

Sea Level History

Bilal U. Haq and his co-workers have completed an important update of the chronology of coastal onlap and eustatic fluctuations in Mesozoic and Cenozoic time (1). Seismic stratigraphic results are augmented in the new charts by outcrop and well-log studies to document an impressive total of 119 sea level cycles since the beginning of the Triassic. In addition, the Cretaceous results have been published officially for the first time. However, apart from distinguishing between relative changes of coastal onlap and eustasy, the methodology and assumptions are much the same as those used to construct the first version of the "sea level curve" in 1977. In a recent evaluation of the seismic stratigraphic record of sea level change (2), we drew attention to two problems in particular.

1) All of the observed depositional cycles are assumed by Haq *et al.* to be eustatic. Nearly 50% of the sequence boundaries cannot be identified in seismic data. For many of these minor boundaries it is difficult to demonstrate a downward shift in coastal onlap and to eliminate the possibility that a given boundary might be due to autocyclicity or to fluctuations in sediment supply rather than to a lowering of depositional base level. In spite of considerable recent efforts to calibrate sequence boundaries, it is not possible to determine the ages of many of the minor ones (that is, third-order cycles) sufficiently well to permit objective correlation between basins because the spacing of the boundaries is close to or finer than biostratigraphic resolution. Matching patterns of sequences are valid only insofar as a global sea level signal is known to be present. We agree with Haq *et al.* that of the 61 seismically resolvable sequence boundaries, many may prove to be of eustatic origin, but questions remain about the calibration even of these boundaries to the geological time scale.

2) The global onlap chart, which forms the basis for the smoothed eustatic curve, has little physical meaning. Coastal aggradation (the vertical component of coastal onlap) is primarily a result of basin subsidence. It is not even a good approximation of relative sea level rise because the datum changes according to whether the onlapping strata are truly coastal or accumulated in an alluvial environment. In addition, the amount of aggradation measured in a given sequence varies from one seismic section to another, and where differential subsidence is pronounced, it is critically dependent on the

path taken across any particular section. Because coastal aggradation is measured incrementally, corrections for subsidence are difficult to apply. Downward shifts in onlap are a response not to a sea level fall, but to an increase in the rate of sea level fall. Thus even if a downward shift is somehow corrected for the effects of subsidence and for datum errors, the shift in onlap still provides little information about the magnitude of sea level change. Similarities in the patterns of coastal onlap for different basins for the most part indicate similar overall subsidence history. Combining onlap charts for different basins is equivalent to estimating the average subsidence history of basins. Although Haq *et al.* title their article "Chronology of fluctuating sea levels since the Triassic" and portray the inferred amplitudes of eustatic oscillations only as a best estimate, we do not think that smoothing a global onlap chart is a valid method for making such an estimate.

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Response: We are sympathetic to the arguments presented in the critique by Nicholas Christie-Blick *et al.* (1). They state that the new version of the sea level curves is based on much the same methodology as the 1977 version. As we pointed out, the new version is based on the recognition of depositional sequences in outcrops and well logs, in addition to seismic data. Sequences are subdivided into genetically related sedimentary units (systems tracts) that are interpreted as the sedimentary response to various phases of the sea level cycle. Sequence analysis of outcrops can be undertaken independent of seismic data and therefore represents a new methodology that augments the approach used in preparing earlier versions of sea level curves. Those were based entirely on coastal onlap patterns in seismic profiles, dated by biostratigraphy from well data.

We disagree with the critique that all observed depositional cycles are assumed to be eustatic. The succession of sea level events interpreted from depositional sequences and systems tracts and corrected for

subsidence in any region are correlated with similar successions of events from other, often widely separated, regions. Rigorous pattern-matching of not only the sequences but also the systems tracts within the sequences, together with litho- and biofacies information, help weed out local events and ensure the retention of consistent and widely distributed events.

