop Plateau, above the Rift escarpment, along the eastern flank of the Kenya Rift Valley. It is worth noting certain disagreements as to the stratigraphic situation of these sites with reference to rift faulting.

At Cartwright's site, originally found in the course of excavations for house foundations, the occurrence of stone artifacts (both Pseudo-Stillbay flake implements and Fauresmith-like bifaces) is in silts with derived volcanic rocks. The locality is over 700 feet (*ca.* 7500 feet above sea level) above the oldest upper Pleistocene strandline of the Naivasha basin, and slightly below the Kinangop Plateau surface. A crystal vitric tuff, with some lapilli, which *seals* in the site has been determined as 439,000 years of age. At Wetherall's site, not far distant to the south, and about due east of the town of Naivasha, Pseudo-Stillbay artifacts occur on land surfaces, with pisolitic iron concretions, in relation to 2 successive marls (swamp deposits), separated unconformably by a volcanic ash. A welded tuff underlying the lowermost artifact occurrence has been determined to be of an age of 557,000 years.

There is a question as to whether the artifact-containing deposits antedate the main faulting of the Rift Valley here and constitute basal sediments capping the Kinangop Plateau, as Leakey (and Nilsson) have long maintained, or whether these are merely Upper Pleistocene swamp deposits on the margins of the plateau. Shackleton (1955) observed that

> along the edge of the Kinangop escarpment the implementiferous lateritic beds lie unconformably on the trachytic tuffs which form the escarpment. The lateritic beds are only a few meters thick. At Wetherall's (*sic*!) site they lie on the slopes of a wide shallow valley cut back into the plateau. The layers dip towards this valley about 3°. Subsequently the valley has been eroded more deeply. At another locality (Duries site) he recognised that "the laterites pass smoothly, on a 5° slope, over the edge, overstepping successive beds of the trachytic tuff beneath." He concluded that "thus it seems clear that an escarpment already existed when the lateritic beds were forming.

At Wetherall's site, where there are obvious unconformities, is it not likely that the age determination relates correctly to the underlying tuff, presumably of (earlier) Middle Pleistocene age? And at Cartwright's site is it certain that the overlying tuff is wholly primary, and not in part derived or contaminated, perhaps from the widespread, and stratigraphically lower tuff, which covers this flank of the Gregory Rift Valley?

The available data are inadequate to resolve either the relative age, generally taken to be "end-Kanjeran," whatever that now means, or the affinities of Pseudo-Stillbay assemblages. Aside from the 2 aforementioned occurrences there are, to my knowledge, at least 5 other occurrences of similar assemblages farther north along the Kinangop Plateau. The artifacts are usually made of obsidian, with other lavas being rarely utilized, although chert/chalcedony may occur infrequently. There are many small points, either squat or more elongate triangular, on flakes with prepared or simple platforms; not infrequently the platform may be thinned by secondary trimming, although unthinned, thick bases also occur. Secondary retouch is often unifacial, but bifacial retouch is not unusual. Some of the points have an elongate tip, with retouch along each margin distally to produce a small bec-like nose. Some assemblages have occasional notched flakes. And rare Levallois flake-blades and small squarish Levallois cores may also occur. Very small hand-axes have also been sometimes found in the same context as the flake implements.

It should be pointed out that present interpretations of continental Pleistocene successions, including the results of K-Ar age determinations, are surely not in agreement with late Cenozoic time-scales based on analyses of cores of deep-sea sediments (cf. Howell 1962). There are now principally 2 such time-scales—1, based on intensive studies of a few cores, developed by Emiliani (1955; 1958; also 1961; 1956) and others (Rosholt *et al.* 1961), and the other, based on a large suite of Atlantic cores, developed by the group at the Lamont Geological Observatory, Columbia University (Ericson, Ewing, and Wollin 1964; also 1963; Ericson, Ewing, Wollin, and Heezen 1961; Ericson and Wollin 1956 also 1964). The former indicates an extrapolated age of some 300,000 years for the earliest major Quaternary glaciation ("Günz" = "Nebraskan"). The latter indicates an age 5 times greater—1,500,000 years.

The (generalized) temperature curve from oxygen isotopic (O^{18}/O^{16}) analyses by Emiliani (*et al.*), in which both radiocarbon age determinations and protactinium/thorium (Pa^{231}/Th^{230}) measurements provide time estimates to *ca.* 150,000 years, is not necessarily inconsistent with continental stratigraphy—but, only if the major temperature minima (≐ glacials) and maxima (= interglacials) are equated differently. Thus the whole succession would extend from the termination (final Elster = Mindel complex) of the traditional earlier Middle Pleistocene, rather than from the beginning of "Günz," to the postglacial. In this case the curve, and its parts, are wholly consistent with both continental stratigraphy and available K-Ar age determinations on volcanic rocks.

The other curve, developed by Ericson *et al.*, bears no specific agreement with current knowledge of the Pleistocene succession. Oceanographic studies to undertake critical appraisals of the varied methods were employed to derive this succession and the palaeotemperatures. However, it is worth remarking that the analyses by these workers of the upper segments of some demonstrably young cores are in broad agreement with the oxygen isotopic analyses. Moreover other cores show an assemblage of changes in planktonic microfossils, which may be also paralleled in generally, but not precisely, the same ways in Pacific deep-sea cores (Riedel *et al.* 1963), which may well be taken to correspond to the Tertiary/Quaternary "boundary." However, to fill the gap between these cores of older sediment and those of the Upper Pleistocene is another matter. It is in regard to this aspect of the problem where the major difficulties seem to arise. Obviously we must leave to our colleagues in stratigraphic geology, geochemistry, and oceanographic studies to undertake critical appraisals of the varied methods employed to derive this succession, palaeotemperatures, and intra-core correlations.

By ADOLPH KNOPF

Stanford, Calif., U.S.A. 14 xii 64

The papers by Hay and by Evernden and Curtis brilliantly complement each other. The paper by Hay gives the chief results of his 8 weeks' field investigation at Olduvai Gorge. It clarifies the seeming discrepancy in the age dating of the deposits that contain the famous hominid remains. This dating had been made, on the 1 hand, according to orthodox geologic concepts, and subsequently it was made by the potassium-argon technique, which gives the age in years. It is the extreme antiquity indicated by the potassium-argon dating, nearly 2,000,000 years, that has so greatly astonished the world.

The paper by Evernden and Curtis on potassium-argon dating contains a large amount of new data of very great interest and importance. The authors themselves have presented 11 important conclusions, but readers will find that many other significant conclusions remain to be drawn. Undoubtedly, the most important result of this paper will be the recognition that the dates in years, the so-called absolute chronology of Pleistocene events, will necessitate a considerable revision of Pleistocene geochronology.

The lower boundary of the Pleistocene has long been, and still is, a matter of acute disagreement. In 1948 a Commission (Oakley 1949; King 1950) of 15 geologists appointed by the Council of the 18th International Geological Congress at its London session unanimously reported:

1. The Commission considers that it is necessary to select a type-area where the Pliocene-Pleistocene (Tertiary-Quaternary) boundary can be drawn in accordance with stratigraphic principles.
2. The Commission considers that the Pliocene-Pleistocene boundary should be based on changes in the marine faunas, since this is the classic method of grouping fossiliferous strata. The classic area of marine sedimentation in Italy is regarded as the area where this principle can be implemented best. It is here too that terrestrial (continental) equivalents of the marine faunas under consideration can be determined.
3. The Commission recommends that in order to eliminate existing ambiguities the Lower Pleistocene should include as its basal member in the type-area the Calabrian Formation (marine) together with its terrestrial (continental) equivalent the Villafranchian.
4. The Commission notes that according to evidence given this usage would place the boundary at the horizon of the first indication of climatic deterioration in the Italian Neogene succession.

These eminently logical recommendations were unanimously accepted by the Council.

They have also been widely accepted by many geologists and palaeontologists. On the other hand, they have been ignored by many, who continue to believe that the beginning of the Günz glaciation in the Alps marks the beginning of the Pleistocene. The Günz has been correlated with the Villafranchian, of necessity by a very roundabout way, and the correlation is therefore insecure.

On the basis of the Milankovitch astronomic theory, the Günz glaciation is thought to have begun 600,000 years ago. This figure was accepted by Stille and many others, but Emiliani (1956) thought it to be 300,000 years—to cite only 2 of several discrepant figures obtained by interpretation of the same data. Since Stille and many others believed the Günz to mark the beginning of the Pleistocene, they considered the Pleistocene to be 600,000 years long. Emiliani thought the Günz to be of mid-Pleistocene age; consequently he also estimated the length of the Pleistocene at 600,000 years.

However, the potassium-argon ages now available indicate that the Pleistocene is much longer than 600,000 years: it is about 2,000,000 years. It is of interest to recall that the eminent palaeontologist Schuchert wrote as long ago as 1936 that Stille's estimate of 600,000 years is too short; and that the length of the Pleistocene is at least 1,000,000 years and will eventually be shown to have been 2 or 3 times as long. This prediction is now amply verified by use of the high-precision potassium-argon technique of dating which has been brought to such perfection by Evernden, Curtis, and associates.

As shown by Woldstedt (1958: 300, 393) in his masterly monograph *Das Eiszeitalter* (unfortunately not consulted by most of those who write on the Pliocene-Pleistocene boundary), the concept of 4 ice or cold periods no longer suffices. Instead, there are 5 or 6 such periods. The newly recognized periods are earlier than the 4 classic ice "ages" recognized in the Alps by Penck and Brückner. They have been named (1) Brüggen and (2) Eburon by Woldstedt, and they are separated by a warm period called the Tegelen. The deposits that represent these new units occur in the Lower Rhine region, Holland, and adjacent Belgium. The associated oldest Pleistocene, the Amstelian, (Zagwijn 1957) rests upon the Pliocene and carries a Villafranchian fauna which indicates a cold or cool climate.

In conclusion, the paper by Evernden and Curtis will undoubtedly become a milestone in our progress toward understanding the Pleistocene.

By MIKLÓS KRETZOI☆

Budapest, Hungary. 17 xi 64

Researches during the last 2 decades in so-called absolute geochronology left a wide gap between the years 50,000 and 10,000,000. Several branchs of science, particularly that of human evolution, are very seriously affected by this deficiency. Therefore, from the point of view of geochronology, we consider the study by Evernden and Curtis of epochal significance, as they practically bridge over this gap with the help of new data gained by greatly improving the K-Ar method. The vast significance of their research justifies our emphasizing some of their statements and making comments on some others which bear relation to formations younger than 10,000,000-12,000,000 years.

1. The most important result of the new chronology is the great alteration in the respective durations of the Pliocene and Pleistocene to the cost of the former. Hitherto, under the paralysing influence of the Milankovich chronology, which estimated the time range of the Pleistocene at some 600,000 years, the earlier parts of the Pleistocene had to be abnormally reduced in length. This continued in spite of the fact that faunal examinations in recent years more and more tended to prove that between the age of the so-called Hipparion faunae and the end of the Lower Pleistocene (according to other nomenclatures, the lower part of the Middle Pleistocene, viz. the "Mindelian," a series of faunal changes took place (Kretzoi 1956; 1959; 1962). These changes required a much longer span of time than the subsequent time-units of the Pleistocene, as has been emphasized by

several authors (Kurtén 1960*c*; Földvári-Vogl and Kretzoi 1961; etc.). This deficiency was completely remedied by Evernden and Curtis when they placed the Pliocene-Pleistocene boundary at 2,000,000-3,000,000 years.

