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Climate of the Past Discussions

Interactive comment on "Influence of the Atlantic thermohaline circulation on neodymium isotopic composition at the Last Glacial Maximum – a modelling sensitivity study" *by* T. Arsouze et al.

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Review of Influence of the Atlantic thermohaline circulation on neodymium isotopic composition at the Last Glacial Maximum a modeling sensitivity study by Arsouze et al.

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This paper considers possible changes the ratio of Nd143 and Nd144, as represented by EpsNd, to plausible changes in ocean circulation and source values during the Last Glacial Maximum (LGM). The justification for such a study is the exciting possibility that

EpsNd might be used to reconstruct changes in ocean circulation on millennial time scales in the past using observations derived from sediment cores. As the authors point out, several tentative studies have already attempted to do just this. This is the first modeling paper to look in detail at the possible changes to EpsNd in the ocean during the glacial period and, as such should be published. I find several issues that need to be addressed or at least noted and also that some restructuring of the paper may be necessary to make it more accessible to the reader.

General Comments

1. I am concerned that the authors exaggerate somewhat just how effectively their model simulates the modern Eps Nd distribution. Neither this paper nor the last paper of this group gives a rigorous, quantitative comparison of all of the available data with their model. This is of special concern because the limited comparison available in the paper is weak. My understanding of the boundary exchange model (BE) is that it has the strongest influence in the upper water column at the margins. It is of special concern then that the model appears to fail in the surface ocean, where one would expect the model to have most direct influence (P.318, line 13 to 16). In fact the comparison of data and model in Fig. 4 seems rather weak.

It is an obvious concern to worry about simulations looking at change when the control state compares weakly with data. Even the GCM used seems to poorly recreate the observed MOC strength (the 11 Sv generated by the model is very weak). I suggest that the authors do more to reassure the reader that the model does reasonably well at representing the data. At least crossplots of data and model and the plotting of additional data to that shown would be useful. Data and model should be plotted on single water column profiles to check the vertical variation in Eps Nd more precisely.

2. I felt that the structure of the results and discussion were hard to follow. At least in the presentation of results it would make sense to consider each of the simulations separately under separate subsections before synthesizing the results in the discussion

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section. More could be included in tables instead of detailed listings in the text that are hard to follow. The results and discussion both refer to the absolute values in the LGM simulations as well as the differences between the simulations with the modern control. However, only the difference plots are shown. Please show both the difference plots and the absolute values.

3. The figures need to be replotted with attention paid to the specific format of this journal. The figures need larger font, labels on all axes and data points need to be larger. At least in the pdf I printed off the figures ended up quite small. The pdf for this journal is designed to fit the monitor screen so it is landscape, rather than portrait. This means that figures designed as portrait end up very small. Ideally split the series of four transects into 2 plots of 2 transects over 2 pages to make best use of the journal format.

4. I question the authors implicit assertion that the average basin values are important. They do not explicitly argue why they are important. Indeed it would seem obvious that changing the Nd input is the only way to change the average basin value while changing the circulation alters the Nd distribution. A lot of emphasis is placed on the basin average while this is less important for the result of Pitotrowski et al. than the changes in Nd distribution.

Specific Comments

5. P.311, line 11, cite a time range for the LGM. It was likely not one moment in time. 25-18 ka BP might be OK.

6. P.312, line 10, Yu et al was not associated with this result. In fact Yu et al concluded that there was no change in Pa/Th and therefore overturning strength at the LGM. Marchal et al. 2000 showed that, in fact the Yu et al compilation of data could not show that there was indeed no change in circulation at the LGM. The McManus et al. result focused on changes during Heinrich event 1 and found some evidence for reduced change at the LGM. Other Pa/Th results from Gherardi et al and Hall et al have reached

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broadly similar conclusions. See Siddall et al 2007 for a recent discussion and contact Jeanne Gheradhi for some new results which are in press with Paleoceanography.

