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Biogeosciences Discussions

Interactive comment on "Increase in water column denitrification during the deglaciation controlled by oxygen demand in the eastern equatorial Pacific" *by* P. Martinez and R. S. Robinson

Anonymous Referee #1

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Philippe Martinez and Rebecca Robinson submitted a well-written, concise manuscript, describing the timing and potential causes explaining increased denitrification in the eastern tropical North Pacific across the last glacial termination. They provide an original approach to a long-standing debate as to whether increased export production and/or changes in the oxygenation of subsurface waters controlled denitrification in the region. The manuscript is well balanced and follows a logical progression. However, I feel their argumentation is weakened by the fact they only provide a limited array of observations, while a truly multi-proxy approach would have been better adapted to support their hypothesis. This being said, this manuscript still has the potential to be considered for publication provided the authors address the comments listed below.

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general comments:

- Corg (and related TN) is the only proxy considered to reconstruct changes in export production through time. As the authors certainly reckon, the sedimentary Corg mass accumulation rate (MAR) is highly dependent on oxygen exposure time and the oxygen concentration at the water-sediment interface as well as within the sediment (e.g. Hedges et al., 99). I agree that there seems to be a "good agreement between all cores" shown in Fig. 2 but this does not necessarily mean that the main and only parameter controlling Corg accumulation is export from the surface as inferred by the authors in a very vague unreferenced statement (p. 5151, l. 1-2). In particular it is striking that the Corg MAR at site ODP 1242 (where "biological productivity is relatively low compared to other continental margin settings" - p. 5149, l. 22-23) is at least five times higher (if I'm correct – there is a typo in the label) than at site Me05-24JC (Kienast et al., 06) and almost 10 times higher than at ODP 1240 (Pichevin et al., 09). Can this exclusively be explained by changes if export flux of organic carbon? The study would benefit a lot by considering additional proxies for export production such as opal, biogenic barium (where applicable) and/or biomarkers as well as records of redox-sensitive trace metals. I do not necessarily mean that the authors should provide more measurements from their own archive but they should significantly expand the argumentation to other available records from the literature as well as to discuss the potential for lower oxygenation to modulate to sedimentary Corg distribution and its related impact on the discussion.

- the entire argumentation is based on the assumption that "nitrates are (AND WERE) completely consumed annually" (p. 5149, l. 24) at the site location. While this does not seem unreasonable I would urge the authors to provide at least a reference to support their assertion or even better a map of annual mean nitrate concentration in Fig. 1.

specific comments:

- p. 5150, l. 23-24 One should keep in mind that "conventional mass accumulation

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rates calculations" are highly dependent on age model constraints and are nothing more "than a little better than a guess (Catubig et al., 98). Please clearly state that this approach can only be used as a first approximation especially in a sediment record where the sedimentation rate varies significantly over time. Moreover, the "double peaked maxium during the deglactiation" while undoubtedly present in the ODP 1242 record does not show significantly higher values when compared to the LGM. - p.5152, l. 22 " The deglacial peaks in export production in the EEP are COINCIDENT with the peaks in denitrification. . ." The "equatorial organic export – oxygen demand" on Fig. 3 clearly LEADS (and is certainly not coincident with) changes in denitrification inferred from bulk d15N records.

technical comments p.5147 - l. 7-10. Please add reference p.5148 – l. 11. Galbraith et al., 2006 p. 5150 – l. 2. export instead of exportation p. 5151 – l. 15. Pichevin et al., 09 Nature p.5152 – l.9-10. This statement is rather unsubstantiated. Are there other arguments to shift the age model so that it fits with the other records? p.5152 – l. 22 in the EEP instead of the in EEP Fig. 2 - TOC burial rate ODP 1242 (g m-2 an-1)?

Interactive comment on Biogeosciences Discuss., 6, 5145, 2009.