about by biological processes *within* the ecosystem in question." These statements define the conventional concept of autogenic succession and exclude changes brought about in the community by changes in the physical environment, allogenic succession in the usual terminology. In his discussion of lake succession "progressing in time" from the less productive (oligotrophic) lakes to more productive (eutrophic) lakes, Odum attributes this trend to the addition of nutrients from outside of the lake from the surrounding watershed. Erosion or leaching as a geological process is not a biological process, although organisms may modify the rate and kind of nutrient flow. In this sense, succession (eutrophication) caused by ordinary nutrient flow into a lake would not fit into Odum's definition of succession.

After commenting that nutrient import into a lake produces a successional change from oligotrophy to eutrophy, Odum states that adding nutrients to an experimental ecosystem is its equivalent, and the system (lake) is "pushed back" to a "younger" or bloom state. This implies a retrogressive succession in the common terminology, as younger is clearly an earlier or pioneer state or, one infers from the earlier statement, an oligotrophic state.

The apparent confusion may arise from a confounding of lake succession in the sense of oligotrophic to eutrophic with what Odum terms "seasonal successions which follow the same pattern." These might better be regarded as annual periodicities. In any event, the "bloom" or younger state referred to results, according to Odum, from an addition of nutrients and is thus parallel to eutrophy, the later stage in the ordinary lake succession, rather than earlier or oligotrophic state of lake succession, which contradicts his assumption of equivalence.

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Lake eutrophication as a successional process can be interpreted in several ways, as McIntosh has pointed out. I chose to limit "ecosystem development" to what he has called "autogenic succession" because the biotic processes of community development are quite different in nature and often in end result from the geochemical forces (erosion, glaciation, mountain building, and so on) or climatic changes that bring about "allogenic succession." The latter, in my view, is not really "succession" in terms of a unidirectional strategy, which is why I prefer the phrase "ecosystem development" to "ecological succession." Physical forces can cause changes in biological systems in all kinds of directions (such forces can both create and destroy lakes, for example). The history of a lake is, of course, the resultant of the interaction of biotic processes and physical factors, but I feel that a conceptual distinction between the autogenic and the allogenic process is of great practical importance, because it is man the "mighty" geological agent, rather than man the animal, who is causing the greatest disruption of those biogeochemical cycles which have been biologically controlled under near steady-state condition for long periods of time.

Perhaps the well-documented story of Lake Washington in Seattle (*1*) will illustrate my position. Studies on diatom remains in the sediments in this lake (*2*) indicated a "near steady state condition" (the investigators' words) for many years preceding the period when sewage enrichment caused a progressive decline in water quality (decreased transparency, nuisance algal blooms, oxygen depletion, species change, and so on). When the citizens of Seattle and its suburbs finally passed a "metro" bond issue that resulted in complete diversion of treated sewage from the lake during 1964 to 1967, reversal of eutrophication was dramatic. Edmondson (*3*) thinks that the lake, if now left to its own devices, will return to its former less eutrophic condition despite the fact that a large supply of phosphorus and other nutrients introduced during the sewage era remain in the sediment. He points out that since exchange between sediments and water involves mostly the surface layers of the former, the excess nutrients will soon be buried and out of circulation. Studies of sediment cores of Italian lakes show that intense road building and other cultural activities of the Romans caused marked eutrophication which reversed when these activities declined (*4*).

Enrichment or eutrophication is a natural process during the early stages of ecosystem development. However, continued enrichment to the point of producing cancerous overgrowth and the ultimate destruction of the system is not part of life-system strategy, but is usually caused by some vast outside disturbance that produces inputs of materials or energy at rates that cannot be assimilated.

As I already said, "it is the entire drainage or catchment basin, not just the lake or stream, that must be considered the ecosystem unit if we are to deal successfully with our water pollution problems" (*5*). Much of the money now allocated by federal bureaus for water pollution study and abatement is being wasted in nearsighted mission orientation. The cause of and the solutions for water pollution are not to be found by looking into the water; it is bad management on the watershed by terrestrial man that causes lakes and streams to sicken and die.

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25 July 1969

Caribbean Cores P6304-8 and P6304-9: New Analysis of Absolute Chronology

Taken at face value, the new Pa^{231}/Th^{230} ages obtained by Rona and Emiliani (*1*) provide strong support for the absolute time scale previously obtained for Caribbean cores (*2*). Moreover, these authors conclude that the ages they obtained have greater precision (*3*) than the previously published results and provide a more accurate time scale for the past 170,000 years. We question the reliability of the data from which these new dates were calculated. Shown in Table 1 are comparisons between the data published by Rona and Emiliani (*1*) and those we have obtained on the same cores. In three of the four pairs the samples are separated by sufficiently small depth intervals to permit direct comparison. Differences greatly exceeding the experimental uncertainties are present.