2. The time-scale for Barstovian-Clarendonian-Hemphillian is convincing and in accordance with geological and palaeontological evidence—in respect of North America. But—as the authors also point out—the divisions cannot be brought into correlation with the European stratigraphical standards. Consequently this involves a difference between the 2 continents in the place of the Pliocene-Pleistocene boundary as well. The difficulty is due to the ambiguous usage of the European terms. In order to help elucidate the problem I should like to add the following:

(*a*) In Middle Europe the Sarmatian means the brackish Upper Miocene overlying the Vindobonian (= Helvetian + lower part of the Tortonian). In the stratigraphy of Western Europe it is the synchronic facies of the Tortonian. In Eastern Europe it goes under the name of Lower Sarmatian (= Volhynian), the Upper and, in part, the Middle Sarmatian here being already a Hipparion-bearing "true" Pliocene.

(*b*) In Western Europe the Pontian (in the broadest sense) is usually placed at the top of the Miocene. In this concept the Pontian begins with the appearance of the Hipparion-wave in the Old World and embraces the period of Hipparion-faunae. The Eastern European classification roughly follows the above system with the difference that it uses the name Pontian in a restricted (original) sense, only for the upper part of the stage in the broad sense; for the lower and middle part of the Pontian in the Western European sense, here the Meotian stage and the Bessarabian substage of the Sarmatian are used respectively.

(*c*) The Middle European classification places the Pannonian stage = Pontian, *Sensu lato,* of the French stratigraphy) in the lower half of the Pliocene. Recent evidences show that in the Lower Pannonian the "Hipparion"-faunae are still devoid of Hipparions (Flinz-sands, Diósd in Hungary, etc.), whose invasion takes place only in the 2nd half of the Lower Pannonian (Kretzoi 1961).

(*d*) Finally, the Lower Pliocene of the North American classification is the Clarendonian (with Hipparions more primitive than even the most archaic species of the genus in Eurasia) which is followed by the Hemphillian (without true representatives of Hipparion).

These complicated interrelations are shown in Table 1.

TABLE 1
CORRELATION TABLE OF THE MIO-PLIOCENE

WESTERN EUROPE	CENTRAL EUROPE		EASTERN EUROPE		NORTH AMERICA
"Pontian"	Pan-no-ni-an	Upper	Pontian		Hemphillian
			Meotian		
			Sar-ma-ti-an	Bessarabian	Clarendonian
Tortonian		Lower		Chersonian	
	Sarmatian			Volhynian	Barstovian
	"Tortonian"		Konkian		

Miocene-Pliocene boundary
Arrival of Hipparion in Asia-Europe

3. The so-called Upper Pliocene (in Western Europe: Pliocene) seems to be the weakest point in the whole geochronological system. It goes under the names Plaisancian-Astian, Levantian, Dacian, Cimmerian, Kuyalnian, Blancan (in part), etc., without making us able to give a definite stratigraphical-chronological value to these terms, not mentioning their correlations. Therefore in this case an approach to an absolute chronology is a task of utmost importance to be hoped for from future research.

In this respect the examination of the material of Early Post-Pannonian basaltic eruptions in northwestern Hungary could yield data for the drawing of the boundary between the Lower and Upper Pliocene with absolute chronological methods. The question of the upper boundary could be solved with the examination of the younger basaltic generation lying on the northern border of the Pannonian Basin near Ajnácskö (Hajnáčka, southern Slovakia), the relative chronology of which is fixed by its famous Vertebrate fauna (Fejfar 1964). As the Blancan is separated from the Hemphillian by a world-wide erosional cycle, just as in the Old World the Upper Pliocene (Levantian, Plaisancian-Astian, Cimmerian, etc.) is separated from the Upper Pannonian, the solution of 1 problem will help solve the other.

4. According to the decision of the 1948 Geological Congress of London the Villafranchian stage has been referred to the Pleistocene as its lowest member (Movius 1949; Oakley 1950). During recent years, however, the content of the stage—as well as the exact place of the Plio-Pleistocene boundary—has undergone several changes. Important evidence has emerged against the identical age of the 3 stratotypal elements accepted as characteristic of the Lower Pleistocene (Villafranchian, Calabrian, and the 1st climatic deterioration in the Neogene of Italy), and it seems highly improbable that they even belong to the same stage. On the other hand, detailed examinations of rich and continuous micromammalian faunal successions have established that the so-called Valdarno fauna—lying on the supposed bottom of the Pleistocene—is the final member of a long and varied faunal succession which can by no means be separated from preceding members by a stage-boundary, still less by the Plio-Pleistocene boundary (Kretzoi 1956; 1959; 1962).

The absolute chronological method of the authors would be very helpful in solving these very complicated problems by means of examination of samples from basaltic eruptions securely dated in relative chronology with rich faunas (Ajnácskö, Perrier Mountains).

I could go on with this comment at length, but I do not aim at completeness; I only intend to show that some basic prerequisites for research on human descent have been provided by the brilliant method of Evernden and Curtis. But their method will become the effective absolute geochronology of the embraced time range when, after having tested the methodologically only too important African and isolated Upper Quaternary Italian samples (being geographically distant from the territory of the classical stratigraphical framework), they complete their work by dating the European and North American localities of stratotypal significance. I do hope we shall be soon provided with these important data as well.

Nairobi, Kenya. 21 xi 64

Hay's preliminary statement on Beds I-IV of Olduvai is excellent, and when the final report, including another season's work which has now been carried out, is published in volume form, it will provide an outstanding example of a study of a site with important Pleistocene stratigraphy.

My only disagreement with Hay is where he states towards the end of his article that "... the climate was relatively dry, at least seasonally, throughout most or all of the lengthy period ..."

This might be read to mean that the climate was dry relative to that which pertains in the Olduvai region today, but I understand from discussions with Hay that he did not intend to convey this meaning, and it is certainly not one that could fit the facts.

The position, as I see it, is that the climate at Olduvai was wet—indeed very wet—when compared with the climate of Olduvai as it is today, during a large part of the time we are concerned with. This can be shown by the presence, in the lake deposits, of fossil remains of fish, including *Tilapia*, crocodiles, and hippopotami, as well as many fresh water shells.

None of these could survive in the Olduvai region with the climate as it is today, still less so if the climate were drier.

On the other hand, the geological evidence suggests the presence of a relatively saline and somewhat shallow lake, such as those which occur in the Rift Valley further north today—Lake Elmenteita, Lake Nakuru, Lake Naivasha, etc. These are in an area where the climate is dry by comparison with *that in the surrounding mountain masses* and plateau, but not a dry climate at all. For example, the Elmenteita-Nakuru lakes, which contain hippopotami, are in a region with a rainfall of about 30-35 inches a year, whereas the higher country immediately to the east and west has a rainfall of 45 or more inches a year. Lake Naivasha, which has fish and hippopotami, is in an area with an annual rainfall of perhaps 35-40 inches, but still much drier than the neighbouring Kinangop Plateau.

An average rainfall of the same general order as that which maintains the present very shallow Nakuru and Elmenteita lakes, as well as the slightly deeper Lake Naivasha, would have been necessary to form and subsequently maintain the shallow, partly saline lake which Hay has proved existed at Olduvai from time to time. Otherwise it could not have had a fauna including fish, crocodiles, and hippopotami, fossil remains of which are so plentiful. There were, of course, also long intervals of desert conditions when the various aeolian deposits were formed.

Clearly the Olduvai region could have had the degree of rainfall needed to form and maintain a lake like the Nakuru group of lakes only if the climate of East Africa as a whole had had greatly increased precipitation compared with today, i.e., pluvial conditions. The rainfall of Olduvai at the present time averages about 10 inches a year.

The keynote of Evernden and Curtis' report may be found in their final sentence "5 years from now, few questions about the general time-scales of Pleistocene glaciation and of human cultural evolution will remain to be answered." This faith in the infallability of the K-Ar technique to the exclusion of all other evidence pervades the whole article. Added to this, the authors have not always taken the trouble to quote up-to-date literature (e.g., instead of making use of the many reports published in the past decade, they several times, directly or indirectly, quote the 1960 edition of *Adam's Ancestor's*, which, apart from the brief prologue, was clearly stated to be an unaltered print of the 1952 edition). This gets them into greater difficulties than they would otherwise have encountered.

I do not propose to reply to any of the matters concerning the dating of the Olduvai deposits, preferring to leave these to my colleague, R. L. Hay, in order to avoid unnecessary duplication. I will, however, deal briefly with other matters in which the conclusions do not seem justified in the light of the other evidence. Nor will I refer, item by item, to the statements which are summarised in the Introduction, but will deal rather with those which concern my field, in the order in which they appear in the text of the article.

The 1st matter for discussion, then, is the conclusion reached concerning the age of what are termed the "Rusinga Beds," and the suggestion that these are not of Lower Miocene age but "may be approximately 3,000,000 years older than the Fort Ternan fauna ..." From the text and from the tables it seems that this conclusion is based upon Sample KA 336, which is said to represent the "Kiahera Series." My recollection of collecting at Rusinga, when this and other samples were taken, is that some samples were obtained from the "Kiahera Series" in Shackleton's meaning of that term, while others were from deposits round Sites R.106, R.106A, and R.107. These lie at the foot of Kiahera Hill and are geographically close to Kiahera, but they are not necessarily part of the "Kiahera Series."

Some of the deposits in this area are strongly unconformable to the Kiahera Series and most probably belong to the Kathwanga Series, although they occur in the Kiahera area. When we were collecting samples, I mentioned the risk that some of the samples collected near Kiahera Hill might not belong to the "Kiahera Series," which Shackleton believes to be the oldest part of the Rusinga beds, though this is not yet proven. Therefore to use Sample KA 336 for dating the whole of the 1000-foot Rusinga sequence is quite unjustified. I believe that other samples from Rusinga gave dates of 25,000,000 and even of 40,000,000 years; these are not mentioned in the text.

To support the suggestion that the Rusinga Beds are about 3,000,000 years older than Fort Ternan, the authors state " 'Lower Miocene' is not required by the fossil data." I do not know what this statement is based on, but it is not valid, on the basis of the present known fossil evidence.

Some 2 years ago, Evernden suggested that the reported presence of such genera as *Sivapithecus, Mesopithecus, Pliohyrax,* and some Tenrecidae was suggestive of a date much later than Lower Miocene. I pointed out then that the specimen which had been tentatively called *Mesopithecus*, by MacInnes in 1942, was only thus listed in order to avoid creating a new genus on the basis of a single specimen. In 1962 I informed Evernden and his colleague Savage that it was not a *Mesopithecus*. I also pointed out that although Arambourg had provisionally listed *Pliohyrax* as present in East Africa, and although we had found specimens of a similar fossil hyrax at Rusinga and had therefore *provisionally* included *Pliohyrax jeannelli* in the 1951 lists, that genus had since been withdrawn and shown to be *Megalohyrax*, a much more primitive member of the family. Moreover the presence of Tenrecidae (which are today confined to Madagascar) supports, rather than negates, a Lower Miocene date. The *Sivapithecus* specimen had only been put in that genus very tentatively.

There is also another line of evidence which is contrary to the conclusions reached by Curtis and Evernden. They assign a date of 12,000,000-14,000,000 years to the Fort Ternan fossil beds: if this is correct, then the main Rusinga deposits with a much more primitive fauna cannot possibly be only some 3,000,000 years older. The palaeontological differences between the faunas of Fort Ternan and Rusinga are far too great for such a small interval. Moreover, as further identifications of the Rusinga collections are made available, it is evident that the fauna has many affinities with the Upper Oligocene of other parts of the world.