Admittedly, minor events are more readily identified in outcrops and well logs. However, it is not strictly correct to say that minor sequences can not be identified on seismic profiles. The characteristic geometric response of minor sea level falls may be subtle, but the development of these geometric patterns is a function of sediment supply, and minor sequences are easily resolvable in thick sections. Moreover, when seismic data is augmented with well data, even in relatively thin sections minor sequence boundaries become easily detectable. A good example of this is provided by the third-, fourth-, or even fifth-order sequences of the Quaternary that are obvious on seismic profiles and well logs in the Gulf of Mexico (2). Downward shifts of coastal onlap are more difficult to demonstrate for minor boundaries. However, minor sequence boundaries are identified by a number of criteria, not just downward shifts of coastal onlap. A sequence boundary is marked and identified on well logs and in outcrops by (i) truncation below the boundary, (ii) onlap onto the boundary, and (iii) a basinward shift in facies associated with the boundary (a basinward shift in facies is characterized by shallow-marine or nonmarine rocks above the boundary resting sharply on deeper marine rocks, such as shelf mudstones, below the boundary with no intervening rocks deposited in intermediate depositional environments). These three criteria must be identified regionally before the surface that they define as a sequence boundary can be interpreted. When observed in well-log cross sections and outcrops, these three characteristics indicate that sequence boundaries are the result of a relative fall in sea level and are not the product of autocyclicity. Distributary channel erosion, associated with rapidly prograding shifting delta lobes, can locally produce erosion and an apparent basinward shift in facies. However, where they erode into prodelta mudstones, distributary channel sandstones are laterally encased in stream-mouth bar and delta-front rocks. This lateral facies relation exists because the distributary channel cannot build seaward unless it moves over subaqueous delta platform, even if the deltaic progradation is extremely rapid. The resulting vertical succession of facies, with local exceptions produced by rapid progra-

dation of a delta is a normal vertical association of facies. In this way, autocyclic events can be identified within systems tracts and depositional sequences. Our observations of regional basinward shifts in facies at minor sequence boundaries are in contrast to this normal succession. Influence of autocyclic events and base-level changes can also be discounted by cross-comparison of sequence and systems-tract patterns in different sections within a basin and with other basins. Events caused by autocyclic processes will not correlate either within the basin or with other basins.

On the other hand, third-order (minor) cycles are eminently correlatable between basins. The authors' critique would be valid if one were to attempt to correlate any singular event with another event in a different part of the world. Instead a series of sea level events interpreted from depositional sequences, with their characteristic systems-tract patterns and constrained between well-established stratigraphic datums, are correlated with similar series of events in other areas that show the same characteristic patterns of systems tracts, number of sequences, and constraints between similar datums. Biostratigraphic data alone may not help correlate all events from one area to the other, but the reproducibility of the number of sequences and the characteristic stacking patterns of sequences and systems tract patterns from one area to the other, helped by local, admittedly gross, stratigraphies, lend considerable confidence to their correlation. Such pattern-matching between widely identifiable datums is a well-established methodology in stratigraphy and is somewhat analogous to matching magnetic reversals or isotopic variations constrained by good datums. We do, however, agree that our curves ought to be rigorously tested in areas other than our reference areas.

We disagree with the critique that the coastal onlap curve is physically meaningless. This curve is an expression of the sedimentary wedges associated with sequences and represents the geometric shape of genetic sedimentary packages after corrections for subsidence and depth components have been applied. Once corrections for tectonic subsidence have been made, in addition to the normal corrections for loading and compaction, [the method is discussed in (4)], the coastal aggradation component is indeed a good measure of the relative sea level rise. Christie-Blick *et al.* point out that downward shifts in onlap are not in response to sea level fall per se, but are an increase in the rate of sea level fall. We agree; however, the shifts of onlap patterns below the preceding shelf-slope break can occur only as a result of a rate of sea level

drop that is faster than the rate of tectonic subsidence at the shelf edge. The magnitude of this fall can be approximated from the thickness of the onlapping coastal sediments deposited seaward of the shelf-slope break, after correction for subsidence and compaction (5). By the same token, similarities in coastal onlap curves that have been corrected for subsidence no longer represent similar subsidence histories in different basins. A good example is provided by the coastal onlap curve from Gippsland Basin, off Australia (6), whose shape was dissimilar to the global curve until it was corrected for subsidence, after which the two curves showed remarkable similarity. In our view, close similarities in the subsidence histories of disparate basins is much more implausible than similarities in sequence and systems tract patterns driven by sea level changes.

We titled our summary article the "chronology of fluctuating sea levels" to underscore the considerable amount of effort that had gone into tying the third-order events as closely as possible to the biostratigraphic schemes. Obviously these correlations need to be rigorously tested, and this will eventually be ascertained or modified.