The largest differences are in uranium content. Although Rona and Emiliani find (on a carbonate-free basis) values of 0.91 to 1.13 parts per million, we obtain 2.08 to 2.39 ppm. Figure 1 is a scatter diagram of the uranium content in the noncarbonate fractions of six cores from the Caribbean. If the data of Rona and Emiliani (*1*) are excluded, a total of 34 measurements made by D. L. Ferguson and E. J. Fennelly (both of the U.S. Geological Survey) and by us gives an average of 2.4 ppm, the lowest value being 1.6 ppm. That most of the uranium concentrations in the clay fraction of deep-sea sediments lie between 2 and 3 ppm has been noted by Ku (*4*). Among over 90 measurements made on 11 *Globigerina* ooze and red clay cores from different parts of the ocean (*4*), only four samples yield values less than 1.5 ppm.

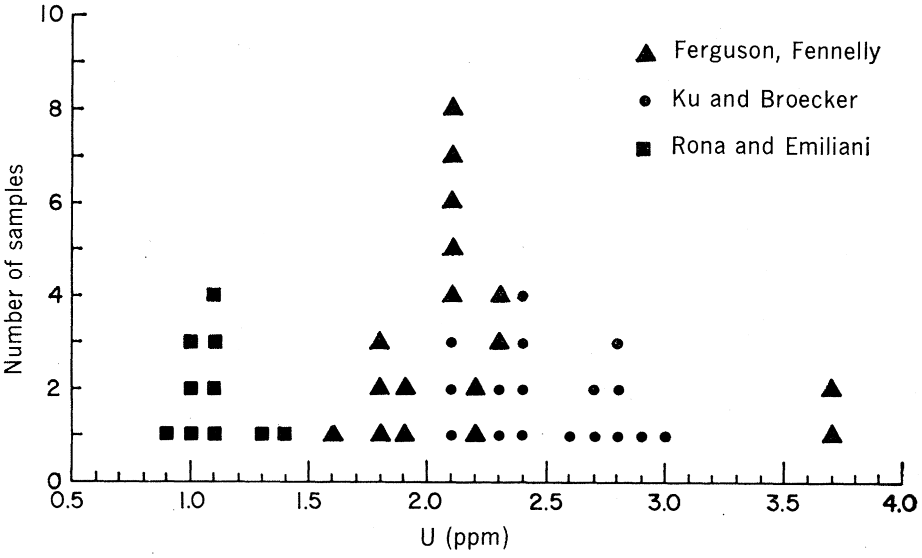

Fig. 1. Scatter diagram of uranium content (on a carbonate-free basis) in six Caribbean cores (V12-122, A240-M1, A179-4, A254-BR-C, P6304-8, and P6304-9). Sources of data: Ferguson and Fennelly [cited in Rosholt *et al.* (*2*)]; Ku and Broecker (*13* and data reported here); and Rona and Emiliani (*1*).

Although the uranium content of the cores is needed only to correct for the amounts of parent-supported Pa^{231} and Th^{230}, an increase of a factor of 2 in the correction will cause a substantial change in the ratio of unsupported Th^{230} to Pa^{231}. Table 2 shows how these ratios change if the uranium content of all the samples analyzed by Rona and Emiliani is taken to be 2.2 ppm. At the top of the core the ratios decrease, and below a depth of about 300 cm they increase (*5*). Ages for the deeper sections of the cores calculated from the corrected ratios would be considerably higher (up to at least 20 percent) than those given by Rona and Emiliani (*1*). Until the question of the uranium content has been resolved, the ages given by these authors cannot be accepted.

We have used the results obtained on core V12-122 as evidence that, beyond the range of C^{14}, the time scale given by Rosholt *et al.* (*2*) is 25 percent too low (*6*). Rona and Emiliani (*1*) cast doubt on this core because it fails to yield the ideal ratio of Th^{230} to Pa^{231} at its top [that is, that ratio which would obtain if no separation between Pa^{231} and Th^{230} occurred during their removal from seawater (*7*)]. The commonly made assumption that Pa^{231} and Th^{230} are geochemically coherent is not valid. A large number of deep-sea cores show $\mathrm{Th}^{230}/\mathrm{Pa}^{231}$ values for the top layers higher than the ideal ratio of 10.8 (*4, 8*). The anomalies cannot be explained on the basis of missing core material, sediment reworking, or upward migration of Th^{230} (*4*). On the contrary, of the 27 manganese nodules analyzed (*9*), 21 samples display surface $\mathrm{Th}^{230}/\mathrm{Pa}^{231}$ ratios ranging from 2 to 10. By chance, Caribbean cores come closest to the ideal value. Our analyses of P6304-8 and P6304-9 suggest top values close to 13, the value we obtain for V12-122. Thus we claim that these two cores analyzed by Rona and Emiliani (*1*) are no different in this respect than V12-122. They do not have the ideal initial ratio.

