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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 #2

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I have reviewed the manuscript: "Increase in water column denitrification during the deglaciation controlled by oxygen demand in the eastern equatorial Pacific" by P. Martinez and R. S. Robinson submitted for publication in Biogeosciences. In this paper, the authors present new sedimentary data from ODP Site 1242 located in the eastern Equatorial Pacific off Costa Rica. The new data presented is the C and N composition of sediments (wt% and MAR) and bulk sedimentary d15N. The authors use their data and data published in the literature to claim that the apparently synchronous increase in denitrification (high d15N in several sedimentary archives) in the eastern Pacific was caused by enhanced oxygen demand (higher productivity) in the eastern equatorial Pacific.

I think this paper presents interesting new data as to justify its publication in Biogeosciences. I also applaud the author's search for a common driver that could explain the deglacial d15N observations along the eastern margin of the Pacific. However, as I will argue in my general comment, the authors arrive to a conclusion that is not supported by the data presented in the manuscript. On this basis, I will recommend that the authors provide a significantly revised version with a more in depth discussion of the points discussed below.

General Comment:

Overall, my main concern with this manuscript is that its treatment of the new and existing data is too weak. The authors arrive to a conclusion that -in the data as presented- is ambiguous to say the least; i.e. that the changes observed in denitrification records along the eastern Pacific are all related to enhanced oxygen demand in the equatorial region during the deglaciation. I think that the authors overlooked the inherent heterogeneity of the records and chose one among many explanations. In order to consider this manuscript suitable for publication, I would require that the authors provide an in depth discussion of the following observations:

1. If the equatorial oxygen demand was the only forcing behind denitrification changes in the oxygen minimum zones of the eastern North and South Pacific, one would expect very similar changes in the d15N records shown in the manuscript (off Chile, off Costa Rica and off Mexico). Just consider what is shown in Figure 3. The transition from low glacial values to the deglacial maximum in core ME0005A-11PC (off Southern Mexico) starts at 18ka BP. The core off Chile starts at 17.5ka BP and the ODP Site 1242 (off Costa Rica) very slowly at (arguably) 20ka BP. The new data (off Costa Rica) doesn't show a deglacial maximum as the other sites (it reaches its maximum as a double peak centered at 10ka BP). To me, the 2.5 kyr difference between the onsets of denitrification is rather large as to consider them synchronous (even with the age model limitations). Moreover, The ODP 1242 Site d15N does not show a reduction towards the Holocene as the other records. The deglacial rate of change in the d15N

records is also very different. While the Chile core shows a very dramatic increase (1kyr), the Mexico site is a bit slower (2kyr) and the Costa Rica site is very gradual (5ka, if one considers the local maximum at 15ka BP to be the deglacial maximum). How the authors explain these differences if they consider the oxygen demand as the single driving mechanism? I need a better argument than just referring the records as being 'in good agreement'.

2. In the same line, when one compares the different records of organic C or N export from the surface to the ocean floor (MAR, %wt, 230Th fluxes), there seems to be more heterogeneity between them to accept the blank statement that "a good agreement between all the cores is observed" (p.5145, line 10). For example, the ODP 1242 Site (off Costa Rica) shows initial TOC and TN increase at 25ka BP, the same can be argued in the lower resolution 230Th-normalized C fluxes in cores ME0005-24JC and TN in ODP Site 1240. Paradoxically, the d15N records presented in Figure 3 show decreasing! trends (lower denitrification) during this time. Again, I want the authors to discuss how these differences could arise if oxygen demand in the equatorial region is the main driving mechanism.

3. To wrap up this comment and touching what the authors discuss in the last paragraph of the paper (p.5153, lines 7-19), I would want the authors to explain in more detail why denitrification levels remained high during the Holocene in the site off Chile and Costa Rica. In my view, there are different mechanisms operating in the region not only during the Holocene, but also different mechanisms behind the deglacial onset of denitrification in the different areas. I therefore, highly encourage the authors to provide a revised version of the paper where this differences are brought to the surface instead of trying to overlook them. The all too brief discussion about these differences in the last paragraph of the manuscript is no enough. I would like to remind them that they are presenting only one additional record that seems to open more questions than actually it resolves.

Specific comments:

There a couple of syntax errors:

p.5152, l.20 should read "by the influence of variable local hydrologic conditions". p.5152, l.22 should read "...peaks in export production in the EEP..."

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