which is not clear. As with the complicated shape of the Haq *et al.* "long-term" curve, the authors should propose a mechanism.

Further refinements to the eustatic curves will come from additional isotopic data on deep sea cores and from additional core drilling in bank margin carbonate regions with relatively simple subsidence history and open-ocean, high-quality chronostratigraphy. Use of a high-quality eustatic curve in a forward model of sea level interaction with basin subsidence (*9*) will help bring stratigraphic prediction to more complicated regions and be productive to the long-term progress of the emerging science of deductive stratigraphy (*10*).

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9 July 1987; accepted 19 February 1988

*Response*: Matthews suggests that, because we did not discuss the physical explanation for sea level changes in our article, the wealth of scientific observations on which the sea level curves are based is somehow invalidated. There would be little progress in geological sciences if we ceased to make inferences from a multitude of observed facts every time we could not speculate about their causal mechanisms. In the case of sea level changes, however, Matthews is mistaken: we suggested such mechanisms in our earlier papers (*1*). It is well known that long-term (second-order) global sea level changes are most likely related to changes in the volume of ocean basins driven by changes in the rate of sea-floor spreading and ridge volume. The shorter term eustatic changes (on the time scales of third-order events) are most probably due in large part to changes in polar ice volume and possibly in part to some other, as yet unknown, mechanism. Studies in different parts of the world clearly indicate global changes on the time scale of third-order events. Proving the relation between ice volume and sea levels or finding other mechanisms capable of explaining these fluctuations should remain a top priority, but they should in no way lessen the value of these studies.

We pointed out in our article that our cycle charts are restricted to first-, second-, and third-order events. The eight major late Pleistocene sea level highstands that Matthews mentions are fourth- and fifth-order events that are beyond the resolution aimed for in our cycle charts of the last 250 my. In fact, the last 800,000 years are represented by only a part of a third-order cycle (lowstand systems tracts), whereas there are numerous higher-order events in the same interval, as there are throughout most of the Plio-Pleistocene.

Matthews compares his (as yet unpublished) oxygen isotope-based "sea level" curve with ours. The consensus opinion is that, since the mid-Tertiary, the isotopic signal may be in large part due to the ice-volume effect. However, its relation to ice volume, if any, before the mid-Tertiary remains debatable. Although it is our opinion that ice-volume fluctuations can best produce third-order sea level changes, we do not think the oxygen-isotopic signal for much of the Tertiary is a true representation of the ice-volume effect.

Matthews appears to have misinterpreted our long-term curve. As we stated in our article, both the long-term and short-term curves have been corrected for the tectonic (subsidence) component. Coastal onlap and geohistory patterns are the backbone of our interpretations, and unlike Matthews, we consider them "solid geophysical" data. These data, and sequence-stratigraphic analysis of sections around the world, provide important constraints for the long-term curve, which essentially represents the ice-free highstand envelope. That is the reason for the suppressed amplitude of the short-term curve compared with that of the long-term curve since the mid-Tertiary.

Matthews also argues that the mid-Oligocene event does not show up in the deep-sea isotopic record and therefore it must be due to tectonism. In the geological record this event is depicted as a basinward shift of strandlines and canyon cutting on the shelves of many disparate parts of the world, which would effectively rule out a tectonic explanation. Also Matthews states incorrectly that the event is scarcely observed in the deep sea isotopic record (see references in *2*).

Finally, Matthews assumes that tropical sea-surface temperature has remained constant through time, implying that the oxygen-isotopic signal is largely due to ice volume throughout the Tertiary and is therefore a true representation of sea level change. We consider this assumption to be uncalled for! Although the isotopic data will inevitably help in this effort, further refinements of the sea level curves will come from the detailed studies of continental margin sections, a better understanding of their subsidence histories, and the use of better and multiple stratigraphic tools to ensure accurate correlations.

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20 October 1987; accepted 19 February 1988

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The article "Chronology of fluctuating sea levels since the Triassic" (*1*) provides a new version of the Exxon-based sea level and sedimentary cycles chart, calibrated to a new geological time scale. It is not clear to us (i) how and why this time scale differs from other recently published scales (*2–5*); (ii) what the criteria are that are used to correlate the first-, second-, and third-order sedimentary onlap events; and (iii) where the type sections are for the sedimentary sequences.

Our commentary is directed to the following points: (i) the time scale appears to be constructed from mixtures of low- and high-temperature ages, which arbitrarily lengthens or shortens its segments; (ii) some correlations are not well documented and, where sufficient documentation exists, in several cases the correlations can be shown to be erroneous; and (iii) the chronostratigraphic framework is insufficient to test the existence and age of third-order cycle boundaries.