In the 1951 report quoted by the authors, the study of the Rusinga mammals had only just begun, and it was considered then that there might be no great difference in age between the fossils from different parts of the sequence. Now it seems likely that the beds cover a more prolonged period. *If the date for sample KA 336 is correct*, it may well be that a part of the deposits, including the Kathwanga series, are of Upper Miocene age. That would, however, not justify dating the whole 1000-foot sequence to this period, when the palaeontological evidence is to the contrary.

Turning next to that part of the paper headed "Time-Scale of Human Cultural Evolution" (pp. 357-60) and leaving the Olduvai matters to Dr. Hay, I will comment 1st on paragraph 5, the age of the *Pseudo-Stillbay* culture.

"A crystal vitric tuff containing some lapilli buries a Pseudo-Stillbay culture at Cartwright's site, Kenya." This is unquestionably a statement of fact. The authors believe that the tuff in question is "primary" and quite uncontaminated. It has yielded a date of 440,000 years, which on the authors' own showing elsewhere is more or less the same as that obtained from the "Abbevillian-Acheulian site at Torre, in Pietra, Italy."

While I have always maintained that the geological deposits in which the Pseudo-Stillbay specimens are embedded, along the edge of the Rift Valley, antedate the main faulting of the eastern branch of the Great Rift Valley in this region, I cannot accept that this culture is as old as the Chelleo-Acheul transitional period. Among other reasons, I must point out that at Wetherell's site and other similar sites which yielded quantities of specimens of the Pseudo-Stillbay culture *in situ*, the same beds have also yielded a few unrolled and apparently contemporary hand-axes of *very late* Acheulean type. This indicates that the Pseudo-Stillbay was contemporary with the final Acheulean, which is dated by C-14 at Kalambo Falls at *ca.* 57,000 years ago. It is definitely contrary to all the available evidence to place the Pseudo-Stillbay as contemporary with Chelleo-Acheulean transition and as far back as 440,000 years.

Dealing next with the Kenya Stillbay: Quantities of artefacts typical of this culture have been found, over the past 35 years, in the stratified lake silts, sands, and gravels exposed by the Malewa Gorge, Naivasha. These occur in the top part of the 150-foot thick sequence, where they are contemporaneous with tools of the Kenya Capsian. This is due to the fact that during the closing stages of the Gamblian Pluvial period, the Kenya Capsian and the Kenya Stillbay were coexistent in this area, with the former mainly concentrated in rockshelters and the latter mainly in open station sites.

At the place where the authors obtained the sample for dating, there is a "pumice lapilli and crystal layer" which has yielded a date of 240,000 years. The fact that this layer overlies the deposits containing Stillbay tools is not in dispute. But such a date for it is not acceptable, for a number of reasons. The sequence of beds at Malewa was laid down in the lake of the Naivasha basin, 200 ± feet deep, during Upper Gamblian times, when the lake had an outlet at Njorowa Gorge at the level of *the present floor of the gorge*. The fauna of these beds is closely comparable to that of the present day (with 1 major exception —Nilsson's buffalo). The explanation given by the authors about this outlet is, I believe, wholly incorrect. They state, "In very late Pleistocene times, long after the above-described events took place [i.e., the deposition of the bed dated to 240,000 years] Lake Naivasha was dammed by an eruption of tuff and lava. The lake rose, overflowed the lava, and cut a channel through the lava and tuff."

As one who carried out nearly 2 years' study of the Naivasha basin, partly alone, partly with Dr. Erik Nilsson, and later with Sir Vivian Fuchs, I must reject this view and give what I believe to be a more accurate version. After the main faulting of the eastern branch of the Great Rift Valley a line of volcanos and fissure eruptions developed diagonally across the floor of the Valley, beginning near Kijabe Hill and passing through Longonot, to continue further in a southwesterly direction. These eruptions took place in the early part of the Upper Pleistocene. As a direct result of this volcanic activity a closed basin was formed on the floor of the Rift Valley in the Naivasha region. This basin gradually filled up during the 1st phase of the Gamblian Pluvial to become the 1st Naivasha Lake, with a beach level of 400 ± feet. The water found an outlet at this level over the lowest part of the dam formed by lava and tuff. The lake thus became stabilised at 400 ± feet and cut well defined beaches. Beneath the lava and tuff forming the "dam", there were soft, porous beds, and the water gradually seeped through these and cut an underground channel which finally caused the overlying columnar lavas to collapse, along the line of the subterranean channel. Following this collapse, the water flowed through the collapsed area and formed a new outlet which had a floor level some 200 ± feet above the lake as it is today. A well defined new beach was then cut at this level, extending all round the basin. It was in this 2nd, 200 ± foot, lake basin that the deposits were formed which contain Stillbay culture throughout the Naivasha basin. Evernden and Curtis claim an age of 28,000 years for the tuffs "which formed the dam" and an age of 240,000 for deposits which appear to have been formed long after the dam was cut through! One of these dates must clearly be at fault. In any event, there are a number of carbon-14 dates (some of which the authors actually quote) for the Stillbay culture (or its equivalent) from all over Africa. It seems that these are more likely to be accurate, since they tally closely with each other although obtained from different areas.

The authors next present what they consider to be a valid geological argument to support the great age which they propose for the Pseudo-Stillbay. They refer to "a canyon . . . cut into these lavas . . . by a very small intermittent stream" at Cartwright's site (a similar situation is also present at Wetherell's site). It is true that the stream, today, is "very small and intermittent."

The authors envisage a tiny stream, like that of the present day, throughout what they believe was the past 440,000 years! My knowledge of the climatic changes indicates that even if the cutting of this canyon only started during the Gamblian Pluvial, it must have been cut by a large river, fed by melting snow and ice from the Aberdare Mountains, during a period of climate far wetter than that of today. The argument, therefore, that the depth of canyon cut by a "small intermittent stream" required 440,000 years to achieve falls to the ground.

Leaving aside some minor matters, we next come to the age of the Magosian culture at Deighton's Cliff. This is unquestionably a Mesolithic culture, often associated with simple pottery, and cannot reasonably be supposed to date as far back as 100,000 years, a date which is contradicted both by the cultural evidence and by a number of C-14 dates from various other sites.

Finally, I would point out that the suggestion that the Gamblian Pluvial, with its developed Levalloisian, Kenya Capsian, and Kenya Stillbay cultures, can be correlated with the Riss Glaciation in Europe, is wholly contrary to all the available cultural evidence.

Where dating by a new technique such as K-Ar is based upon a long series of samples, taken from a number of sites but at the same horizon (as for Bed I, Olduvai), and where the resulting dates are reasonably in keeping with one another and with the faunal, stratigraphical, and cultural evidence, the results may be accepted with caution. But where the results are at variance with all other lines of evidence and are based upon relatively few samples, the position is entirely different.

By Harold E. Malde

Denver, Colo., U.S.A. 2 xii 64

Evernden and Curtis (pp. 355-56) employ a pair of K-Ar dates from the United States (Bishop Tuff and Bruneau Formation) and a solitary K-Ar date from a Rhine River terrace to suggest that Nebraskan Glaciation in North America was older than Günz Glaciation in the Alps. I will comment briefly on the problem of relating glacial stages to the Bruneau Formation (1.36×10^6 years; Evernden *et al.* 1964:191, sample KA 1188), which Evernden and Curtis assume might be as old as Nebraskan. The Bruneau date comes from the youngest of several basalts in this formation, which consists of a rather complicated array of intertonguing lake beds and lava flows about 800 feet thick.

One line of reasoning, based on evidence from fossils and physical geology, indicates that the Bruneau Formation is post-Nebraskan.

Vertebrate fossils from the Bruneau come from beds stratigraphically below the dated basalt. C. B. Schultz, L. G. Tanner, and G. E. Lewis (Malde and Powers 1962:1211) report, in addition to *Mammuthus* as an indicator of Irvingtonian provincial age, a "distal fragment of humerus of *Gigantocamelus* sp. of Aftonian to late Kansan age," and parts of *Equus* sp. comparable in size to *E. giganteus* "hitherto reported only from Yarmouth to mid-Wisconsin rocks." D. E. Savage also identifies *Paramylodon.* From these few vertebrate remains, the Bruneau might be as old as Nebraskan, but a younger age fits the presently known ranges of the fossils better.

Correlation of the Bruneau Formation with glacial chronology must also reckon with its physical position in the geologic sequence (see Table 1).

The Bruneau occupies a canyon nearly 1000 feet deep that was cut after erosion of the Glenns Ferry Formation and deposition of the Tuana Gravel, a thick blanket of coarse alluvium. Deposition of Tuana Gravel on the voluminous fine-grained beds that characterize the basin sediments, and its subsequent entrenchment by a deep canyon, are impressive geologic events that cannot be ignored in estimating the age of the Bruneau. Was this coarse debris deposited in response to Pleistocene climatic change? If so, Nebraskan Glaciation is more likely represented by the Tuana than by any part of the Bruneau.

TABLE 1

Outline of Quaternary Stratigraphy in Western Snake River Plain

Stratigraphic Unit	Description
Melon Gravel	Flood debris deposited by catastrophic discharge from Lake Bonneville, about 30,000 years ago, as determined by several radiocarbon dates.
Lava flows	Fresh basalt and pillow lava.
Crowsnest Gravel	Outwash from glaciated mountains in central Idaho. Forms terrace 200 feet above Snake River.
Lava flows	Basalt in ancestral Snake River canyon.
Sugar Bowl Gravel	Outwash from glaciated mountains in central Idaho. Forms terrace 400 feet above Snake River.
Lava flow	Basalt in ancestral Snake River canyon.
Black Mesa Gravel	Pediment deposit older than Snake River canyon and 550 feet above Snake River.
Bruneau Formation	Canyon fill 800 feet thick of lake beds and lava flows. Youngest basalt dated 1.36×10^6 years by K-Ar.
Tuana Gravel	Alluvial deposit locally 200 feet thick on erosion surface 850 feet above Snake River.
Glenns Ferry Formation	Fine-grained basin deposits at least 2000 feet thick yielding abundant vertebrates of Blancan provincial age. Intercalated basalt dated 3.48×10^6 years by K-Ar (Evernden *et al.* 1964:191, sample KA 1173).

On the other hand, to illustrate further the uncertainty of correlating nonglacial deposits with glaciers, it is easy to argue that the Bruneau is preglacial. From the date for Melon Gravel (see Table 1), older deposits along the Snake River Canyon are evidently pre-"classical" Wisconsin. Crowsnest Gravel and Sugar Bowl Gravel, on terraces 200 feet and 400 feet, respectively, above the river, might be correspondingly representatives of Illinoian and Kansan Glaciation. The Black Mesa Gravel, precanyon pediment deposit, might equal the Nebraskan, and the underlying Bruneau Formation therefore might be preglacial.

In the light of these conflicting arguments, speculative correlations based on a few K-Ar dates of uncertain affinity to glacial stratigraphy ought to be avoided. Clearly, many more K-Ar dates of established significance are required.

By J. R. Richards

Canberra, Australia. 9 xi 64

This paper is most welcome on 2 counts.

First, it is good to see a published description of the Berkeley argon extraction procedures. Since Evernden introduced them in Canberra 4 years ago, we have used them, and adopted many of the successive modifications, with gratifying success. The techniques at their present state of refinement seem now able to deal with the most important of the foreseeable sources of error. As far as can be judged from this paper, we appear to be in essential agreement on potassium analyses.

