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Biogeosciences Discussions

Interactive comment on "Reconstructing the Nd oceanic cycle using a coupled dynamical – biogeochemical model" *by* T. Arsouze et al.

Anonymous Referee #3

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This paper presents and interesting new approach to model the global Nd concentration and isotope composition by a coupled dynamical-biogeochemical model including parameterization of source and sink terms. Despite some shortcomings of the combined models, for example the overestimation of the remineralisation efficiency with water depth and the distribution of biogenic particles dominating the particle field in the PISCES model, this is in my opinion a useful exercise and an important step in bringing these types of models together for the simulation of the distribution of trace metals and their isotopes at a reasonable computational effort. The main finding of the authors besides the unsurprising fact that vertical scavenging needs to be considered is that the Nd isotope composition and concentration at the same time can only be reproduced if boundary exchange is the main source of Nd to the ocean. Given that I am not a modeler this is an important result for my perspective from the data side

because this clearly demonstrates that there need to be dedicated process studies at ocean margins, for example in the frame of the GEOTRACES program, to quantitatively understand the sources of Nd and REEs. So far there are almost no direct observations of the boundary exchange processes and in the moment boundary exchange is sufficiently unconstrained to serve as a free parameter to bring observation and model into agreement. The model also confirms that Nd isotopes can be used best as circulation tracers in areas far away from land and where water mass exchange is fast. In the paper the authors also include a comparison to results and approaches of previous modeling efforts, which will be helpful for the non-modeling readership of Biogeosciences. The paper is generally concise and well-written but could benefit from a proofreading by a native speaker to improve the English. I recommend publication after inclusion of a number of comments detailed below, which I consider a minor revision.

Comments:

One of the shortcomings of the input parameterization at the margins is my opinion the assumption that the input is similar in amount at every margin. There is clear evidence that shelves underlying oxygen minimum zones serve as a stronger source than low productivity margins (cf. Haley et al., 2004). This should be explicitly mentioned and the authors may want to include an estimate of this source in their input function based on the global fraction of such suboxic/anoxic shelves.

It is obvious that the model needs to be extended to different types of particles which most likely will have very different distribution coefficients (Kds) for Nd (similar to the modeling approach by Siddall et al., 2005 for Th and Pa). For this exercise however, a dedicated study of the Kds for REEs needs to be carried because the information on particle type affinities for Nd and the REEs is quite scarce, as far as I am aware.

Minor Comments:

Page 5562, line 12: What is Ap/Ad? I guess it is particulate over dissolved Nd concentrations but the authors need to say so.

BGD

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On top of page 5567, the authors claim that Nd concentrations were simulated "an order of magnitude lower". Lower than what? I don't see that when looking at fig. 5 where the Nd concentrations for Exp 2 and 5 are quite close to the real values.

In figures 3 and 5, the authors need to provide latitudes of their sections or at least provide it in the captions. These figures, as well as figs. 7 and 8 need to be provided at high resolution because otherwise it will be very difficult to identify any details, which is important to distinguish and to compare the results of the experiments. The complete list of references for all data used in these figures needs to be provided in the captions of the figures. There are many data missing from crucial areas, such as for example Spivack and Wasserburg (1988) or Stordal and Wasserburg (1986) for the Atlantic and Amakawa et al.'s surface and deep water data in the Pacific and Indian Ocean. There is also a new and important Nd isotope data set for the eastern Atlantic, in particular the deep southeastern Atlantic Basin (Rickli et al., 2009), which provides strong evidence for continental contributions from Africa and which needs to be included in figures 7 and 8.

For figures 4 and 6 the authors need to provide correlation coefficients. It is virtually impossible to judge the agreement between data and model on the basis of only visual comparison of the cross plots and the correlation lines.

Additional references:

Haley B.A., et al., 2004, Rare earth elements in pore waters of marine sediments, Geochim. Cosmochim. Acta 68, 1265-1279.

Rickli et al., 2009, The hafnium-neodymium isotope composition of Atlantic seawater. Earth Planet. Sci. Lett. 280, 118-127.

Interactive comment on Biogeosciences Discuss., 6, 5549, 2009.