Finally, we differ with Christie-Blick *et al.* when they regard the sea level curve as a smoothed onlap curve. The latter curve obviously helps in the construction of the former, but the sea level curve can be calculated independently: its basic shape is a function of the ages of the sequence and systems-tract boundaries, and the relative magnitude of sea level falls is constrained at type 1 boundaries where the sea withdraws below the depositional shelf edge. The shape of the coastal onlap curve, on the other hand, is a function of sedimentary wedges and genetic packages that form a characteristic succession of sequences. We readily admit that the absolute magnitude of sea level rises and falls is still an open question most likely to be resolved by a multifaceted approach in the future.

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While Haq *et al.* (1) are to be congratulated for their diligence in presenting a study of relative coastal onlap curves and eustatic curves (2), they do not come to grips with a physical explanation of the data. Explanation requires mechanism. In not proposing mechanisms, Haq *et al.* create the ad hoc concepts of "long-term" and "short-term" eustatic curves which, in fact, are inconsistent with physical mechanisms that most likely explain the data.

The problem is best exemplified by comparing the deep sea isotopic record of the last 700,000 years of glacio-eustasy (3) with the representation of that time interval in the Haq *et al.* eustatic curves. Late Pleistocene tropical to midlatitude planktic oxygen isotope records are primarily an ice volume signal and thereby are proxy for glacio-eustatic sea level fluctuation (4). The well-documented 700,000-year record of oxygen isotope variation contains no less than eight major highstands and six major lowstands of glacio-eustatic sea level. This interval is represented on the Haq *et al.* eustatic curve (left-hand portion of Fig. 1) by a single dip from modern to glacial conditions and a return to interglacial conditions at approximately 1 million years. Clearly, however, there are both conceptual and graphic problems with the Haq *et al.* eustatic curve. The high frequency glacio-eustatic sea level fluctuations that characterize the last 700,000 years (and much of the Tertiary) cannot possibly be portrayed in the one-tenth of an inch available on the time scale. If one wishes to graph glacio-eustasy at this scale, the best one can do is graph the envelope of glacio-eustatic highstands and the envelope of glacio-eustatic lowstands.

Figure 1 compares the Haq *et al.* eustatic curve for the Tertiary with a Matthews eustatic curve constructed on the basis of four simple rules.

1) The tectono-eustatic curve for an ice-free world is taken as a straight line from 250 m above present sea level at 90 million years ago to 60 m above present sea level today. Both of these numbers are in agreement with the data of Haq *et al.*, but the straight line removes bumps and wiggles that are not indicated by data on the sea floor spreading rate (5).

2) The glacio-eustatic highstand envelope is taken from the best available oxygen isotope records for shallow-dwelling planktic foraminifer (6), with -3° per mil projected to represent ice-free world tropical Western Pacific values. Anything heavier than -3° per mil is taken to represent the presence of continental ice volume (7). These values are then converted to the sea level equivalent in meters (8) and hung down from the ice-free tectono-eustatic curve.

3) The glacio-eustatic lowstand envelope is added in a similar fashion.

4) High-frequency oscillation is envisioned between the highstand envelope and the lowstand envelope consistent with the orbital forcing periodicities (20,000, 40,000 and 100,000 years). These frequencies are not depicted in Fig. 1 because of the graphic limitations of the highly compressed timescale. If period oscillations of 100 kilometers were drawn at this timescale with a triple zero pen, the area between the highstand envelope and the lowstand envelope would be black.

In the Haq *et al.* short-term curve there is a tendency for the highstands from 21 million years to the present to mimic time and relative magnitude of the highs on the glacio-eustatic highstand envelope of the Matthews eustatic curves.

Discrepancies between the Haq *et al.* eustatic curves and the Matthews eustatic curves and what should be done about them are noted as follows.

1) Bumps and wiggles in the Haq *et al.* long term curve are replaced by a straight line ice-free tectono-eustatic curve. While there may indeed be bumps and wiggles on the tectono-eustatic curve, they are within the error bars of the underlying tectono-eustatic data (5) and must be justified by solid geophysical argument, not by discussion of relative coastal onlap or geohistory diagrams for different stratigraphic sections of uncertain subsidence history.

2) Throughout the Tertiary, the glacio-eustatic highstand envelope has surpassed amplitude relative to the Haq *et al.* "short-term" curve. This probably reflects the fact that the Haq *et al.* short-term curve is in reality an ill-defined conglomeration of the glacio-eustatic highstand envelope (relative coastal onlap) and the glacio-eustatic lowstand envelope (canyon cutting at continental margins). These two concepts cannot be depicted in a single "short-term" curve.