Dating by the Pa^{231}-Th^{230} method should therefore not be done by using the so-called ideal value (*7*) as the initial $\mathrm{Th}^{230}/\mathrm{Pa}^{231}$ ratio. Instead, the logarithm of a series of measured ratios should be plotted against depth, and the slope of the best-fit line should be used to establish the average sedimentation rate. This rate can then be used to calculate ages. If the points fail to define a straight line, then the core does not lend itself to a unique geochronologic interpretation. This is equivalent to saying that a single initial $\mathrm{Th}^{230}/\mathrm{Pa}^{231}$ ratio applies to all levels in the core and that the sedimentation rate has been nearly constant over the past 150,000 years. On the other hand, according to Rona and Emiliani (*1*), average sedimentation rates in the two cores would be subject to considerable changes. For example, in core P6304-9 sedimentation

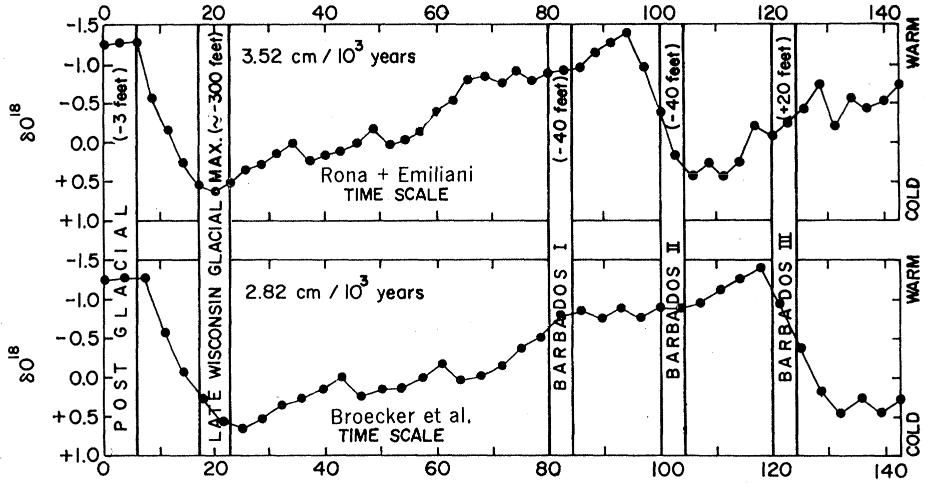

Fig. 2. Correlations between the O^{18} record in core P6304-8 and the times of near present sea stands (*6*), the time of the last glacial maximum, based on Rona and Emiliani's (*1*) time scale for deep-sea cores, and the time scale suggested by Broecker *et al.* (*6*). In each case a constant sedimentation rate best fitting the data in the range from 10,000 to 125,000 years has been applied to the entire record.

occurred for the top 2 m at the rate of 3 cm per 1000 years and for the next 2 m at the rate of 6 cm per 1000 years. Since the carbonate content is rather uniform throughout the cores, such large variations in sedimentation rate are difficult to explain and are contradictory to the conclusion of Emiliani (*10*).

Either the analyses presented by Rona and Emiliani (*1*) or those presented by Ku and co-workers (*4, 6*) must be in serious error. One test of the validity of an analysis scheme is the measurement of samples sufficiently old that Pa^{231} must be at equilibrium with its parent, U^{235}, and Th^{230} must be at equilibrium with its parent, U^{234}. We have analyzed sediments over 2 million years of age (that is, core V18-258, 400 to 405 cm) and a Black Hills uraninite sample having concordant U/Pb ages of 1600 million years. Within the experimental error (1 sigma of counting statistics), activity ratios of unity for Th^{230}/U^{234} and Pa^{231}/U^{235} were obtained (*4*). Furthermore, our measurements on coral samples from the Barbados Island have yielded concordant Th^{230} and Pa^{231} ages (*11*). Thus neither a serious calibration error nor a blank problem exists for the data used in our calculations.

Measurements of magnetic reversals made by Ericson and Wollin (*12*) on cores in which the boundary between the U and V zones has been identified provide further evidence in support of our position. By extrapolation Rona and Emiliani (*1*) obtain an age of 313,000 years for this boundary. A 25 percent increase would alter this boundary to 392,000 years. The data on magnetic reversals for five cores yield ages ranging from 390,000 to 440,000 years.

