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Geoscientific Model Development Discussions

# *Interactive comment on* "Neodymium isotopes in the ocean model of the Community Earth System Model (CESM1.3)" *by* Sifan Gu et al.

**Anonymous Referee #1**

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Neodymium (Nd) isotopes are useful tracers increasingly used as a paleoclimatic proxy of oceanic circulation evolution. In this paper, the authors have implemented Nd isotopes in the ocean model of the community Earth System Model (CESM). This implementation follows a relatively long series of Nd modeling efforts during the past 15 yearsÂă: Tachikawa et al. (2003), Arsouze et al. (2007, 2008, 2009, 2010), Siddall et al. (2008), Jones et al. (2008), Rempfer et al. (2011, 2012), Sepulchre (2014) or Ayache et al. (2016). All these studies were motivated by the needs of two main scientific communities: geochemical (to better constrain the oceanic cycle of the Nd isotopes and its potential as an oceanic circulation proxy) or paleoclimatic (to understand the past variations of oceanic circulation and comparison with Nd archives). This present paperonly thoroughly reproduces (including the exact same sensitivity tests and diagnostics) with the CESM model, what has beendone in the study by Rempfer



et al. (2011) with the Bern3D model and does not provide any new insight on either of the scientific understanding or technical aspects of what has been done in the previous papers. An interest of such a study could have included an accurate comparison between these new results and Rempfer et al. (2011) (or Arsouze et al. (2009)) in order to identify if some specific drawbacks of one model in some areas were not reproduced by another model, etc... but most of the time the description of the comparison is limited to "similar". GMD papers should propose cutting-edge new parameterizations or enhancements in existing models, motivated by our limits in answering scientific questions. This already existing (first implementation published in 2009 !) implementation of Nd isotopes in the CESM oceanic model is clearly missing a scientific objective, whether motivated by numerical or geochemical questionings.

The main interest of this paper lies in a new modeling tool made available for the CESM users community, and therefore should probably be released as a technical internal note. The article is rather clear and well written. The validation of the simulations, although too descriptive and simple, is done thoroughly and should provide a useful (internal) reference for future publications of Nd isotopes simulations with CESM model. On reading this paper, I do not see significant improvement concerning the understanding of the Nd oceanic cycle, nor the improvment of the Nd cycle modeling, nor the intercomparison of Nd modeling with different oceanic models which could be of interest to the GMD readership. Hence, I cannot recommand this article for publication in GMD.

Please find below some comments below:

The authors consider a $\pm$ 3eNd metric to validate the simulations. However, variations in Nd archives are often within this range. This strong limitation to the validation should be mentioned.

On all the results and figures: it is not clear what variable is shown ? Is it a snapshot of the last month or last year of simulation ? Is it averaged over the last XX years ?

Most a the description of the simulation refers to Rempfer et al. (2011), but Rempfer et al. (2011) originally refers to Arsouze et al. (2009) and Tachikawa et al. (2003). This should be corrected.

l61: Siddall et al. (2008) state that both lateral advection and reversible scavenging are needed

l61: you should probably also mention Boundary Exchange as a possible process to explain the Nd paradox

l129: can you specify the time period simulated ("normal year") and the interannual variability of your model ? I am not sure to understand: do you run the full CESM or do you read oceanic fields offline from a pre-computed simulation ?

l132-134: This paper is a validation paper but it is stated that future simulations (in particular for paleoclimate studies) will be carried out with a different version (higher resolution) of the model. I understand the interest of performing a large range of simulations with a low resolution model to optimize coefficients but at least one simulation (and even BS-XX and PD-XX simulations) at 1° resolution should have been performed. Does the higher resolution model improve the Nd results ? Are the parameters selected relevant at higher resolution ? Are they still optimum ? Is the sensitivity to parameter changes the same ?

l154: you need to tell a little bit more about the particle fieldsÂă: what is exactly the difference between "eco" and "abio" ? I understand that "abio" comes from an output of a previous simulation, but what is this simulation ? Does the "eco" simulation use the same setup for the biogeochemical model as the one used to generated the "abio" fields ? In other words: do differences between those two simulations only reflect online vs offline effect or also a change in the particle concentrations / fluxes ? Do you expect an optimized coefficient change with consequent particles distribution changes (as possibly expected in paleo studies) ?



l397: can't this shift in depth be attributed to a too slugish AMOC that favors vertical cycling rather than lateral advection ? Actually, the core of NADW visible from eNd data is rather 1500m than 3000m used here. l404-406: it looks that you use the same justification as Rempfer et al. (2011). What could be the drawbacks of the "sources simplifications" ? As you have a higher resolution of the CESM model available, did you test if improving the resolution helps reducing your biases ?

l425: altough this sensitivity test has been performed in Rempfer et al. (2011), you should precise what your motivations are for performing such a sensitivity test.

throughout the text: fbounday $\rightarrow$ fboundary

Fig2: it does seem that you include oceanic ridges in your sources. Wether ridges are sources (in the Pacific ?) or sinks (as we'e thought for a long time), it is very unlikely that values can be -10/-5 eNd.

Fig8: should put a colorbar here

Fig12: if you only look at online vs offline effects here (not sure this is the case), we would rather expect to see variations near the surface, which should be more relevant to look at.

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