Second, and most importantly, this paper represents a land-mark, with the closure of the final gap in the radiometric age spectrum, between the few tens of thousands of years attainable from 14-carbon measurements and the few million which has hitherto been the potassium-argon lower limit. These authors now make the startling claim that they could date, with precision of $\pm$ 25%, 20 grams of potash feldspar as young as 2.5 *thousand* years. This number is so small that it begs to be checked, an operation only possible if one makes a guess at the information which should have been, but is not, supplied.

From KA 1197, Table 1, the air argon contamination may be estimated for each determination if we assume a reasonable value, say 10%, for the potassium content of the mineral. Then by a rash assumption of constant air contamination per gram of material, one may extrapolate graphically to a value for 20 grams of around 2.5×10^{-11} mole of adsorbed air ^{40}Ar. The corresponding amount of radiogenic ^{40}Ar for a 2,500-year-old feldspar would be 0.9×10^{-12} mole. Thus the air correction would be around 96%. This is high, and the calculation is crude. But reference to the results in Table 1 for the Epomeo

Tuff, Ischia, suggests that a claim for 25% precision may not be too unreasonable. This is most impressive, for a result which would have to be obtained on as little as 2×10^{-8} cubic centimetres of radiogenic argon at standard temperature and pressure.

The authors quite rightly take pains to discount the possibility of pre-eruption argon in the sample of volcanic anorthoclase which they have examined. As far as I am aware this phenomenon has never been observed in rocks such as those studied here, which have been emplaced at or near the earth's surface. Yet there is beginning to be found a number of examples of minerals which contain more argon than can be explained by the simple models, either of accumulation of radiogenic ^{40}Ar, or of diffusive loss against zero back-pressure. Examples may be found in Hart and Dodd 1962; Richards and Pidgeon 1963; McDougall and Green 1964; and Lovering and Richards 1964. It seems significant that all the cases quoted involve some depth of burial at the critical time (contrast with McDougall 1964:110). No examples of feldspar showing this phenomenon have yet been found. It is to be noted that the present authors omitted to list the anomalously high result for anorthoclase KA 652 in this section. This result must remain a puzzle since insufficient information is available for its assessment.

I do not wish to comment upon the geological aspects, save to observe that the correlation with Hay's paper on Olduvai Gorge appears to be good, and that this is the sort of information which should be available before samples are collected for dating.

I have 1 final plea, that these authors reform their chemical symbolism and refrain from further confounding the existing confusion about the proper position for the mass number index. Many physicists persist in writing Ar^{40}, despite the prior reservation of this position for ionic charge or valence state. The IUPAC rule 1.31 of 1957 (see Chemical Society, London, *Handbook for Authors*, p. 18) adopts the convention ^{40}Ar, which seems to me to be more logical. The usage by Evernden and Curtis, Ar^{40}, is to be deprecated.

By Donald E. Savage

Berkeley, Calif., U.S.A. 17 xi 64

Evernden and Curtis must be commended highly for ascertaining many geologic-radiometric data that bear upon problems of correlation within Pleistocene time. Their contribution also makes giant strides toward the more exact knowledge of the sequence and duration of stages in the morphologic and cultural evolution in the human family. Their proposed correlation between fossil-mammal Ages of North America (Blancan, Irvingtonian, etc.) and the Ages that are customarily recognized in Europe is subject to less confusion in meaning, especially to those of us who specialize in stratigraphic palaeontology and stratigraphy, than the previously employed and more uncertainly delimited Epoch subdivisions such as "late Pliocene" and "middle Pleistocene." This technique does not divorce us from the Epoch terminology, for the time-scale of Epochs and their subdivisions will remain the international language for communication of ideas about succession and correlation of the later episodes of earth and life history. But rather, this new procedure will help to put in its proper place the correlation of the time-scales based on phases of mammalian evolution, changes in plant distribution, or changes in human cultures with the generalized, Epoch scale as the final step in the synthesis of knowledge regarding geochronolgy.

Samples that are suitable for potassium-argon radiometry are usually selected because of a direct stratigraphic association with the data providing recognition of a Stage-Age (the European sequence); or a Mammal Age (North American Cenozoic nonmarine sequence); a palaeontologic correlation of another order is implied when the K-Ar sample is assigned a late Pliocene, or middle Pleistocene, or some other such position. This may be a trivial and unimportant distinction when the palaeontologic evidence is voluminous and strong. Nevertheless, the Evernden-Curtis procedure is a refinement in method that deserves re-emphasis because of its obscurity to those who are not well acquainted with the principles and methods of palaeontologic stratigraphy and chronology.

The tentative conclusion of Evernden and Curtis that the Late Villafranchian of Europe is probably correlative with Irvingtonian or Early Irvingtonian is an inference of secondary possibility in my opinion. The purely palaeontological data indicate that the Irvingtonian ("early *Mammuthus*") fauna of North America is partly characterized by mammalian genera and species whose closest relatives are the early *post*-Villafranchian forms of the Old World. This is not a clean-cut distinction, and many imponderables remain, such as the time lapse in dispersal of mammals between continents, the difficulty of segmenting for terminology the gradual change that is morphologic evolution, etc. Thus, a Late Villafranchian—Irvingtonian correlation cannot be proclaimed to be an impossibility; but the data from K-Ar datings have not yet overruled the usual palaeontologic correlation of Villafrancian with Blancan.

By H. E. Wright, Jr.☆

Minneapolis, Minn., U.S.A. 21 xii 64

Evernden and Curtis' paper is a mixture of (1) highly technical description of analytical methods, (2) defense of the accuracy of the K-Ar method, (3) presentation of dating results, and (4) speculation on the impact of this method on problems of Pleistocene chronology and correlation.

The description of the analytical problems is welcome, except that the presentation would be more understandable to the non-technical (yet intelligent!) reader without its jargon ("titanium gettering," "sample take-off") and its confusing expressions, which rival modern advertising copy in ambiquity ("angstrom holes," "argon extraction line schematic"). These problems tend to annoy the reader who is trying to follow the text with care. Such a reader (and especially the foreign reader), having traced (with these hazards) the steps of extraction and purification of argon, is then left in mid-air without a description of the means of actually measuring the argon, there being only oblique mention of the mass spectrometer.

Despite the difficulties, the detailed presentation of analytical techniques engenders a degree of confidence in the care with which the authors have introduced refinements to bring the method within the range of late-Pleistocene problems. Their insistence that samples be adequately understood stratigraphically before they are collected is a refreshing view for geochemists.

The K-Ar technique clearly opens a way in certain areas for new correlations and for the possibility of relating geological, biogeographical, and archaeological events in widely scattered places, events that otherwise must be dated or correlated by climatic theory or by finger-counting on an incomplete record. Thus it may supply the stimulus for more critical examination of early and middle Pleistocene problems in the same way that radiocarbon analysis has done for the late Pleistocene. Its application is limited to igneous rocks, however, so the possibilities are not good for dating glacial events in mid-continent America or in northern Europe, or for dating the sequence of Palaeolithic cultural stages or hominid evolution in Europe. So it will be some time before the relation will be established between the official beginning of the Pleistocene (Villafranchian or its North American palaeontological equivalent) and the glacial sequence in the Alps, northern Europe, and North America. The authors list 11 dates "related to glacial events" (Table 8), but, in all cases except the Rhine Terrace sample, which is controversial, the relation involved seems to rely on climatic correlation or is concerned with local mountain glaciations in isolated areas (Kilimanjaro, California) rather than with major areas. Attempts to date the Nebraskan (or Kansan or Illinoian) drift by correlation with the Bishop Tuff of the Sierra Nevada are unsatisfying, as is the long-distance correlation of the mammoth-bearing beds in Idaho on the basis of a single date, which, incidentally, has no obvious relation to glacial events at all. But if nothing else, these 1st results should stimulate a re-examination of world-wide palaeontologic correlations and the use of index fossils. It is unfortunate that the widespread Pearlette ash, which serves as an undisputed stratigraphic marker in the glacial and periglacial sequence of the American Great Plains, has not been dated at all.

The list of over 100 K-Ar dates is impressive, primarily because it demonstrates that results can be successfully duplicated on material that is properly placed stratigraphically, adequately evaluated geologically, free from contamination, and analyzed with precision. The great number of dates from the Olduvai area may place that sequence on firm chronological ground, and these dates have great significance for anthropologists concerned with the absolute time available for hominid evolution. But the Olduvai region is not critical for climatic chronology, whether the dates be in the range of the European glacial sequence or not.

The Italian dates also concern primarily a local problem, that of volcanic history, only indirectly related to the Italian glacial sequence. This is not to deprecate their importance for European Palaeolithic prehistory, but they contribute little to the problems of climatic chronology. If the East African and Italian groups of dates be excepted, the rest are so scattered and so unrelated to many of the central areas and problems of Pleistocene chronology that the concluding statement of the paper hardly seems justified. I have serious doubts that "5 years from now, few questions about the general time-scales of Pleistocene glaciations and of human cultural evolution will remain to be answered."

Replies

By J. F. EVERNDEN☆ and
G. H. CURTIS☆

We do not pretend to be either palaeontologists or anthropologists and have entered these fields only to make some temporal interpretations using K-Ar data where they appear to us superior to previously held opinions of a subjective nature. We confess that we do tend to believe our dates after we have eliminated what we consider to be all probable sources of error. Thus we do not take seriously the opinions of people (mentioned by Bishop) who still do not believe that meaningful K-Ar dates can be obtained for ages less than 30 × 10^6 years. While we were not able to quote any dates obtained by a different decay scheme for material from Olduvai Gorge at the time we submitted our paper, this is no longer the case. A paper in press by Fleischer, Price, Walker, and Leakey reports the results of dating pumice from Bed I by the new fission track technique. They obtained an age in the range 1.7 to 2.1 × 10^6 years ± 25%. To quote from their paper:

> At this place in the discussion it should be noted that the possible sources of wrong ages in the K-Ar and fission track methods are different. For example, the presence of inherited argon in the anorthoclase, which would lead to too *high* an age, would not affect a fission track age. On the other hand fission track ages are usually more sensitive to heating, which would lead to too *low* an apparent age. The fact that the two ages agree within the precision of the experimental procedures is therefore strong support for the validity of an age of nearly two million years.

With reference to Bishop's comment on the exclusion of all but 19 dates from Table 6, we stated earlier (Curtis and Evernden 1962) that the basalts at the base of Bed I were unreliable for K-Ar dates owing to extensive alteration and that we ran them, in response to the note of von Koenigswald, Gentner, and Lippolt (1961), "only in order to compare the numbers [not the ages]." We worked on the problem of these altered Olduvai rocks long before Hay went to Africa, and obtained, shortly after his return from his 1st trip there, an age of 1.92 × 10^6 years, which he asserted agreed with his interpretation of the geology.

We have stated elsewhere our reason for not believing the older dates obtained from samples of tuff on Rusinga: the high probability of contamination from basement rocks. The date of 19 × 10^6 years obtained by Damon (cited by Bishop) "for an identical Miocene assemblage at Napak" would place the fauna as equivalent to Hemingfordian of the North American mammalian ages, a position which seems to us suitable. On the European age scale it would be late Burdigalian at the oldest and possibly only Vindobonian.

As to the dates obtained in the Gregory Rift, where these differ from the geologic interpretation we feel that the interpretation is more apt to be wrong than the dates. We do not recall collecting any samples where a rock determined to be older by K-Ar dating actually lay directly on another for which we obtained a younger date. We discounted the date of 486,000 years for Ol Orgesailie not, as Bishop suggests, for anthropological reasons but because a rerun gave 2.9 × 10^6 years. Such a discrepancy indicates contamination, and, since the runs were splits of the same sample, it is unlikely that 1 of the runs received all of the contaminating material.

