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Biogeosciences Discussions

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Interactive Comment

Interactive comment on "North Pacific-wide spreading of isotopically heavy nitrogen from intensifieddenitrification during the Bølling/Allerød and post-youngerdryas periods: evidence from the Western Pacific" *by* S. J. Kao et al.

Anonymous Referee #1

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Kao and coauthors present a new record of bulk sedimentary d15N from an intermediate-depth core in the Okinawa Trough, on the western margin of the northern subtropical Pacific gyre. The record shows little variability overall, but displays two clear millennial-timescale excursions during the latter half of the deglaciation that appear similar to many other sites in the North Pacific. They follow the existing paradigm, interpreting the deglacial excursions as reflecting changes in denitrification rates in the eastern tropical North Pacific shadow zone. These new results contrast with previous results from the nearby South China Sea, which did not show similar variability through the deglaciation. The new data are a useful addition to the North Pacific d15N literature, and could provide an important constraint on the cross-basin gradient. However, the record does not stand very sturdily on its own, and would greatly benefit from ancillary measurements, as I suggest below. The discussion makes a general summary of the existing literature, but does not arrive at any firm or novel conclusions. In addition, there are some poorly-supported reasonings that need to be addressed. As such, I would recommend that the authors do a considerable amount of additional work, including gathering more data and making a more thorough analysis, if the article is to be published.

Regarding the data: The d15N record appears to be of good quality, but contains a relatively small number of measurements from only a single core. Thus the interpretation is not very robust, and would greatly benefit from additional measurements. I would suggest two ways in which the intrepretation could be strengthened. First, make additional measurements in the nearby MD12403 core, to verify the regionality of the signal and its magniitude. Second, measure d15N-NO3 in the water column. I believe d15N-NO3 measurements techniques have improved since the Liu (1996) data were published, and it would be very interesting to explore the suggestion that the South China Sea nitrogen supply is isotopically distinct from the OT nitrogen supply with modern data. I recognize that this would be contingent on the authors being able to obtain appropriate water samples, and that this may not be possible; however, given the potential these measurements would have to strengthen the interpretation of the sediment data, I would strongly encourage them to pursue it.

Regarding the interpretation: Contrary to the authors' contention, the d15N record does not need local N2 fixation in order to explain it. The average sedimentary value is 4.8 +/- 0.2, with two excursions to higher d15N during the deglaciation. The value for NPIW is not an appropriate indicator of sub-euphotic zone nitrate; the data of Liu et al.

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(1996) show large gradients in d15N-NO3 through the upper 400m of the water column, with values <2 and >5 permil. The sedimentary values are, generally, higher than the shallow d15N-NO3, which is what would be expected to supply the surface nutrient pool. What's more, diagenetic alteration of sinking and sedimenting nitrogen has been shown to alter d15N in many environments, preventing the use of the absolute d15N value as a precise measure of organic matter d15N. Although there is ultimately a large contribution of nitrogen fixation to the d15N here - all nitrogen was fixed at some point - the evidence here does not require significant local N2 fixation.

Although a deglacial denitrification peak has been widely proposed in the literature, I think it is still worth acknowledging that the deglacial d15N signal could arise from other effects - not necessarily from a peak in water column denitrification rates. Kao et al. state that it's 'highly probable' that these are due to intensified denitrification in the ETNP - based on what? The overall temporal coincidence? Many things were happening in the North Pacific through the deglaciation, and a number of things occurred within 1ky of the start of the Bolling, including large changes in temperature, ocean circulation, and sea level rise.

I also think it is potentially significant that there is no glacial-interglacial change at this site - the Holocene and LGM d15N are identical, within measurement error. This agrees with the primary conclusions of Kienast (2000), and is markedly different from Western American margin records. I would like to see this more thoroughly commented upon. The theoretical framework of Deutsch (2004) should be very useful in this respect, and comparisons to South Pacific d15N records might also prove illuminating. The placing of this record within the global context is tricky, but important.

I find the references to the earlier works by Kao somewhat confused. Given that they are presented as a central part of the discussion, I would like to see the reasoning and referencing clarified. It seems to me that, in combination with their earlier works, these authors are on the verge of assembling a useful picture of historical variability of physical circulation and biogeochemical cycling in the vicinity of the Okinawa trough.

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However, it seems to me that there are - at present - an unacceptable number of inconsistencies between their various observations, that must be reconciled if this is to prove a useful contribution: in reading over the earlier work, I was worried to encounter what appear to be unexplained inconsistencies between their interpretations. For example, in one paper Corg is taken as a paleoproductivity proxy, while in another, Ba/Al is the chosen paleoproductivity proxy - however, the two quantities show an opposite sense of glacial-interglacial change!

I am not familiar with the use of TS as a proxy for bottom water oxygenation. I would appreciate some references that explain how it is related to bottom water oxygen concentrations in recent marine sediments.

Finally, there are some parts of the discussion that seem out-of-place, or do not serve a clear purpose. The discussion of TS and the 3-D model (paragraph starting "However, sea-level change") seems somewhat aimless. Is this intended to provide an argument against local denitrification? Why is this important? The d13C discussion which follows is also confused and unconvincing. Last, I don't understand why the precession cycle at 30N is shown. If this is to be included, there should be a discussion of a mechanism linking it to the d15N.

Interactive comment on Biogeosciences Discuss., 5, 1017, 2008.

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