7. P.313, line 2, Siddall et al 2005 did not consider LGM simulations. Siddall et al 2007 did consider the effect of MOC slowdowns relative to modern but with the aim of looking at changes during H1 and not the LGM.

7. P.313, line 10, but is EpsNd conservative if isotopes are exchanged with the margin as suggested here and in the other papers of this group?

9. P.313, line 10, I am not convinced that the assertion that biogenic cycles do not affect Eps Nd is true in the Pacific. It seems reasonable to assume that Nd may be scavenged and released by falling particles and released during dissolution. This would explain why Nd concentrations increase with depth and in the deep Pacific, in line with nutrients. In the Atlantic horizontal transport would appear to dictate the distribution of Eps Nd away from the margins but the same vigorous transport does not exist in the Pacific. If biogenic particles do scavenge Nd in the Pacific might this effect not dominate there, in the absence of strong, horizontal transport? If that is the case biogenic cycles could be important.

10. P.314, line 15, I think discussion of the work of Tina van de Flierdt is needed here

11. P.314, line 17, I think the point is that the Nd input may change under different climatic conditions. These occur in a given time frame but it is the climate, not just time which drives the suggested change.

12. P.315, line 15, If the modern circulation is so far from observations then is it useful to use this model to consider times when we have no direct observations?

13. P.317, line 4, at least some discussion of the BE modeling is needed. I suggest a paragraph summary would be adequate.

14. P.318, lines 6 to 7, much more data is available than that shown

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15. P.318, last paragraph, the details given here could be incorporated into Table 1

16. P. 319, if all the outcomes are so similar it seems to make sense to discuss what is similar between each simulation (i.e. the Nd input is similar). Only changing this makes a big difference to the model outcome in terms of basin averages. However, I think the point is that locally big changes are apparent between each run. I question the usefulness of looking at basin averages compared to local effects. Eps Nd is heterogeneous in the ocean and it is the detailed pattern which changes, not necessarily the mean.

17. P.320, line 10 to15, I guess that residence time here has a specific meaning, i.e. residence time on the margin. The paper needs a discussion of BE to make this clear.

18. P.322, line 11, state the changes for the range of simulations as well as ModernM. The point is that only changes in the input change the basin mean but….

19. P.322, line 18 to 21, changes in circulation change the distribution of EpsNd. This is why the authors must plot EpsNd as well as the change in EpsNd for each run. Otherwise we are trying to reconstruct EpsNd in our heads from the difference plot.

Technical comments

The terms since and as are used as a variant on because throughout the paper. This is often confusing because since and as both have time connotations. This paper deals with time issues and so it is particularly useful to stick to because throughout.

THC is used often without proper definition. What is actually meant is tracer-transport pathways, which are linked to ocean dynamics. These pathways likely change alongside changes in gross ocean circulation but the authors should be careful on this point. Wunsch suggests a careful consideration of what is meant by this term and the family of terms used to discuss tracer transport (i.e. THC, MOC, density circulation…).

P. 309, line 10, typo are remain

P.313, line 13, cite review of Goldstein and Hemming

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P313, line 13, suggested paragraph break before The modern..

P.313, last line, typo current knowledge of the

P.314, first line, be specific. Indeed, Piotrowski et al. ….

P.315, line 26, LGMA I suggest LGMa is better because it distinguishes the acronym LGM from the run name a. The other runs would be LGMb and LGMc etc

Results and Discussion sections, very long, meandering paragraphs in this section need subdividing and I suggest a restructuring. See General comments

P. 317, line 1, state where these will be published

P.318, line 9, an Eps Nd…

P.321, line 6, plural OGCMs

P.321, line 19, Urgent is unnecessary hyperbole, remove it

There were enough language issues to make me think that I may have missed a few. Please check the revised final version thoroughly.

Interactive comment on Clim. Past Discuss., 4, 309, 2008.