A point of interest concerns the dating of Bed II: Hay, in correlating the top of Bed II in his various sections, finds that our sample (KA 405, KA 405A) is very close to the top of Bed II (personal communication).

We do not believe that we wittingly or unwittingly attempted, as Damon's comment suggests, to gloss over the problems associated with dating such young volcanic rocks. We suspect that we have thought and experimented more on evaluating the role of excess argon in feldspars than any other laboratory. In our opinion, Damon quotes irrelevant data to support his thesis of doubt. As noted by Richards, the evidence shows that argon excess in pyroxenes is related to depth of burial or genesis. Just as certainly as excess argon has been found in rocks once deeply buried, it has been shown *not* to occur in the surface or near-surface volcanic rocks analyzed so far. It has been shown *not* to occur in pyroxenes from granitic rocks of the Sierra Nevada batholith (R. W. Kistler, personal communication). It has been shown *not* to occur in quartz from granitic rocks of the Sierra Nevada, plutons ranging in age from 80 to 105 × 10^6 years, but to occur in the quartz of some Pre-Cambrian gneisses (this laboratory, unpublished data). Thus, mineral type, site of emplacement and/or cooling history, and argon pressure in the rocks are important factors; and it is irrelevant to quote data for 1 situation as evidence to evaluate a markedly different situation. We probably have more relevant data on this problem than are available for deeply buried rocks. For instance, we have dated several sanidine samples that have yielded essentially "zero ages" (plus or minus about 3—4,000 years); we have obtained concordant plagioclase (K = 0.75%) and biotite (K = 7.01%) ages at 13.0 × 10^6 years; we have obtained concordant plagioclase (K = 0.86%) and sanidine (K = 8.225%) ages at 23 × 10^6 years; we have obtained concordant plagioclase (K = 2.51%) and anorthoclase (K = 3.96%) ages at approximately 1.90 ×

10^6 years which are in essential agreement with a plagioclase-pyroxene mixture from a basalt flow a few feet below the tuff containing the plagioclase and anorthoclase; we have dated 5 plagioclases of known stratigraphic position in the age range 3.5 to 34.0 $\times 10^6$ years, and these dates are in excellent agreement with mammalian vertebrate fossil assemblages associated with each; and we have obtained significant data on the characteristics of argon diffusion in feldspars. We believe that the helium content of magnetite is irrelevant to the problem before us, as are many of the other data quoted by Damon and well known to us. We have stated only that evidence in hand indicates very low to vanishing levels of pre-eruption argon in the minerals of volcanic rocks: as far as we are concerned, we already have many of the data that Damon asks us to collect.

Damon's remark about hindsight versus foresight in our selection of samples is unjustified. We stated in the text that numerous samples were run for no other reason than to evaluate factors of contamination, alteration, etc. We knew of the presence of contamination *beforehand*, and if we had only been interested in obtaining the best dates, most of the samples would never have been analyzed. The only possible way to evaluate disturbing factors is to run controlled samples. Damon knows this, asks us to do it, then castigates us! His Table 1 may be interesting but is again irrelevant and unsatisfactory: all estimates of sedimentation rates in the area during accumulation of Beds I and II strongly deny the possibility that 0.5 $\times 10^6$ years was the period required for the accumulation of the very thin section emcompassed by those 4 tuffs.

He also questions the inclusion of KA 1050, 1046, and 1047 in Table 6 of the text. The possibility of contamination in KA 1050 was suggested by coarse cross-bedding of pumice fragments (i.e., some transport and redeposition), not by rounding of pumice fragments nor by presence of detrital fragments of 1 sort or another. This is a marginal criterion and, if rigidly employed, would cause rejection of KA 1050. Again, KA 1046 is a marginal case in that the presence of lapilli may or may not invalidate the age, depending upon the time of cooling of the lapilli. KA 1046 is indicated as doubtful in the Appendix. We chose to include both of these ages in Table 6 while indicating the existence of a degree of doubt in the Appendix. As regards KA 1047, we find no reference in the text to contamination in this sample.

With respect to Hay's question about the dates of KA 1180 and KA 1047, we are inclined to agree with his interpretation that KA 1180 is 100,000 years too old. It should be noted, however, that the *nuée ardente* deposit is completely separated from the ash which he considers to be its equivalent. He correlates the 2 on the basis of similar mineralogy and presence of pumice fragments in each. These are not always valid criteria for correlating volcanic deposits, particularly if they have originated from 1 magma chamber. It appears from evidence elsewhere that tuffs erupted over a period of more than 1,000,000 years from 1 volcanic center may have almost identical chemical and mineralogic properties.

Hopkins has made some important observations, particularly concerning the validity of the American or Alpine sequences as a complete record of every significant Pleistocene glaciation or interglaciation. No doubt they are not. Iceland, owing to its history of volcanism, will almost certainly have a more complete preservation of glacial events, although it may prove almost equally difficult to decipher. Just as Antarctica is not the best place to establish the termination of the Würm glaciation in Europe or North America, the record of Iceland may be misleading: today, in the midst of an interglacial period throughout most of the world, 1/8 of Iceland is covered by ice. Do we know the size of that ice cap 7,000 or 10,000 years ago? In the end, too, we still have the problem of correlation.

To say, as does Hopkins, that "attempts to correlate earlier components of the 'standard' glacial sequences of north-central United States and the Alps and to relate early and middle Pleistocene events in other areas to 1 of these sequences are so hazardous and speculative that they are almost without value at the present time" is possibly true, but Hopkins supports this counsel of despair by citing 2 extreme examples which already have been refuted, namely, Putnam's correlation of the Bishop Tuff with Illinoian and Emiliani's correlation of the Bishop Tuff with a glaciation earlier than the Donau. At the present moment there is no foreseeable way of getting radiometric dates of any kind directly from the middle or early Pleistocene glaciations of the classic European or North American sections; hence, as radiometric dates of events thought to be correlative with parts of these sections are obtained elsewhere, it is perfectly reasonable to attempt correlations of these events with the classic sections and the classic sections with each other. The program of dating in the "Late Pliocene—Pleistocene" interval (of interest to Kretzoi) is progressing as rapidly as samples become available. Curtis and D. E. Savage, both at the University of California at Berkeley, are most eager to correspond with any palaeontologists knowing of datable sites of this age.

Hopkins' statement that "only when radiometric age determinations are available for deposits closely bracketing or unambiguously assignable to the Donau ... will these 2 sequences become useful standards of reference for glacial and interglacial events in other areas," is in 1 sense true, and follows the reasoning we used concerning the assignment of Cenozoic geologic events outside Europe to Tertiary epochs (Evernden, Savage, Curtis, and James, 1964); however, just as the Tertiary epochs with their poorly defined boundaries have served as a useful standard of reference, so the classic glacial sequences will continue to serve as useful standards until they can be replaced by a new standard, perhaps the Iceland sequence, which must 1st be proven to be better.

We are not quite as impressed as is Hopkins with the evidence correlating in total the Wisconsin and Würm glaciations. To call this a precise correlation appears to us to be unwarranted. We believe that published C-14 dates greater than 40,000 years have a high probability of being off by as much as 100% or more on the young side. Since submitting our manuscript, however, we have become impressed with the evidence that the Riss-Würm interglacial ended approximately 100,000 years ago.

Attention should certainly be paid to Hopkins' remarks concerning contaminating xenocrysts in tuffs. We were aware of contaminating granitic fragments and crystals in the Bishop Tuff but, owing to the welded nature of the tuff, thought that the temperature had been high enough to eliminate inherited argon. In this assumption we were wrong, as the work by Dalrymple has convincingly demonstrated.

Malde has made some good points, but his last illustration (penultimate paragraph) would only be admissible if no fauna were known from the Bruneau; with a fauna of "mid-Pleistocene" age present, this type of argument is invalid and should be left out of any meaningful discussion. We simply obtained a K-Ar date for the Bruneau formation, which is much the same as finding a fossil. If that date be accurate (and we think it is reasonably close), then we are justified in weighing Malde's other possibilities, particularly since the Bruneau's position in the North American glacial sequence is not known precisely.

The presence of *Mammuthus* told Malde that the Bruneau is not Pliocene: the K-Ar date told us that the Bruneau is not mid-Wisconsin! Wright recommends the dating of the Pearlette ash. We have attempted to date samples of this ash from several localities and have obtained ages ranging from approximately 2,000,000 to 12,000,000 years. Either the samples were contaminated (1 certainly was) or they were not samples of the same ashfall. We are still working on this problem and hope to have an accurate date or dates soon.

While we accept Richards' criticism of our errant nomenclature and shall mend our ways, we cannot accept Wright's criticism of our "jargon." This paper is not intended to be a review, but rather a presentation of material virtually all previously unpublished. The mass spectrometer used has been described before so is not mentioned here. Our argon extraction technique is somewhat different from other workers' in this field; therefore we have given details concerning it. Admittedly, these details will have little meaning for some readers, but the paper is intended for a wide audience, and those workers immediately concerned will understand our "jargon." Each research field has its vocabulary and mode of expression which can be confusing to the uninitiated: certainly, anthropology is not an exception to this rule! The examples Wright cites may be clarified as follows: "Gettering" refers to a property of heated metals for combining selectively with and/or adsorbing various gases; titanium is an excellent getter, particularly of nitrogen. The special glass tube in which the clean gas sample is collected is called a "sample take-off" because the tube with gas sample is immediately taken off the extraction line with a "blow pipe" or "blow torch" after collection; the distinguishing features of a sample take-off are its "charcoal finger" for adsorbing gas when the finger is immersed in liquid nitrogen and its "break-off" seal for later release of the gas at a desired time.

We appreciate Clark's comments but do not understand his remarks, and those of others, about Dighton's Cliff Extension; we thought we made it clear in the text that we were unable to obtain satisfactory dates for that site as well as for Ol Orgesaille and Kariandusi.

With regard to the dates for Pseudo-Stillbay and Stillbay sites (questioned by Leakey as well as Clark), we have had long discussions with many people about these dates and are aware of the negative feelings of anthropologists concerning them. Owing to uncertainties in the field relationships and to the fact, mentioned by Clark, that neither the Kenya Stillbay nor the Pseudo-Stillbay artifacts have been "precisely described nor comprehensively illustrated," we have chosen deliberately to argue the case on the basis of our interpretation of the geology together with the samples we have dated which appear to be good. If all of this discussion stimulates further clarification of the problem, our purpose will have been achieved.

Clark doubts the date of 1.1×10^6 years for KA 664; as can be seen from Hay's comments, this date should be forgotten. The sample, originally sent to us by Leakey, was supposedly collected in Bed I. Curtis visited the site but could not find a biotite tuff at the spot from which it was said to have been collected. He examined the section there carefully and found a bed containing a few biotite flakes high in Bed I, from which he surmised Leakey probably had obtained the sample; we published a note to that effect (Leakey, Evernden, and Curtis 1961). Hay's attributing this date to a contaminated sample of Bed V makes sense for the following reason: Blocks of Bed V rich in biotite are to be found at the site from top to bottom of the cliff. While Leakey himself pointed out to Curtis the danger of mistaking these blocks for a layer *in situ*, some blocks are so located as to appear almost certainly in place, and only the most painstaking efforts can reveal that they are simply part of the talus.