The authors (*1*) criticize the preferential use of one set of isotopic ages over another (referring to high- versus low-temperature ages) and indicate that adoption of one technique over another introduces a "distinct bias" and "ignores a large body of potentially valuable analytical and empirical data. . . ." They base their time scale on both high-temperature and low-temperature (glauconite) isotopic ages. Other time scales have been based primarily on either high-temperature (*2, 3, 5*) or low-temperature (*6*) ages. Although some of the glauconite ages used by the Exxon group are concordant with the high-temperature ages used (Fig. 1), many are discordant. In general, glauconite ages are younger than correlative high-temperature ages (Fig. 1). In this regard it should be noted that the author of most of the glauconite ages (7) adds qualifications such as that for sample NDS 2: "39.6 ± 1.8 Ma [million years ago] is a minimum age . . . bearing in mind the long time necessary for the evolution of the dated glaucony. One should therefore add 1.5 to 2 my (about 1 biozone + duration of genesis) to give a number representative of the limit. . . ." It appears to us that this qualification should be applied to any and all suitable glauconites. In the absence of independent criteria, it appears prudent not to mix high- and low-temperature ages if at all possible (*8*).

Because the low- and high-temperature dates are mixed, the Exxon time scale shows the Jurassic to be 13 my longer than in other scales (*3–5*). This is because it relies on published high-temperature ages for the base of the Jurassic, the average being around 208 to 210 Ma, and low-temperature ages for the Oxfordian-Kimmeridgian of 138 to 148 Ma, as well as several low-temperature (glauconite) ages for the Tithonian (*2, 3, 7–9*). Nevertheless, high-temperature ages for the Oxfordian-Kimmeridgian boundary are greater than 150 Ma (*10, 11*). Similarly, their young Aptian-Albian age of 108 Ma is in disagreement with a series of excellent high-temperature ages of 113 ± 1.4 Ma and 112 ± 2 Ma (*10, 12*) dated with $^{40}Ar–^{39}Ar$ on bentonites from the *Parahoplites nutfieldensis* zone of the latest Aptian age in England and northwestern Germany.

In the Tertiary, high-temperature ages for the early-middle Eocene boundary are at 52 Ma (*13*, p. 162; *14*), while glauconite ages from this interval are 45 Ma (*6*). The Exxon authors (*1*) average these divergent high- and low-temperature ages and place this boundary at 49 Ma. In our view such biased averaging does not increase the reliability or stability of a time scale (*1*, p. 1158).

Every time scale requires some interpolation between directly dated stratigraphic levels. Magnetochronology, biochronology, and statistical techniques have been used by investigators to infer the numerical ages of stratigraphic boundaries including stages (*2, 3, 5, 8, 11, 15, 16*). In our view, the equal duration of standard ammonite zones (*3, 4*) or subzones (*17*) between selected tie points and checkpoints based on clusters of high-temperature dates for the time being is a reasonable approach for the Jurassic. Stages were scaled in time according to the number of standard zones or subzones. The simple postulate of constant mean duration of Jurassic ammonite zones or subzones, although not required by evolutionary theory, is preferred (*17*) over a loose framework constructed with poorly known and biased isotopic age mixtures. The latter makes the Exxon estimates for the (subboreal) ammonite zones in the Hettangian and Aalenian a factor of 4 or more longer than in intermediate stages, which is unlikely.

For the Late Jurassic to Early Cretaceous and from the Late Cretaceous to Recent our interpolation scheme (*3–5, 13*) was based on construction of a standard magnetochronology from the examination and comparison of sea-floor marine magnetic anomaly spacing in different ocean basins (*18*). The Exxon authors (*1*) criticize a tiepoint approach with geological interpolations in between and instead propose a "best-fit" approach with the use of all "analytically sound and stratigraphically constrained radiometric dates." They do not, however, specify criteria for selection or do they refer to the specific dates used, but they imply that their methodology in assigning ages is more objective than previous attempts.

We also question the correlation of various microfossil groups with the magnetic polarity record and with the numerical time scale. The criteria for such correlations are unspecified other than with general references to the literature. We can cite several examples where the correlations provided are either speculative or are based on older literature, and are incorrect.

1) It is premature to extend the M-series of anomalies back to PM29 (*1*), because the pre-M25 biomagnetostratigraphy in the sedimentary record is tentative at best and not correlated with the Pacific or Atlantic ocean anomalies.

2) The Exxon authors define the Jurassic-Cretaceous boundary in a subboreal sense, between the Portlandian and the Ryazanian; but it is not clear how this ties to the magnetochronology at M15–16. Also it is not clear why the *Calpionella* Zone B is placed with polarity chron 17 and in the Berriasian, rather than, as we have shown, with chrons 19n-17 (*19*) at the Tithonian-Berriasian boundary.

3) A few years ago, Epoch 9 was correlated with Anomaly 5 (*16*), and this correlation was used by the Exxon authors (*1*). We have since then shown that Anomaly 5 can be correlated with Epoch 11 (*20*), which requires recalibration of foraminiferal and nannofossil zones near the boundary of the middle and late Miocene. The Exxon authors (*1*) adopted ages for nannofossil zones that are similar to ours and placed the base of the upper Miocene at 10.2 Ma. Yet, both of the correlations are valid only if Epoch 11 correlates with Anomaly 5.