Finally, we call on a geologic argument. If we use Rona and Emiliani's (*1*) results, there is a serious discrepancy between the climatic record implied by cores P6304-9 and P6304-8 and that suggested by the raised coral reefs on Barbados (Fig. 2). The ages of the latter are well documented by independent Pa^{231} and Th^{230} (*6, 11*) dates. Interglacial sea stands occurred 82,000, 103,000, and 122,000 years ago. If Rona and Emiliani's time scale is accepted, the 103,000-year-old reef must have been formed during a prominent O^{18} cold period. On the other hand, if our time scale is adopted, all three periods of coral growth fall within the same O^{18} warm interval.

For these reasons we feel that the conclusions of Rona and Emiliani (*1*) regarding the absolute chronology of the O^{18} record in Caribbean cores cannot be accepted.

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Table 1. Comparison of uranium series results obtained for Caribbean cores by Rona and Emiliani (*1*) with those reported here; dpm, disintegrations per minute.

Analyst	Sample depth (cm)	$CaCO_3$ (%)	U† (ppm)	Th^{230}* (dpm/g)†	Pa^{231}* (dpm/g)†	Th^{230}/Pa^{231}*
				P6304-8		
Rona	21–29	64.3	1.13	7.4	0.67	11.0
Ku	16–17	66.5	2.40 ± 0.09	10.6 ± 0.5	0.80 ± 0.03	13.3 ± 0.8
Rona	321–326	60.0	0.91	3.03	0.107	28.4
Ku	311–312	53.2	2.10 ± 0.09	3.91	0.135 ± 0.007	29.0 ± 2.0
				P6304-9		
Rona	82–86	56.2	1.13	4.9	0.38	13.0
Ku	16–17	59.5	2.08 ± 0.08	14.8 ± 0.6	1.16 ± 0.05	12.7 ± 0.7
Rona	321–326	57.0	0.95	3.64	0.123	29.5
Ku	311–312	48.6	2.39 ± 0.08	4.03 ± 0.25	0.100 ±0.006	40.3 ± 3.5

* Corrected for uranium-supported activities. † Calculated on a carbonate-free basis.

Table 2. Sensitivity of unsupported Pa^{231} and Th^{230} activities and of their ratio to choice of uranium content.

Depth (cm)	Reported uranium content			Uranium content of 2.2 ppm		
	Th^{230} (dpm/g)	Pa^{231} (dpm/g)	Th^{230}/Pa^{231}	Th^{230} (dpm/g)	Pa^{231} (dpm/g)	Th^{230}/Pa^{231}
			P6304-8			
25	7.38	0.67	11.0	6.52	0.63	10.4
155	3.96	.20	20.2	3.01	.157	19.2
324	3.03	.107	28.4	2.03	.062	32.8
392.5	2.93	.090	32.6	2.03	.048	42.2
635	0.95	.013	71.0	0.28	−0.017	
			P6304-9			
84	4.91	0.38	13.0	4.05	0.34	11.9
194	5.66	.27	21.2	4.65	.22	21.1
323.5	3.63	.123	29.5	2.66	.080	33.2
398.5	3.43	.107	32.2	2.73	.075	43.0
533.5	1.87	.033	56.0	1.00	−0.007	

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3. Rona and Emiliani do not give the errors associated with their radiochemical measurements, but they use the spread given by two or three duplicate measurements for the assignment of age errors. This procedure is misleading, since the spread of a number of data is statistically meaningful only if a sufficiently large number of measurements has been made. Even if we assume that the age uncertainties indicated by Rona and Emiliani give correct estimates of deviation from the means (which in all but one case have a percentage error between 3 and 4 percent), this requires that the standard deviations of the measurements of U^{234}, U^{235}, Th^{230}, and Pa^{231} all must be close to 1 percent. Such a precision, unfortunately, was neither discussed nor demonstrated by Rona and Emiliani.
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5. Only when the measured activity ratio of Th^{230} to Pa^{231} equals that of U^{238} to U^{235}, that is, 21.8, is it unaffected by any uranium correction. Ratios with Th^{230}/Pa^{231} greater than 21.8 will be underestimated if too low a uranium concentration is used for the correction, whereas ratios less than 21.8 will be overestimated; that is to say, an undercorrection for uranium tends to reduce the differences between Th^{230}/Pa^{231} ratios for the top portions of a core and those for the deeper parts, thus giving rise to a sedimentation rate that is too high and ages that are too low.
6. W. S. Broecker, D. L. Thurber, J. Goddard, T. L. Ku, R. K. Matthews, K. J. Mesolella, *Science* **159**, 297 (1968).
7. This is the activity ratio of Th^{230} to Pa^{231} newly generated by decay of the dissolved uranium in the sea and is shown (see *9*) to have a value of 10.8. Rona and Emiliani (*1*) ignore the well-established observation of a 15 percent excess of U^{234} over U^{238} in the ocean and choose a value of 9.4 to fit their measured ratios for the tops of the cores. This is unjustified, as one may ask: What is the fate of those Th^{230} atoms produced from the excess U^{234}?
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