We feel that we should not get enmeshed here in the discussion, begun by Leakey, of the palaeontologic evidence of Rusinga. The purported conversation between Leakey and Evernden certainly did not occur but may have taken place between Leakey and Savage. Leakey's discussion would have been more germane if it had been related to the detailed analysis of the evidence by Savage and James (Evernden, Savage, Curtis, and James 1964: 171-72). Leakey also asserts that while collecting on Rusinga Island with Evernden, he discussed the possibility of an unconformity lying between the beds from which sample KA 336 came and the underlying Kiahera Series. This was not the case, and no mention of such an unconformity was made until after the dates were obtained. That such an unconformity may exist is, of course, possible, and should be the object of careful investigation.

Leakey's opinion that the Wetherell and Cartwright sites were occupied at a time antedating the main faulting of the eastern branch of the Great Rift Valley appeared reasonable to Curtis when he was there, but he saw no indications that the small canyon next to the Wetherell site was ever cut by a river: it is not a box canyon indicative of rapid downcutting by a large volume of water, but is narrow at the bottom, widening progressively upward without terraces or break in cross-sectional profile. While, indeed, the deposits at the Kenya Stillbay site at Malewa gorge may have been laid down during the last high water of Lake Naivasha, it is possible that they were deposited at an earlier time and subsequently inundated.

Our statement beginning "5 years from now few questions . . .," was based on what now appear to be unjustified hopes for numerous technically good samples. We agree with Leakey, however, that optimism is the intended tone of the paper: the K-Ar technique can produce answers if samples are provided.

In conclusion, we shall answer Brace's practical questions in the order given:

1. Any potassium-argon laboratory that is interested can do analyses such as these. Inquiries might be made at any of the following laboratories: United States Geological Survey, Menlo Park, California, and Denver, Colorado; National Bureau of Standards, Brookhaven, New York; Department of Geophysics and Geochemistry of the Australian National University, Canberra, Australia; the universities of Oxford, Cambridge, Heidelberg, Arizona, Minnesota, Columbia, British Columbia, Massachusetts Institute of Technology, Alberta, and Hawaii.
2. Concentration of minerals and analysis after the sample has reached our laboratory costs about $150-200. We do only samples that fit into our program of research, none commercially.
3. We have attempted to state the necessary controls on sample collection in this and previous papers. We agree that samples should be collected by trained geologists, and we insist that the entire sample must reach our laboratory for all phases of analysis other than field relationships.
4. Standard deviation estimates based upon statistical analysis of the data of each run would be approximately 2% or less on all runs with no figure indicated. Repeat runs support this estimate. Multiple runs of minerals and runs on concentrates from different sites in the same volcanic stratum suggest a higher age scatter.
5. We are interested in all datable samples which occur in close relationship to hominid fossils and invite correspondence concerning them.

By R. L. HAY☆

I will comment on the nature of the Pleistocene climate of the Olduvai region in the light of recent field work, which substantially modifies my earlier conclusions on the nature of Beds III and IV (see Fig. 1). I now subdivide the Olduvai Beds into 2 genetic sequences—a lower sequence (Sequence A) comprising Bed I and most of Bed II, and an upper sequence (Sequence

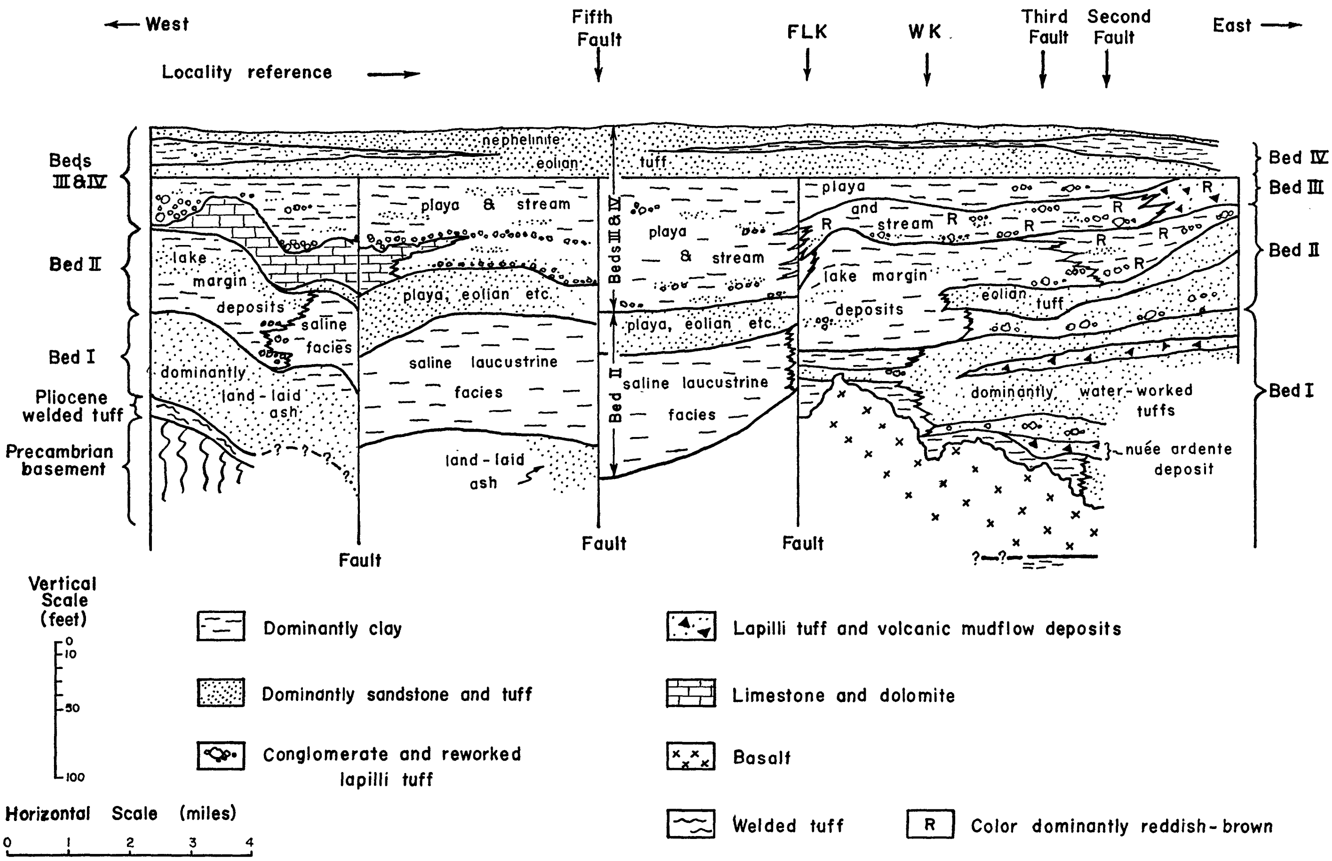

FIG. 1. Stratigraphy of Beds I through IV along the Olduvai Main Gorge as revised on the basis of field work in 1964. The sequence is reconstructed using the base of the eolian tuffs of Bed IV as a horizontal datum. The indicated fault displacements took place prior to the accumulation of the eolian tuffs of Bed IV. This interpretation is proved by the abrupt thickening of individual stratigraphic units across the faults.

B) comprising the upper part of Bed II and all of Beds III and IV.

Sequence A begins with interbedded lacustrine and fluvial deposits, lava flows, and land-laid tuffs. I have seen fresh-water molluscs, crocodile remains, and fish bones in this sequence. Perennial streams were present, but a large perennial lake was not yet established. The impermanent sheet of water which existed was probably fresh or brackish and fluctuated greatly in extent. The base of Bed II marks approximately the horizon at which a perennial saline lake became established, and this lake existed for most of the lengthy duration of Bed II. The saline-lacustrine deposits are notable for their absence of fossils. I have not observed saline minerals in these deposits, but beds of dolomite, abundant coarse euhedral crystal of calcite, and the authigenic minerals K-feldspar, phillipsite, and searlesite suggest that this water was rich in dissolved sodium carbonate and frequently attained salinities of 5 to 10%. During periods of heavy rainfall the lake expanded 2 to 3 miles east of its usual margin, flooding the marginal terrain with fresh or brackish water.

Faulting in the Olduvai region was initiated during the deposition of Bed II (see Fig. 1), destroying the perennial lake and resulting in a broad playa (seasonally dry) lake, which existed for most of the duration of Beds III and IV. Perennial streams flowed across the playa deposits, and Dr. Leakey has pointed out to me in these stream-channel deposits pelecypods, fish bones, and a horizon of crocodile remains.

From these data I draw the following conclusions:

1. Geological data are inadequate for a relatively precise climatic interpretation of Bed I.

2. The lake of Bed II was almost certainly more saline for most of its history than lakes Naivasha, Nakuru, and Elementeita, with which Leakey compares it.

3. Termination of the lake of Bed II has no climatic significance, as it was tectonically controlled.

The mid-Pleistocene Peninj Beds, on the west side of Lake Natron and 50 miles northeast of Olduvai Gorge, are highly significant in the interpretation of Bed II and the climatic history of East Africa. These beds, mapped by Glynn Isaac, are dominantly lacustrine deposits which are correlative at least in part with the lacustrine deposits of Bed II. The upper half of the 250-foot sequence of Peninj Beds is lithologically and mineralogically similar to the saline-lacustrine facies of Bed II but also widely contains casts and molds of the saline minerals trona and gaylussite—both of which crystallize today from the brine of Lake Natron. Thus, the mid-Pleistocene lake in the vicinity of Lake Natron was for much of its history filled with brine similar to that of Lake Natron, suggesting a hot, semiarid climatic regime not greatly different from that of today. Annual rainfall in the vicinity of Lake Natron may, however, have been perhaps as much as 50% higher than the present amount of 15 inches extrapolated from rainfall records at Lake Magadi, several miles to the north. Runoff from the highlands to the west into Lake Natron may, however, have been more than 50% in excess of that at present.

The saline lake of Bed II lay no more than 40 miles, and possibly less, to the southwest of the mid-Pleistocene lake in which the Peninj Beds accumulated. I suspect that the climate of Bed II resembled that of the present to about the same extent that the mid-Pleistocene climate in the vicinity of Lake Natron resembled that of the present climate there. The climate of Bed II may have been slightly drier than that suggested by this comparison, however, as the volcanoes Ngorongoro, Oldeani, and Lemagrut were higher than at present and would then have formed a more effective rain shadow over the Olduvai region than they do at present. Because of greater precipitation on higher parts of the volcanoes, runoff to the Olduvai region may have been double or triple the amount which presently flows from these volcanoes into the Balbal depression.

In summary, I consider the Pleistocene climatic regime to have been much like that of the present—with possibly as much as 50% more rainfall than today during the time of Bed II. Surface runoff into the Olduvai region may, however, have been double or more the amount received in this region today. At times the climate may have been slightly drier than at present (see my 1963 paper, reprinted here). The wider climatic fluctuations visualized by Leakey are possible but are not demanded by the geologic evidence. Palynological data would of course be a more accurate means than the geology in reconstructing the climatic history of the Olduvai sequence.

References Cited

AZZARIOLI, A. 1953. The deer of the Weybourne Crag and Forest Bed of Norfolk. *Bulletin British Museum (Natural History), Geology* 2:1–96. [FCH☆]

BAKER, B. H. 1958. Geology of the Magadi area. *Geological Survey of Kenya,* No. 42.

BINGE, F. W. 1962. Geology of the Kericho area. *Geological Survey of Kenya,* No. 50.

BISHOP, W. W. 1959. Kafu stratigraphy and Kafuan artifacts. *South African Journal of Science,* 55:117–21. [JDC☆]

——. 1964. More fossil primates and other Miocene mammals from north-east Uganda. *Nature* 203:1327–31. [WB☆]

BLANC, A. C. 1957. On the Pleistocene sequence of Rome: Paleoecologic and archeologic correlations. *Quaternaria* 4: 95–109. [FCH☆]

BLANCHARD, R. L. 1963. Uranium decay series disequilibrium in age determination of marine calcium carbonates. Unpublished Ph.D. dissertation, Washington University, St. Louis, Mo. [DMH☆]

BOULE, M., and H. V. VALLOIS. 1957. *Fossil men.* New York, Dryden Press.

