Biogeosciences Discuss., 7, C278–C282, 2010
www.biogeosciences-discuss.net/7/C278/2010/

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.

F. Jorissen (Referee)

frans.jorissen@univ-angers.fr

Received and published: 22 March 2010

This paper presents an excellent contribution to the long-standing discussion on Mediterranean sapropels. It questions the hypothesis that Mediterranean sapropels are at least partially due to increased primary and export production. The authors present convincing arguments, that the δ15N, but also the Ba/Al ratio and Corg content, have a strong diagenetic overprint. They argue that the particular signature of these "paleoproductivity proxies" in sapropel levels is the result of a better Corg preservation in the dysoxic water column and on the anoxic sea floor, and not of increased export production, as is generally suggested. If true, this would mean that enhanced primary

production is not a necessary prerequisite for sapropel formation.

Some comments:

p. 1134: the authors compare modern Eastern Med δ15N data with S1 ones, and conclude that both systems have strikingly low values in common. However, the recent EM values are still considerably higher than S1 values.

p. 1135, lines 2-17: The idea that today's low δ15N values are due to incorporation of 15N-depleted atmospheric NOx is largely based on a single paper (Mara et al., 2009). I wonder whether the authors really need this for their interpretation of the sapropel signals?

p. 1135, line 22-24: pristine δ15N values have been obtained in diatom frustules, foram shells and biomarkers. What are the results of these analyses?

p. 1135, line 25 – p. 1136, line 3. The authors argue that "reoxygenation would most seriously compromise the reliability of as a proxy in environments with low sedimentation rates". I admit that in many Mediterranean areas sedimentation rates are very low, but the ultimate conclusion is that the reliability of δ15N has to be questioned everywhere! And, rather surprisingly, in view of the predominance of records in settings with well oxygenated sea floors, at present, the sapropel records are difficult to interpret, because the original signal is exceptionally well perfectly!

p. 1136, lines 5-7. The authors suggest that they can sort things out by looking at nitrogen isotopes, Ba/Al ratios, total nitrogen, and bulk organic carbon at the same time. Later in the paper it turns out that this is not true, since they argue that all these parameters are strongly affected by the anoxic bottom waters!

p. 1139, sediment traps. The info on these trap data is really very limited. It would be interesting to know whether there is a seasonal variability in δ15N! There is a difference of 1.4‰ between the extreme average values. Do the authors have any idea why?

p. 1141, paragraph 4.1. The authors are faced with the apparent contradiction of a significant decrease in the Corg flux between their traps at 1500 and 2500 m depth, which is not resulting in a significant change in the Corg quality (as expressed by DI). They explain this as "disaggregation and disintegration of sinking material without compound-specific fractionation. It is difficult to understand why the loss of 75% would not lead to a change in δ15N and DI, whereas further, quantitatively less important, losses at the sediment–water surface would cause a large shift in these proxies. Perhaps the authors can give a quantitative estimate of the Corg losses at the sediment-water interface (compared to the losses during the transit of the water column).

p.1143, lines 7-10. I fail to understand this argument. Why would the fact that recent fresh Corg has a similar isotopic signature as S1 Corg mean that the δ15N of primary produced Corg was the same in both periods? This is only logical when we admit that absolutely no alteration has occurred in sapropel times. But this conclusion comes only later in the paper! Since DI is higher in S1 than in recent sediment trap material, Corg would be (slightly) better preserved during S1 times. This would then suggest slightly higher δ15N values of recent PP Corg compared to sapropel times.

p. 1143: line 23: "the protosapropel that developed under suboxic conditions". Several papers (e.g. Passier et al., 1996) have suggested that the protosapropel has been deposited under fully oxic conditions, and that the grey color is due to a downward migration of sulphides, leading to the formation of pyrite. In most sapropel records, the onset of anoxic conditions is almost instantaneous (at most 1-2 centuries). The lower δ15N of the 2 protosapropel samples in fig. 4 could be due to a shortening of the residence time in oxic conditions (at the sea floor) due to the onset of anoxic conditions AFTER the deposit of these sediments. However, in order to know whether such a hypothesis can be envisaged, it is necessary to know at what exact level (with respect to the sapropel basis) these protosapropel samples were taken!

p. 1144, line 56: The authors should better explain how they arrive at +2‰

p. 1145, line 27 – p. 46, line 2. The authors write that the higher δ15N values in three

S1 records are due to the fact that the northern sub-basins have not been fully anoxic. They cite Kuhnt et al (2007) to justify this statement (Kuhnt et al., 2008, should not be cited, since they do not present faunal data). Several shallower Aegean records (e.g. Abu-Zied et al., 2008) confirm the presence of benthic foraminiferal faunas during S1. However, this is not the case for the Adriatic Sea, where no S1 records below 800 m depth contain benthics foraminifera. Therefore, such a scenario can not explain the maximum δ15N values found in S1 of the Adriatic Sea.

p. 1145: comparison S5, S6 and S1. Line 24: during S5, "PN was uncharacteristically depleted". I don't understand what the authors mean. In the lines before they suggest that primary produced PN had the same composition in all sapropels, which is logical. So I guess this statement pertains to sediment PN?! But I do not see why it is uncharacteristic. Following the reasoning of the authors, the negative δ15N values could simply be indicative of a perfect preservation, since we are not outside the range of -2 to +1‰ for PN resulting from N2-fixation.

Lines 25-28: the authors suggest that the differences between the sapropels must be due to "variable degrees of anoxia". First, anoxia means zero oxygen, there is no such a thing as "variable anoxia". Next, since the sea floor was anoxic for all three sapropels, the difference must be in the water column. Unfortunately, earlier in the paper, the authors argue that the water column transit has no impact on δ15N, which is largely determined by diagenetic processes at the sea floor. This is an apparent contradiction (the arguments for both opinions are valid), which should be mentioned. This gives an extra argument to the alternative mechanism, proposed in paragraph 4.5!

Some details:

p. 1134, line 3: "hemipelagic carbonates" → rather say "hemipelagic carbonate-rich muds".

Line 11 "that are similar to"" → ',similar to"

Line 22: please write “aka” in full!

p. 1135, line 26: “enrichment in δ15N” $\rightarrow$ you mean 15N enrichment ?? Or rather “relative 15N enrichment” (since the 14N disappears?!)

p. 1141, lines 14-19. This first sentence is grammatically completely mixed up.

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