The ability to differentiate interregional sea-level fluctuations and thus test the Exxon cycle chart is a function of stratigraphic uncertainties. In order to establish the validity of the cycles as eustatic events, they must be proved to be synchronous in different locations. Recent studies have determined that it is possible to establish good interre-

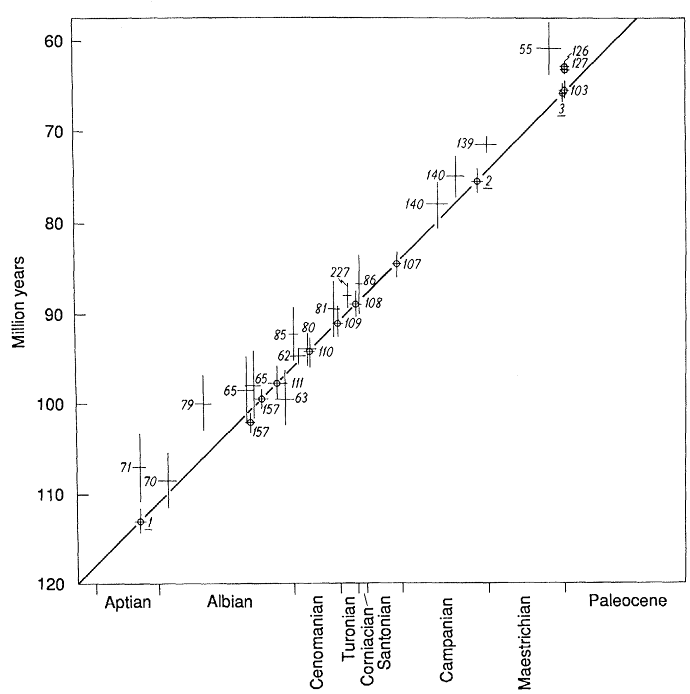


**Fig. 1.** Plot of radiometric ages based on high-temperature (ϕ), and low-temperature (+ = glaucony) dates, that shows the systematic trend in the glaucony dates toward younger values. Numbers identify specific dates as reported [after Aubry *et al.* (*22*)].

gional correlations of the second-order sequence boundaries (*21*), strengthening the case that these were, in fact, caused by eustatic falls. We believe it is premature to correlate consistently and interregionally higher-order sea-level cycles. For example, the mean duration of the third-order Cenozoic cycles is about 1.5 my, while biostratigraphic resolution in this interval is typically 1 to 2 my. Until it is documented in detail that (i) minor cycles can be recognized both regionally and globally and (ii) these minor cycles are synchronous in a global sense, we believe such minor sequences cannot be used for global geochronology.

Nevertheless, we admire the attempt to unify such a large body of data.

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9. Ages between 150 and 157 Ma from U-Pb analysis in zircons from igneous rocks in northern California and Oregon bracket the *Buchia concentrica* zone, mid-Middle Oxfordian to mid-Upper Kimmeridgian [J. B. Saleeby, J. D. Harper, A. W. Snoke, W. D. Sharp, *J. Geophys. Res.* **86**, 3831 (1982); J. B. Saleeby, *Geol. Soc. Am. Ann. Mtg. Alaska* **16**, 5, 331 (1984) (abstract)].
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3 November 1987; accepted 19 February 1988

*Response*: Gradstein *et al.* do not address the main issues relevant to the discussion of sea level curves, but instead criticize the time scale to which the curves are calibrated. When we began assembling global sequence stratigraphic data, we noted the wide diversity of approaches and varying rigor in the treatment of data in the existing time scales. We recognized the need for a chronostratigraphy with internal consistency for the Mesozoic and Cenozoic that integrates geochronologic and magneto-, bio-, and sequence-stratigraphic data. At the same time, we did not want to ignore a large body of good analytical data or make unnecessary assumptions. We believe that we have been successful in these objectives and that the resulting chronostratigraphic criteria are robust and reliable. However, as *Science* is not a place to publish detailed documentation, most of the questions posed by Gradstein *et al.*, including those about our time scale, our criteria for correlation of onlap events, and our reference sections, are addressed in a detailed forthcoming paper (*1*).

The crux of the disagreement between our approach and that of Gradstein *et al.* lies in the use of radiometric data. Whereas they appear to favor a few selected high-temperature dates as isolated tie points, we regard the assumption of constancy of rates between widely separated anchors that this approach implies as unwarranted. We prefer to constrain our time scales with both high- and low-temperature dates, but we employ the latter with an important qualification [see discussion in (*1*)]. Many glauconite dates do have inherent analytical and geochemical problems, but so do most high-temperature dates. We find it regrettable that, whereas low-temperature dates have been criticized widely, often for good reasons, very little is said about equally significant problems with high-temperature dates.