3) Finally, the mid-Oligocene regression of Haq *et al.* scarcely appears in the deep sea isotopic data. This probably represents local tectonism in one or more "classic" oil exploration areas. It may represent diachronous biostratigraphy. It may represent a rapid tectono-eustatic event, the mechanism of

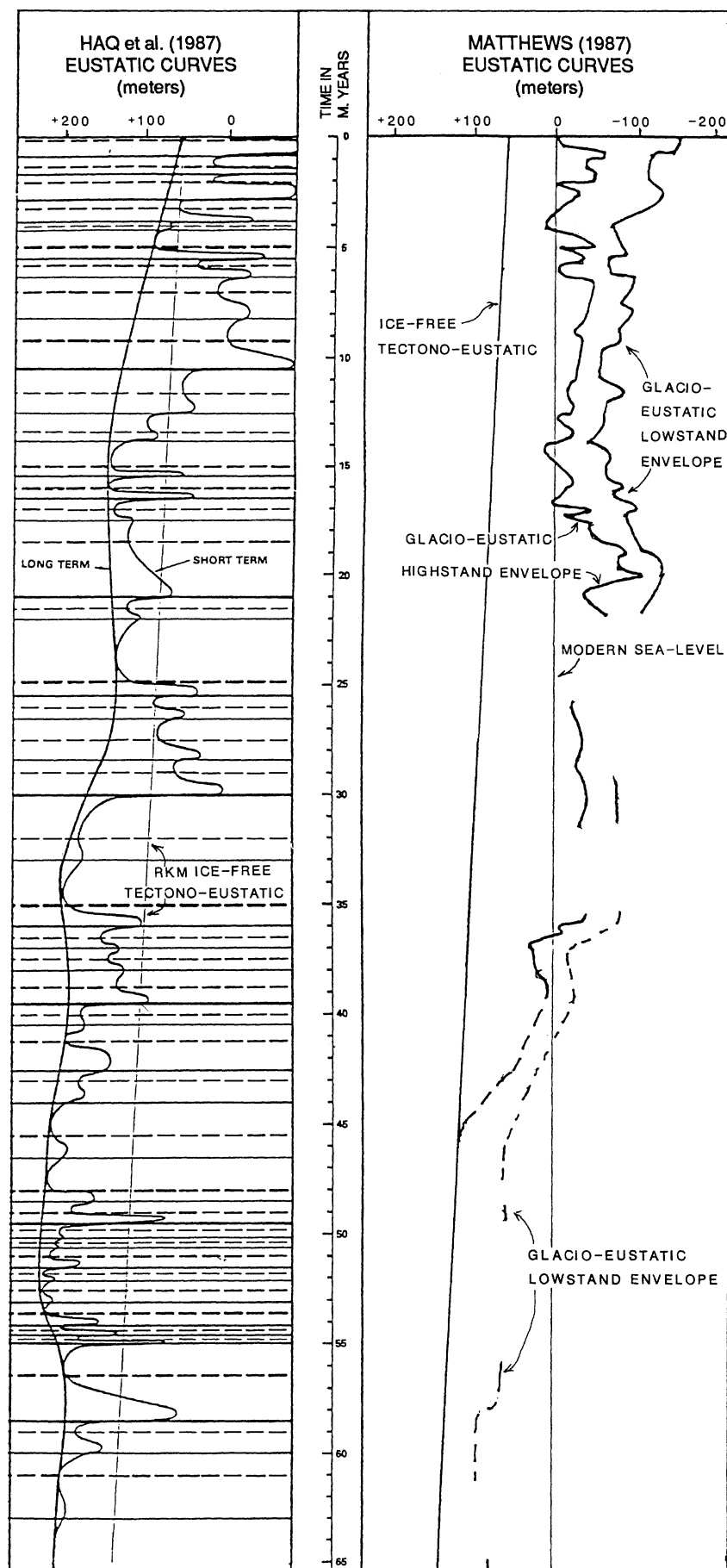


Fig. 1. Comparison of Haq *et al.* (1) eustatic curves with Matthews eustatic curves.

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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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 (low-stand 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 oxy-

gen-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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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