

Interactive comment on “Modeling the Nd isotopic composition in the North Atlantic basin using an eddy-permitting model” by T. Arsouze et al.

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To the Editor,

This letter aims to be a point-by-point reply to the suggested corrections provided by the two reviewers. Sentences from reviewers start with the ‘ (RC) ’ symbol, whereas author’s reply sentences start with the ‘ (AR) ’ symbol.

A revised manuscript, modified according to the suggestions of the reviewers has been prepared and is ready for submission.

Sincerely,

Thomas Arsouze, on behalf of all the co-authors of the manuscript.

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Referee #1: Mark Siddall

Optional Comments:

(RC) If there is one significant addition to the paper that would be useful to both authors and readers, it would be some discussion as to how to parameterise these effects in lower resolution models (useful because they can model the globe and not just the high-latitude north Atlantic). Similarly it would be useful to discuss what the implications might be for models that include the flux of Nd into the ocean, rather than simply EpsNd.

(AR) Global low-resolution models already include an explicit parameterization of the sub-scale physics. However, the artifacts of dynamical representation induce a different estimation of the relaxing time: about 1 year in ORCA2 configuration compared to few days to 6 months in NATL4 configuration. This regional high-resolution study will help for future developments with global high-resolution models. The expected implications on a model that includes flux of Nd into the ocean might not be as spectacular as the effect that can be observed when only modeling ϵ Nd. Indeed, the source of Nd on the margins is compensated very rapidly by subtraction, hence implying almost no change in the concentration signal. On the contrary, the ϵ Nd signal at the margin is instantly recorded and memorized by the water mass. These comments have been added in the discussion section.

Minor Comments:

(RC) Abstract, 1st sentence: reference needed

(AR) As suggested by the reviewer, we added a reference in the manuscript that supports this first sentence.

(RC) P977, L15-20: would it be useful to mention the scope for using the high-res model to develop more realistic parameterisations of exchange/input of Nd here?

(AR) A note concerning this suggestion has been added accordingly.

OSD

7, C348–C352, 2010

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(RC) P980, L18-20: is the better fit simply a function of capturing more heterogeneity in the Nd boundary condition?

(AR) We're sorry, but we don't understand this remark.

(RC) P982, L9: '. . .surface water. . .'

(AR) This has been changed in the manuscript.

(RC) P982, L17: '. . .river discharge. . .'

(AR) This has been changed in the manuscript.

(RC) P982, L24: '. . .which are more realistic. . .'

(AR) This has been changed in the manuscript.

(RC) P982, L28: '. . .the ORCA2. . .'

(AR) This has been changed in the manuscript.

(RC) P983, L2: '. . .boundary current representation to simulating. . .'

(AR) This has been changed in the manuscript.

(RC) P983, L4-8: I don't quite understand this sentence, please make clearer and check with a new reader that the meaning is clear.

(AR) The sentence has been slightly modified, and detailed concerning the consequences on the Nd signal.

(RC) P984, L1-2: this needs further explanation for the non-expert. For example, what is a topographic sediment delivery map?

(AR) The manuscript now provides more details concerning both the limitation of a topographic map approach, and the advantage of using a geographic sediment delivery map.

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(RC) P984, L7: 'The characteristic. . .'

(AR) This has been changed in the manuscript.

Referee #2: Anonymous reviewer

General Comments:

(RC) Page 979 Lines 5-10. The authors "make the implicit hypothesis of constant Nd concentration." I find this unrealistic. The solubility of Nd in seawater is influenced by the redox chemistry of the water as well as its carbonate concentration. These conditions can change geographically (especially along continental margins) and temporally and likely play a role in the rate of BE at a given location. The discussion could be improved by addressing the extent that the above assumption is valid and whether or not a model could ever include geographical variations in Nd concentration.

(AR) This study is a first attempt to model Nd in an eddy-permitting resolution model. Modeling Nd concentration as well as Nd isotopic composition, would imply an estimation of the flux of the sources, and a modeling approach similar to what has been done in Arsouze et al. (2009). However, as stated in the manuscript, we did not adopt this approach that induces very high computational costs. We are aware that is an important limitation, but don't think this affects the general conclusions of the study. Also this approach of constant concentration has already been validated on a global scale modeling (Arsouze et al., 2007). In the manuscript, we added a sentence explaining the choice of this hypothesis, and also reminded at the end of the discussion that the constant concentration hypothesis still limits us in constraining the value of the relaxing time.

(RC) Page 982 Lines 6-10. This is confusing. If BE provides >90% of oceanic Nd, how could river input dominate the isotopic composition?

(AR) It is suggested that more than 90% of the global Nd sources to the ocean come from BE (Tachikawa et al., 2003, Arsouze et al. 2009). However, this source applies

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from 0m to 3000m. Other sources like river discharge or atmospheric dusts are concentrated on the surface, and thus able to dictate the isotopic signature of surface and subsurface seawater. This section has been slightly rearranged in the manuscript.

Technical Comments:

(RC) Last sentence of the abstract is unclear

(AR) This sentence has been rephrased.

Interactive comment on Ocean Sci. Discuss., 7, 973, 2010.

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