BOURDIER, F. 1961. *Le bassin du Rhône au Quaternaire. Géologie et Préhistoire.* Tomes I, II. Paris: Éditions du Centre national de la recherche scientifique. [FCH☆]

BOUT, P. 1960. *Le Villafranchien du Velay et du bassin hydrographique moyen et supérieur de l'Allier.* Le Puy (Haute-Loire): Imprimerie Jeanne d'Arc. [FCH☆]

——. 1963. Le Quaternaire du bassin supérieur de la Loire, des bassins moyen et supérieur de l'Allier et de leurs marges. *Bulletin de la Société Géologique de France,* sér. 7, 5:472–82. [FCH☆]

BRELIE, G. VAN DER. 1959. Probleme der stratigraphischen Gliederung des Pliozäns und Pleistozäns am Mittel- und Niederrhein. *Fortschritte in der Geologie von Rheinland und Westfalen,* 4 (Pliozän und Pleistozän am Mittel- und Niederrhein): 371–88. [FCH☆]

CHEMICAL SOCIETY, LONDON. 1960. *Handbook for authors.* Special Publication No. 14. [JRR☆]

CHERDYNTSEV, V. V., and L. V. KOZAK. 1949. Report. *Academia Scientiarium U.S.S.R., Reports* 69:829. Results discussed in *Abundance of chemical elements,* by V. V. Cherdyntsev, pp. 86–87. Translated by Walter Nichiporuk. Chicago: University of Chicago Press. [PED☆]

CLARK, J. D. 1959. *The prehistory of southern Africa.* Harmondsworth, England: Penguin Books Ltd.

——. 1960. Human ecology during Pleistocene and later times in Africa south of the Sahara. *Current Anthropology* 1: 307–24.

CLARK, W. E. LE GROS, and L. S. B. LEAKEY. 1951. *The Miocene Hominoidea of East Africa,* pp. 1–117. British Museum (Natural History), Fossil Mammals of Africa, No. 1.

COLE, S. 1954. *The prehistory of East Africa.* Harmondsworth, England. Penguin Books.

——. 1964. *The prehistory of East Africa.* New York: Macmillan. [JDC☆]

CONDRA, G. E., E. C. REED, and E. D. GORDON. 1947. Correlation of the Pleistocene deposits of Nebraska. *Nebraska Geological Survey,* No. 15.

COOKE, H. B. S. 1958. Observations relating to Quaternary environments in East and Southern Africa. *Geological Society of South Africa, Transactions (Annexure)* 60:1–73. [FCH☆]

COX, A., R. R. DOELL, and G. B. DALRYMPLE. 1963. Geomagnetic polarity epochs and Pleistocene geochronometry. *Nature,* 198:1049–51. [FCH☆]

——. 1964. Reversals of the earth's magnetic field. *Science,* 144:1537–43. [DMH, FCH☆]

CURTIS, G. H., and J. F. EVERNDEN. 1962. Age of basalt underlying Bed I, Olduvai. *Nature* 194:610–12. [FCH☆]

DALRYMPLE, G. B. 1963. Potassium-argon dates of some Cenozoic volcanic rocks of the Sierra Nevada, California. *Bulletin Geological Society of America* 74:379–90.

——. 1964*a*. Argon retention in a granitic xenolith from a Pleistocene basalt, Sierra Nevada, California. *Nature* 201:282. [DMH☆]

——. 1964*b*. Potassium-argon dates of three Pleistocene interglacial basalt flows from the Sierra Nevada, California. *Bulletin Geological Society of America* 75:753–58. [DMH☆]

DAMON, P. E. and J. L. KULP. 1958. Excess helium and argon in beryl and other minerals. *American Mineralogist* 43:433–59. [PED☆]

DEPÉRET, C. 1885. Considérations générales sur les vertébrés pliocénes de l'Europe. *Annales Société Géologique de France* 17: 231–72. [FCH☆]

——. 1893. Notes sur la succession stratigraphique des faunes mammifères pliocènes d'Europe et du plateau central en particulier. *Bulletin de la Société Géologique de France,* sér. 3, 2:524–40. [FCH☆]

——. 1909. L'évolution des mammifères tertiares, importance des migrations; époque pliocène. *Comptes Rendus des Séances de l'Académie des Sciences, Paris* 148:140–43. [FCH☆]

DIETRICH, W. O. 1953. Neue Funde des etruskischen Nashorns in Deutschland und die Frage der Villafranchium-Faunen. *Geologie* 2:417–30. [FCH☆]

EMILIANI, C. 1955. *Pleistocene temperatures. Journal of Geology* 63:538–78. [FCH☆]

——. 1956. Note on absolute chronology of human evolution. *Science* 123:924–26. [FCH, AK☆]

——. 1958. Palaeotemperature analysis of core 280 and Pleistocene correlations. *Journal of Geology* 66:264–75. [FCH☆]

——. 1961. Cenozoic climatic changes as indicated by the stratigraphy and chronology of deep-sea cores of *Globigerina*-ooze facies. *Annals of the New York Academy of Sciences* 95(1):521–36. [FCH☆]

——. 1964. Palaeotemperature analysis of the Caribbean cores A254-BR-C and CP-28. *Bulletin Geological Society of America* 75:129-44. [DMH☆]

ERICSON, D. B., M. EWING, and G. WOLLIN. 1963. Pliocene-Pleistocene boundary in deep-sea sediments. *Science* 139:727–37.

——. 1964. The Pleistocene epoch in deep-sea sediments. *Science* 146:723–32. [DMH, FCH☆]

ERICSON, D. B., M. EWING, G. WOLLIN, and B. C. HEEZEN. 1961. Atlantic deep-sea sediment cores. *Bulletin Geological Society of America* 72:193–286. [FCH☆]

ERICSON, D. B., and G. WOLLIN. 1956. Micropalaeontology and isotopic determinations of Pleistocene climates. *Micropalaeontology* 2:257–70. [FCH☆]

——. 1964. *The deep and the past.* New York: Alfred A. Knopf. [FCH☆]

EVERNDEN, J. F., G. H. CURTIS, and R. KISTLER. 1957. Potassium-argon dating of Pleistocene volcanics. *Quaternaria* 4:1–5.

EVERNDEN, J. F., G. H. CURTIS, R. W. KISTLER, and J. OBRADOVICH. 1960. Argon diffusion in glauconite, microline, sanidine, leucite, and phlogopite. *American Journal of Science* 258:583–604.

EVERNDEN, J. F., and J. R. RICHARDS. 1962. Potassium-argon dates in eastern Australia. *Journal of the Geological Society of Australia* 9:1–50.

EVERNDEN, J. F., D. E. SAVAGE, G. H. CURTIS, and G. T. JAMES. 1964. Potassium-argon dates and the Cenozoic mammalian chronology of North America. *American Journal of Science* 262:145–98. [DMH, FCH, HEM☆]

FEJFAR, O. 1964. The Lower Villafranchian Vertebrates from Hajnácka near Filakovo in Southern Slovakia. *Rozpravy Ústřed-Úného Ústavu Geologickěho* 30:1–112. [MK☆]

FERGUSSON, G. J. and W. F. LIBBY. 1963. "U.C.L.A. Radiocarbon dates II," in *American Journal of Science, Radiocarbon Supplement* 5. Edited by E. S. Deevey and R. F. Flint, pp. 1–22. [JDC☆]

——. 1964. "U.C.L.A. Radiocarbon dates III," in *American Journal of Science Radiocarbon Supplement* 6. Edited by E. S. Deevey and R. F. Flint, pp. 318–339. [JDC☆]

FLINT, R. F. 1959. Pleistocene climates in eastern and southern Africa. *Bulletin Geological Society of America* 70:343–74.

FLINT, R. F., and F. BRANDTNER. 1961. Climatic changes since the last interglacial. *American Journal of Science* 259:321–28.

FLINT, R. F., and E. S. DEEVEY. Editors. 1960. *American Journal of Science Radiocarbon Supplement* 2. [JDC☆]

——. n.d. "U.C.L.A. radiocarbon dates IV," in *American Journal of Science Radiocarbon Supplement* 7. Edited by R. F. Flint and E. S. Deevey. In press. [JDC☆]

FÖLDVÁRI-VOGL, M., and M. KRETZOI. 1961. Kritische Untersuchungen über die Anwendbarkeit des Fluorverfahrens. *Acta Geologica* 7:13–28. [MK☆]

FRECHEN, J. 1959. Die Tuffedes Laacher Vulkangebietes als quartär-geologische Leitgesteine und Zeitmarken. *Fortschritte in der Geologie von Rheinland und Westfalen,* 4 (Pliozän und Pleistozän am Mittel- und Niederrhein): 363–70. [FCH☆]

FUNNELL, B. M. 1961. The Palaeogene and Early Pleistocene of Norfolk. *Transactions, Norfolk and Norwich Naturalists' Society* 19:340–64. [FCH☆]

FUNNELL, B. M., and R. G. WEST. 1962. The early Pleistocene of Easton Bavents, Suffolk. *Quarterly Journal Geological Society of London* 118:125–41. [FCH☆]

GERLING, E. K., YU. A. SHUKOLYUKOV, T. V. KOLTSOVA, I. I. MATVEYEVAN, and S. S. YAKOVLEVA. 1962. Dating of mafic rocks by the K-Ar method. *Geokhimiya* 11: 1055–62. [PED☆]

GROMMÉ, C. S., and R. L. HAY. 1963. Magnetization of Bed I, Olduvai Gorge, Tanganyika. *Nature* 200:560–61. [DMH, FCH☆]

HART, S. R., and R. T. DODD, JR. 1962. Excess radiogenic argon in pyroxenes. *Journal of Geophysical Research* 67: 2998–99. [PED, JRR☆]

HAY, R. L. 1963. Stratigraphy of Beds I through IV, Olduvai Gorge, Tanganyika. *Science* 139:829–33, also CA 6:386–89.

HIBBARD, C. W. 1953. Equus (Asinus) Calobatus Troxell and associated vertebrates from Pleistocene of Kansas. *Transactions Kansas Academy of Sciences* 56: No. 1.

