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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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This is on the whole a well written manuscript and should be published. It shows clearly that diagenesis needs to be taken into account in interpreting 15N signals in the EMS which is important. I have made a number of minor comments, sometimes making the English a bit clearer. However in two places they make what I feel are unjustified speculation which have rather profound implications for the behavoir of the system. In both cases, what they suggest could be correct but is not the only explanation. I have explained where these are and suggested possible ways of rewriting the text.

Detailed comments: 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.

Line 63 remove uncharacteristically

Line 76 remove originally

Line 85. after fixed N. The authors argue this is wrong later

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.

Line 126 I suggest this sentence should be modified by changing would most to could and proxy in environments to proxy for the original environment

Line 135 remove indeed

Line 138 add modern before EMS

Line 141 add signal after N

Line 151 modify to say during a series of three to seven month periods from . . ..

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.'

Line 227 change whereas to while

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 modifications; 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.

Line 299 traps Line 296 should read the shallower and the deeper traps

Line 300 replace shallow with shallower; to this reviewer shallow is 300m not 1500m

Paragraph 2 page 10 starting line 336: This is major speculation based on very little evidence. Once almost all of the OM has been oxidised then the residual d15N and DI signal is likely to be very similar regardless of the nature of the original OM isotopic ratio or amount. Indeed there is some very preliminary differences in data which could possibly reflect differences in sources i.e. some of the sample groupings in Figure 4 might suggest different processes. This requires much more detailed data to define which I would encourage the group to develop. In the meanwhile I suggest that that this paragraph is made much less definite and must include some sentence which points out that once almost all the OM has been oxidised it is very difficult to define the original d15N of the OM produced in the surface waters.

Line 355 add Table 4

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

Line 379 remove even

Line 414 add I to denitrification

Table 4: Could you define what is meant by recent marine sediments possibly add (0-x cms)

Table 4: How and why were these data estimated

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 Emies and Weissert, 2009.

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