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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23. We thank L. N. Ford, Jr., G. Jones, and R. C. Tjalsma (Brea, CA); A. C. Grant (Dartmouth, Nova Scotia); and L. Mayer (Halifax, Nova Scotia) for constructive criticism of the manuscript. This is Geological Survey of Canada publication number 29987.

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.

In reality, most radiometric dates are affected by several sources of error inherent in the samples and the analytical techniques that limit their usefulness. We have discussed these issues at length in (*1*).

Unlike Gradstein *et al.*, we reject the use of widely separated tie points because that approach would ignore possible trends inherent in the data. In our view, a qualified use of a large set of analytically acceptable dates with known stratigraphic limits can provide important constraints for the time scale and can better approximate reality than the use of singular dates, no matter how good, used in isolation to nail down relatively long segments of the stratigraphic column.

In our experience, when reliable high- and low-temperature dates are available for the same stratigraphic interval (see *1*, figures 3 to 6) there is significant overlap between the ranges of high-temperature dates and the older part of the ranges of low-temperature dates. We have qualified our use of the low-temperature dates by weighting the best-fit solution in favor of the older range ends of these dates when no high-temperature data are available. This approach not only guards against "arbitrarily" lengthening or shortening segments of time scale, but also ensures the detection of inherent trends in the data. As for adding error-bars on radiometric data for low-temperature dates, we suggest adding such qualifications to high-temperature dates as well. We prescribe such limits of uncertainty for all boundaries in (*1*).

Gradstein *et al.* cite two dates to support their case for an older age of the Aptian-Albian boundary. One of these (112 ± 2 Ma) is a $^{40}Ar/^{39}Ar$ date for a secondary bentonite of Late Aptian age found in a level midway in the Sandgate Beds of the Aptian Lower Greensand Formation (*2*). The Aptian-Albian boundary is placed within the upper Folkstone Beds, above the Sandgate Beds, to which the authors of the date in question (*2*, figure 3) assign an age of 107 Ma—a million years younger than our age of 108 Ma for this boundary. The uncritical use of such dates is a good illustration of why one should not nail down chronostratigraphic schemes with singular dates, no matter how "excellent," while ignoring other, analytically sound, and equally acceptable, data that may not agree with our preconceptions.

Contrary to the suggestion of Gradstein *et al.*, our Tertiary time scale is not a biased averaging of high- and low-temperature dates. Nor is it a simple interpolation between distant points. Instead, it is a conscious effort to find trends in a large set of radiometric dates, with the qualified use of low-temperature dates where no reliable high-temperature dates were available (*1*).

For the Jurassic time scale Gradstein *et al.* defend the assignment of equal duration to ammonite zones (subzones) and use this approach to find the relative duration of the stages. Once again, the implied assumption of uniform evolutionary rates among ammonites over long periods of time is unnecessary (*3*).

Our criteria for the selection of radiometric dates were clearly stated in our article, that is, those dates that are analytically acceptable and stratigraphically constrained (*4*). Our response to the numbered queries are as follows:

1) The authors correctly point out that it is premature to extend the M-series anomalies beyond anomaly M25 and that these correlations should be considered tentative. And yet they, (*5*) among others, have included older anomalies in their schemes, presumably because the implied tentative nature of those anomalies is widely known.

2) Following a North Sea usage, we placed the Jurassic/Cretaceous boundary at the top of Portlandian, which is tied to the boreal *lamplughi* ammonite zone that corresponds to the *occinata* ammonite and *C. elliptica* calipionellid zones of the Tethyan region. These in turn are tied to anomaly M16 (*6*). The authors also refer to the lower boundary of *Calpionella* zone B and polarity chron M17, which is not shown on our Cretaceous cycle chart. We assume they are referring to an unpublished earlier composite that was corrected in a later published version (*8*).

3) Concerning the correlation of magnetic epoch 9 with anomaly 5, we had originally planned not to use the older system of magnetic epochs and instead to use the new chron terminology consistently for the Callovian to Recent (*9*). However, to facilitate comparison with earlier references to magnetic epochs in pre-1983 literature, we subsequently added magnetic epochs to the scheme following the most recent correlations suggested by leading paleomagneticists (*9*). The revised correlation of anomaly 5 with epoch 11 was, however, brought to our attention by other colleagues (*10*) in time for us to correct this in our later published version of the chart (*8*).

We agree readily with Gradstein *et al.* that stratigraphic resolution is of prime importance in testing the eustatic model. We have attempted to present a refined model of the sea level changes of the past 250 my that is calibrated to a state-of-the-art and internally consistent biochronostratigraphic scheme. It is based on a well-documented methodology and on data largely from the public domain. Our global documentation consists of rigorous pattern-matching of sequences and systems tracts in different basins, including litho- and biofacies analyses that help weed out local events and ensure the retention of consistent and widely distributed events. As this model is tested by others in places away from our reference areas [listed in appendices C to F of (*1*)], and as the use of multiple bio-, magneto-, chemo-, and sequence-stratigraphic tools lends greater confidence to such correlations, the model will inevitably be modified and refined where needed. Such is the nature of our science.

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4. Criteria for evaluating the analytical soundness of results obtained from various radiometric techniques are discussed at length by authors in G. S. Odin, Ed., *Numerical Dating in Stratigraphy* (Wiley, New York, 1982). Dates from samples that cannot be placed with confidence within fairly narrow chronostratigraphic limits are considered irrelevant.
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22 June 1988; accepted 24 June 1988

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