Biogeosciences Discuss., 7, C1596–C1600, 2010
www.biogeosciences-discuss.net/7/C1596/2010/

Biogeosciences
Discussions

Interactive comment on "Diagenetic control of nitrogen isotope ratios in Holocene sapropels and recent sediments from the Eastern Mediterranean Sea" *by* J. Möbius et al.

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Received and published: 29 June 2010

Response to the review of Mike Krom:

We thank Mike Krom for thorough on our manuscript and for stylistic and technical suggestions. We translated the line numbering and page numbering used in the review to the manuscript uploaded to BGD.

We followed essentially all suggestions on style.

Regarding the technical comments:

P 1133, line 17-18 (Line 60-61). Here they quote a paper with Emeis as author which

states that 'This model combines increased productivity during S-1 deposition with enhanced preservation' yet in this text the same author claims there is no enhanced preservation.

We don't understand this comment: The present paper concludes that preservation indeed is the dominant control on sapropel formation and questions the need for enhanced productivity. This is at odds with the Emeis and Weissert (2009) publication (postulating a combination of productivity and preservation acting in concert) and highlights a pretty radical rethinking on the part of Emeis: The Emeis and Weissert (2009) paper was written as an overview text on the basis of our knowledge in 2007. The present paper is based on the dissertation of Moebius that has since then made Emeis "eat his words. . .. . ."

P 1134, line 15 (Line 85). After fixed N. The authors argue this is wrong later.

We don't understand this comment: Our conclusion is that the hypothesis of Sachs and Repeta (1999) is the most likely scenario for sapropels, and we speculate that N2 fixation may have been the norm in the pre-industrial eastern Mediterranean Sea

P 1135, line 4 (Line 102) 'scarce and ambiguous (Mara et al., 2009).' I think the authors can make a much stronger statement about this based on Krom, Emeis and van Cappellen (2010) which shows that there is no significant N2 fixation in the modern EMS.

We cite Krom et al. (2010, in press) now.

P 1139, line 14 (Line 226) Remove sentence starting 'on the other hand. . . The authors have one shallower (but still 1500m trap) and 3 deeper traps. This does not constitute enough data to define a 'depth dependence.'

We removed "depth dependence"

4. Discussion 1st paragraph; This contains within it a major and unproven statement, namely that in pre-industrial times the EMS was N limited. I suggest the following mod-

ifications; Replace Although with It has been shown that Replace may not be with is not Replace that with which Replace dominates with dominate Put a full stop after Mara et al (2009). This replaces this Remove thus Replace most likely has been prevailing with represents a possible Add present before water column If the authors wish to retain the original idea then it needs explicit justification – which I do not think they have.

To not push a disputed point, we followed the reviewer′s suggestions.

p 1144, line 1 (former paragraph 2 page 10 starting line 336):

We agree with the reviewer′s argument and now acknowledge that the analyses only cover a small fraction of the primary produced OM. Little variability in DI and δ15N possibly reflects changes of N sources that are not considered in the reconstruction.

P 1142, line 22 (Line 300) replace shallow with shallower; to this reviewer shallow is 300m not 1500m

The reviewer is right and we changed this

Line 358 why was DI not available for some of these data?

1. for some cores there was no more material available 2. AA-analysis is a very time consuming and expensive technique. . .we had to economize

Table 4: Could you define what is meant by recent marine sediments?

0 to 1 cm. We add this in the table caption

Table 4: How and why were these data estimated?

Data where interpolated from the ODV map in figure 1 because sediment surface samples are lacking for these stations.

Section 4.6: The authors argue that the 'after deposition' TOC flux in sapropel S-1 (450) is the same as the TOC flux in their sediment traps while this reviewer notes that it is almost twice as high. They then point out that the range of TOC% is the 'same'

as that found in S-1 which again is not necessarily true. Traps are 2.5% while S-1 is 0.8-2.3%. So if 250 flux results in 2.5% as the characteristic OM reaching deep traps in oxic water, then does 450 at 0.8-2.3% prove that the OM flux leaving the surface waters was the same in sapropel times? I think not. They then argue that the Ba/Al in S-1 is actually partially removed by diagenesis with organic rich sapropels but not in the burnback zone. This is an interesting idea which merits further work. We have some data albeit very close to the Nile which shows an increase in Ba/Al before the onset of S-1 as defined by anoxia which we interpreted as showing that productivity increased before anoxia increased i.e. both existed during S-1. Unfortunately our fellow authors removed the benthic foram data from the submitted manuscript to protect an M.Sc. student still writing up. When I spoke to Gerte de Lange he also suggested that he had evidence of increase in productivity before anoxia but again did not provide a quote. I suggest the authors soften their statement, which in any case seems to contradict Emeis and Weissert, 2009.

We beg to differ from the reviewer in this : The monitored TOC flux of 1300 mg C m-2 y-1 at 1500 m water depth in the single shallower trap is almost three times higher than 450 mg C m-2 y-1 that accumulated in the S1 at 1300 m water depth (core 569). Even at 2600 m water depth, which is 800 m deeper than the oxycline during S1 and 1300 m deeper than core 569, the flux is between 200 and 300 mg C m-2 y-1. In our opinion this is enough sinking distance/time spent in oxic water to remineralize some organic C. All evidence suggests that TOC flux decreases exponentially with depth oxic water columns (e.g., Suess, E. (1980), Organic carbon flux in the oceans: Relation to surface productivity and oxygen utilization, Nature, 288, 260-263).

Our results in table 4 show TOC means of 0.8 to 2.3% in the S1. This is in the lower range of other studies; Moodley et al. (2005) and Thomson et al. (1999) determined TOC contents up to 4% and 3%, respectively, in Levantine Sea cores.

We did not wish to prove that the flux was the same, but we noticed (and mentioned in our text) that the present-day flux it is of the same order of magnitude as the flux at S1 time. We added some numbers (water depths and TOC of Moodley et al. (2005)) to make our consideration more comprehensible.

Regarding the Ba/Al: Our statement is speculative as there are many “maybes”, and specifically mark it as a speculation (“we consider it possible”). Because we just wanted to point out another possible view on this matter, we decided not to discuss all pros and cons of Ba/Al ratios and their relationship to enhanced productivity.

Interactive comment on Biogeosciences Discuss., 7, 1131, 2010.