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Biogeosciences Discussions

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 #4

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General comments:

In their paper Kao et al. present an interesting sediment record of glacial to Holocene changes in nitrogen isotopic composition from the Okinawa Trough in the West Pacific. This record is particularly interesting because to date not many records exist from sites distant from the regions of water column denitrification in the major oxygen minimum zones. In order to understand the effect of glacial-interglacial changes in the strength of water column denitrification on the global nitrogen budget and ultimately on climate, records from outside those regions are crucial. Therefore, this paper is in general well-suited for publication in Biogeosciences and the well-dated δ^{15}N record presented here will be an important contribution to the growing global data set of δ^{15}N records.

In my opinion the structure of the paper needs to be improved and the discussion needs to be expanded, however. The way the authors interpret their data as reflecting water column denitrification in the Eastern Tropical North Pacific (ETNP) is one possible way to explain the record, but as nitrogen isotope records are inherently complicated, the other potential influences need to be discussed in more detail. Finally, the authors should work on improving language and grammar in their manuscript.

Specific comments:

In the discussion of the record with regard to water column denitrification in the ETNP it should be made clearer how the authors think their data will help to resolve open questions. The sentence on p. 1019, l. 16-17 ('Such synchronicity between the western and eastern North Pacific allows us to infer enhanced production ...') is unclear; it is already known from the East Pacific records that water column denitrification varied. Do the authors mean that the fact that the same signal is seen in the West Pacific (and its magnitude) allows reconstructing the magnitude of water column denitrification (in a modeling approach)?

Furthermore, the authors should state whether they interpret the δ^{15}N variations at their site as a signal that is directly imported by advection of heavy nitrate, or whether they think their data reflects changes in the global mean isotopic composition of nitrate (see Deutsch et al. 2004). Both are plausible and should be discussed. To this end, a more detailed discussion of North Pacific water masses and currents at the relevant water depths would be helpful (e.g., what is the connection between Eastern and Western North Pacific, how are KIW and NPIW linked, ...). Concerning mean

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ocean nitrate δ^{15}N, it is important that only water column denitrification has a strong fractionation effect on nitrate isotopic composition, whereas sedimentary denitrification leaves little signal (the authors should always state which kind of denitrification they refer to). The relative extent to which global denitrification occurs in the water column is therefore an important influence on global mean nitrate δ^{15}N (Brandes and Devol 2002). For example, a deglacial peak in mean ocean nitrate δ^{15}N would be expected if water column denitrification increased earlier than sedimentary denitrification during deglaciation (Deutsch et al. 2004).

Other influences on sedimentary δ^{15}N are only partly discussed in the current version of the manuscript. This discussion should be expanded and structured more systematically, for example by discussing each factor separately and stating the arguments for or against a significant influence.

1) Terrigenous N: It would be helpful to plot the δ^{13}C data discussed in the paper and to investigate whether there are correlations with δ^{15}N. As terrigenous input is a very local effect, the fact that this influence has been ruled out for the South China Sea (Kienast 2000) does not imply that it is not relevant at the study site. Neither is the small magnitude of δ^{13}C change, since the magnitude of change in δ^{15}N is also small. Do the authors have C/N ratios for their data?

2) Local water column denitrification: The authors use previously published total sulfur (%TS) data to infer more stagnant conditions at their site during glacial times. Hence, during these times local water column denitrification might have occurred, as the authors discuss. They also point out that the timing of changes in %TS and δ^{15}N make a large influence on δ^{15}N unlikely, but it is also important to discuss how this signal would have been transported (e.g., upwelling) and how transport could have changed. Are there any other indications for changes in ventilation, e.g. lamination of the sediment in glacial intervals?

3) N_2 fixation: As the authors point out, the low δ^{15}N values they observe in their core

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suggests that N_2 fixation is influencing the signal. This N_2 fixation could be local, regional (Kuroshio area), or even widespread in the North Pacific (see Deutsch et al. 2007). Changes in N_2 fixation could be due to changes in nutrient composition (feedback to denitrification; Redfield et al. 1963, Haug et al. 1998, Galbraith et al. 2004, Meckler et al. 2007) or to changes in dust (iron) input, or both. Since the Asian monsoon system changed strongly during the deglacial events discussed in this paper, it is likely that winds blowing dust off the continent also changed considerably.

4) Diagenetic alteration: It is typically observed that δ^{15}N is altered during diagenesis in well ventilated, open ocean settings. This may not be a problem at this site, but the authors should explicitly state why. What is the sedimentation rate here?

5) Relative nutrient consumption: The authors should point out that they assume nutrient consumption has always been complete (and why).

Further specific comments:

p. 1019, l. 6-11: The records of both Kienast (2000) and Higginson et al. (2003) show variations on δ^{15}N of up around 1 permil during the deglaciation, which is not so different from the magnitude seen in this study. The authors should examine possible covariations. For glacial data up to 16 ka, those records do not agree (Kienast et al. 2005) and should therefore be interpreted with caution.

p. 1021: It is confusing to start the results section with a discussion of the local setting.

p. 1023, l. 5-6: The relationship with precession is not discussed further and does not seem important for the arguments of the paper; I would suggest omitting this comparison.

p. 1024, l. 19-21: I do not see a clear signal in δ^{15}N in either of the discussed cores during Heinrich events I and II (the JPC56 record is not long enough, site 893 does not show a change, and the signal in MD01-2404 is very weak).

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p. 1024, l. 21-22: A similar mid-Holocene decrease in δ^{15}N has also been observed in Cariaco Basin (Altabet 2007, Meckler et al. 2007), and was attributed to increasing N_2 fixation in the latter study.

Minor comments:

Title: The title is rather long; I would suggest changing 'during the Bolling/Allerod and post-Younger Dryas period' to 'during deglaciation'.

p. 1018, l. 2-5: The first sentence in the abstract does not make sense; maybe part of it got lost?

p. 1019, l. 2: N isotopic composition of organic matter buried in sediments ...

p. 1025, l. 8-10: This sentence could be misunderstood as '... the scale of OMZ intensification ... is likely North Pacific-wide ...', which is probably not what the authors want to say.

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