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Comment

Interactive comment on “Productivity patterns and N-fixation associated with Pliocene-Holocene sapropels: paleoceanographic and paleoecological significance” by D. Gallego-Torres et al.

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We present the reply to the comments by anonymous reviewer:

The second reviewer makes three major and very clear remarks: 1. The paper needs to be shortened by limiting the discussion to new points of view and briefly citing the repetitive or not new discussion: We fully agree. Given that this is a multidisciplinary magazine we expanded the discussion so that a wide variety of scientist from different backgrounds could appreciate the manuscript. However, we admit it is possible to pro-

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vide a more concise second version of the manuscript and we will work with that goal in mind. 2. Figures: It was challenging to compress all the information into the minimum number of figures, and we expected the result to be more convincing. We also agree that more figures with larger scales will facilitate the understanding of the data presented, so we will produce the new figures. 3. Accumulation rates vs. Age models: As mentioned on the reply to Dr. Capozzi's comments, the topic of sedimentation rates calculation has been previously discussed and recently addressed on Gallego-Torres et al, 2010. We are facing a case of circular argumentation with (presently) no unambiguous answer. Although in Gallego-Torres et al (2010) the "isochronous sapropel" model was applied, we chose constant linear sedimentation rate within each i-cycle for the following reason. If we calculate LSR within each sapropel layer (based on isochronous tops and bottoms) we obtain changes in SR's from "background" sediment to sapropel sediments of more than 30% lower or more than 50% larger, which Nijenhuis and De Lange (2000) proved to be inconsistent. Particularly, these authors claim that these increases in SR, and thus on MAR, will produce unacceptable productivity rates, whereas a decrease of more than 30% in SR was proven to be inconsistent by Wehausen and Brumsack (1998). Thus, the "less incorrect" assumption for calculation of LSR's and MAR's needs an approximately constant LSR throughout each i-cycle (as also stated by Nijenhuis and De Lange). In any case, we produced a table comparing MAR calculated based on isochronous sapropels (change in LSR from "background" to sapropel sediments) and considering constant LSR. The main observation from this table is that the same conclusions can be obtained using either of the two models, only the differences between different sapropels are enlarged or reduced (depending on the shift of LSR within the sapropel).

Other specific comments were pointed out: a. We change "smaller" for "lower". b. We consider the sentence to be ambiguous; the combination of proxies ($\delta^{15}\text{N}$ and C:N) is the clue to infer the original signal of N-isotopes. In any case, more recent studies cited in the manuscript (Higgins et al, 2010) validate this assumption. c. The issue on Ba remobilization was also addressed in Dr. Capozzi's comments. As mentioned

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on that reply, we believe that it was actually a decrease in productivity maintaining anoxic conditions (thus preservation of organic matter), with no diagenetic imprint. d. Comments on Figs. 9 and 7 will also be addressed; use MAR instead of % for Fig. 9 and change caption on Fig. 6.

We would like to thank both reviewers for their interesting and constructive comments.

Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/7/C2875/2010/bgd-7-C2875-2010-supplement.zip>

Interactive comment on Biogeosciences Discuss., 7, 4463, 2010.

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