HOPKINS, D. M. 1964. Chetvertichnye morskie transgressi Alyaski. [Late Cenozoic marine transgressions in Alaska]. *U.S.S.R. Institute for the Geology of the Arctic Irudy* 143. In press. (English translation will be available from Office of Technical Services, U.S. Department of Commerce; and American Geological Institute). [DMH☆]

HOWELL, F. CLARK. 1962. Potassium-argon dating at Olduvai Gorge. *Current Anthropology* 3:306–8. [FCH☆]

KING, W. B. R. 1950. The Pliocene-Pleistocene boundary: Introduction. *18th Session, International Geological Congress, London, Report,* pt. 9, pp. 5–6 [AK☆]

KLOOSTERMAN, J. B. 1960. Le volcanisme de la region d'Agde. *Geologica Ultraiestina No. 6, Mineralogisch-Geologisch Instituut der Rijksuniversiteit te Utrecht.*

KOENIGSWALD, G. H. R. VON. 1960. Tektite studies. I and II. *Proceedings Koninklijke Nederlandsche Akademie van Wetenschappen, Amsterdam,* ser. B, 63:135–53. [FCH☆]

——. 1962. "Das absolute Alter des *Pithecanthropus erectus* Dubois," in *Evolution und Hominisation.* Edited by G. Kurth, pp. 112–19. Stuttgart: G. Fischer Verlag. [CLB, FCH☆]

KOENIGSWALD, G. H. R. VON, W. GENTNER, and H. J. LIPPOLT. 1961. Age of the basalt flow at Olduvai, East Africa. *Nature* 192:720–21. [DMH☆]

KRETZOI, M. 1956. Die altpleistozänen Wirbeltierfaunen des Villányer Gebirges. *Geologica Hungarica, Series Palaeontologica* 27:1–264. [FCH, MK☆]

——. 1959. Insectivoren, Nagetiere und Lagomorphen der jüngstpliozänen Fauna von Csarnóta im Villányer Gebirge (Südungarn). *Vertebrata Hungarica* 1: 237–44. [MK☆]

——. 1961*a*. Die Wirbeltierfauna von Diósd und die Frage der Miozän-Pliozän-Grenze. *Földtani Közlöny* 91:208–16. [MK☆]

——. 1961*b*. "Stratigraphie und Chronologie," in A. Rónai, M. Pécsi, and M. Kretzoi. Stand der ungarischen Quartärforschung. *Prace Instytut Geologiczny* 34:313–30. [MK☆]

——. 1962. Fauna und Faunenhorizont von Csarnóta. *Annual Report of the Hungarian Geological Institute* 1959:297–385. [MK☆]

KURTÉN, B. 1957. Mammal migrations, Cenozoic stratigraphy, and the age of Peking Man and the australopithecines. *Journal of Paleontology* 31:215–227. [FCH☆]

——. 1960*a*. The age of the Australopithecinae. *Stockholm Contributions in Geology* 6:9–22. [FCH☆]

——. 1960*b*. Chronology and faunal evolution of the earlier European glaciations. *Commentationes Biologicae, Societas Scientiarum Fennica* 21(5):1–62. [FCH☆]

——. 1960*c*. Faunal turnover dates for the Pleistocene and late Pliocene. *Commentationes Biologicae, Societas Scientarum Fennica* 22(5):1–14. [MK☆]

——. 1962. "The relative ages of the australopithecines of Transvaal and the pithecanthropines of Java," in *Evolution und Hominisation.* Edited by G. Kurth, pp. 74–80. Stuttgart: G. Fischer Verlag. [FCH☆]

——. 1963. Villafranchian faunal evolution. *Commentationes Biologicae, Societas Scientiarum Fennica* 26(3):1–18. [FCH☆]

LEAKEY, L. S. B. 1936. *Stone Age Africa.* London: Oxford University Press. [JDC☆]

——. 1960. *Adam's ancestors.* New York: Harper & Brothers.

LEAKEY, L. S. B., J. F. EVERNDEN, and G. H. CURTIS. 1961. Age of Bed I, Olduvai Gorge, Tanganyika. *Nature* 191: 478. [FCH☆]

LIPPOLT, H. J., and W. GENTNER. 1963. "K-Ar dating of some limestones and fluorites (Examples of K-Ar ages with low Ar-concentration)," in *Radioactive Dating, pp.* 239–44. Vienna: International Atomic Energy Agency. [PED☆]

LIPSON, J. 1958. Potassium-argon dating of sedimentary rocks. *Bulletin Geological Society of America* 69:137–49.

LONA, F. 1950. Contributi alla storia della vegetazione e del clima nella Val Padana—analisi pollinica del giacimento Villafranchiano di Leffe (Bergamo). *Atti della Società Italiana di Scienz. Naturali* 89: 123–78. [FCH☆]

——. 1963. Floristic and glaciologic sequence (from Donau to Mindel) in a complete diagram of the Leffe deposit. *Berichte des Geobotanischen Institutes Rübel in Zürich* 34:64–66. [FCH☆]

LONA, F., and M. FOLLIERI. 1957. Successione pollinica della serie superiore (Günz-Mindel) di Leffe (Bergamo). *Veröffentlichungen des Geobotanischen Institutes Rübel in Zürich* 34:86–98. [FCH☆]

LOVERING, J. F., and J. R. RICHARDS. 1964. Potassium-argon age study of possible crustal and upper mantle inclusions in deep-seated intrusions. *Journal of Geophysical Research.* In press. [JRR☆]

MCBURNEY, C. B. M. 1960. *The Stone Age of northern Africa.* Harmondsworth, England: Penguin Books

MCCULLOCH, D. S., D. W. TAYLOR, and M. RUBIN. 1965. Stratigraphy, nonmarine mollusks, and radiometric dates from Quaternary deposits in the Kotzebue Sound area, western Alaska. *Journal of Geology* 73. In press. [DMH☆]

MCDOUGALL, IAN. 1964. Potassium-argon ages from lavas of the Hawaiian Islands. *Bulletin Geological Society of America* 75:107–28. [JRR☆]

MCDOUGALL, IAN, and D. H. GREEN. 1964. Excess radiogenic argon in pyroxenes and isotopic ages on minerals from Norwegian eclogites. *Norsk Geologisk Tidsskrift* 44(2):183–96. [PED, JRR☆]

MALDE, H. E., and H. A. POWERS. 1962. Upper Cenozoic stratigraphy of western Snake River Plain, Idaho. *Bulletin Geological Society of America* 73:1197–220. [HEM☆]

MEIRING, J. D. 1956. *The macrolithic culture of Florisbad.* Navorsinge die Nasionale Museum Bloemfontein 1(9). [JDC☆]

MOVIUS, H. L., JR. 1949. Villafranchian stratigraphy in southern and southwestern Europe. *Journal of Geology* 57:380–413. [DMH, MK☆]

NILSSON, E. 1932. Quaternary glaciations and pluvial lakes in British East Africa. *Geografiska Annaler* 13:249–349. [FCH☆]

——. 1940. Ancient changes of climate in British East Africa and Abyssinia. A study of ancient lakes and glaciers. *Geografiska Annaler* 22:1–79. [FCH☆]

——. 1963. Pluvial lakes and glaciers in East Africa. *Stockholm Contributions in Geology* 11(2):21–57. [FCH☆]

OAKLEY, K. P. 1949. The Pliocene-Pleistocene boundary. *Geological Magazine* 86:18–21. [AK☆]

——, Secretary. 1950. Recommendations of Commission appointed to advise on the definition of the Pliocene-Pleistocene boundary. *18th Session, International Geological Congress, London, Report,* pt. 9, p. 6. [MK☆]

OHLSON, E. A. and W. BROECKER. 1961. *Lamont natural carbon measurements 6.* American Journal of Science Radiocarbon Supplement 3. [JDC☆]

PUTNAM, W. C. 1960. Faulting and Pleistocene glaciation in the east-central Sierra Nevada of California, U.S.A. 21st Session, International Geological Congress, Norden, Report, pp. 270–74. [DMH☆]

RICHARDS, J. R. and R. T. PIDGEON. 1963. Some age measurements on micas from Broken Hill, Australia. *Journal of the Geological Society of Australia* 10:243–60. [JRR☆]

RIEDEL, W. R., M. N. BRAMLETTE, and F. L. PARKER. 1963. "Pliocene-Pleistocene" boundary in deep-sea sediments. *Science* 140:1238–40. [FCH☆]

ROBINS, P. A. and E. R. SWART. 1964. "Southern Rhodesian radiocarbon measurements I," in *American Journal of Science, Radiocarbon Supplement* 6. Edited by E. S. Deevey and R. F. Flint, pp. 31–36. [JDC☆]

ROCHE, A. 1963. Élaboration d'une stratigraphie paléomagnetique des formations volcaniques. *Bulletin de la Société Géologique de France,* sér. 7, 5:182–87. [FCH☆]

ROSHOLT, J. N., C. EMILLIANI, J. GEISS, F. F. KOCZY, and P. J. WANGERSKY. 1961. Absolute dating of deep-sea cores by the Pa^{231}/Th^{230} method. *Journal of Geology* 69:162–85. [PED, DMH, FCH☆]

SAMSON, P.-M., and C. RADULESCO. Les faunes mammalogiques du Pléistocène inférieur et moyen de Roumanie. *Comptes Rendus de Séances de l'Académie des Sciences,* Paris 257:1122–24. [FCH☆]

SCHUCHERT, CHARLES. 1936. Some recent geologic philosophy. *American Journal of Science* 32:147–50. [AK☆]

SHACKLETON, R. M. 1945. Geology of the Nyeri area. *Geological Survey of Kenya,* No. 12.

——. 1946. Geology of Miqori Gold Belt and adjoining areas. *Geological Survey of Kenya,* No. 21.

——. 1951. A contribution to the geology of the Kavirondo Rift Valley. *Quarterly Journal Geological Society of London* 106:345–89.

——. 1955. Pleistocene movements in the Gregory Rift Valley. *Geologische Rundschau* 43:257–63. [FCH☆]

SLUIJS, G. K. VAN DER, and W. H. ZAGWIJN. 1962. An introduction to the stratigraphy and geology of the Tegelen claypits. *Mededelingen van de Geologische Stichtung,* n.s. 15:31–37. [FCH☆]

SOLOMON, J. D. 1931. "The geology of the implementiferous deposits in the Nakuru and Naivasha basins and the surrounding area in Kenya Colony," in *The Stone Age cultures of Kenya Colony,* by L. S. B. Leakey. Appendix A, pp. 245–66. Cambridge: Cambridge University Press. [FCH☆]

STRAUS, W. L., JR., and C. B. HUNT. 1962. Age of Zinjanthropus. *Science* 136:293–95. [DMH☆]

TAYLOR, D. W. 1960. Late Cenozoic molluscan faunas from the High Plains. *United States Geological Survey Professional Paper* 337.

THURBER, D., W. BROECKER, and A. KAUFMAN. 1964. U^{234}/Th^{230} method of age determination and its application to marine carbonates. *Bulletin Geological Society of America* 75:166. [DMH☆]

VIRET, J. 1954. Le loess à bancs durcis de Saint-Vallier (Drôme) et sa fauna de mamifères villafranchiens. *Nouvelles Archives, Museum d'Histoire Naturelle, Lyon* 4:1–200. [FCH☆]

WEST, R. G. 1961. Vegetational history of the early Pleistocene of the Royal Society borehole at Ludham, Norfolk. *Proceedings of the Royal Society,* ser. B, 155:437–53. [FCH☆]

——. 1963. Problems of the British Quaternary. *Proceedings of the Geologists' Association* 74:147–86. [FCH☆]

WOLDSTEDT, PAUL. 1958. "Das Eiszeitalter," in 2nd edition, *Grundlinien einer Geologie des Quartärs.* Vol. 2, pp. 1–438. Stuttgart: Ferdinand Enke Verlag. [AK☆]

ZAGWIJN, W. H. 1957. Vegetation, climate, and time correlations in the early Pleistocene of Europe. *Geologie en Mijnbouw* 19:233–44. [AK☆]

——. 1960. Aspects of the Pliocene and early Pleistocene vegetation in the Netherlands. *Mededelingen van de Geologische Stichtung,* series C, III, 1(5):1–78. [FCH☆]

——. 1963. Pleistocene stratigraphy in the Netherlands, based on changes in vegetation and climate. *Verhandelingen van het Koninklijk Nederlands Geologisch Mijnbouwkundig Genootschap,* Geologisch serie, 21–22:173–96. [FCH☆]

ZÄHRINGER, J., and W. GENTNER. 1963. Radiogenic and atmospheric argon content of tektites. *Nature* 199:583. [FCH☆]