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

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We thank the reviewer for his/her time for constructive comments.

We are sorry that we have not made it sufficiently clear that the main objective of this paper is a documentation of Neodymium isotopes in the CESM. To emphasize this, we will emphasize this in several additional places in the paper. Indeed, this paper is a follow-up of Jahn et al. 2015, which describes carbon isotope implementation in the CESM. We are implementing different isotopes in the CESM for the purpose of the capability of a direct model-data comparison, which will help the community to better understand past climate changes in terms of better interpretations of different proxy records as well as model validation. We first implement this isotope in the CESM and will be using this module to explore some paleoclimate problems. For example, we are currently using this tracer module to resolve the controversy of available εNd reconstructions in tropical Atlantic (Huang et al., 2014; Pahnke et al., 2008; Xie et al., 2012). Since CESM is a community model and will be used by many users, we were told it is necessary to document Nd implementation in the CESM. Thus, this is a technical paper, which describes and documents a new feature of the CESM. This is why we submit it to GMD. As far as we know, this is the first attempt to implement Nd in the CESM. The reviewer pointed out that the already existing implementation of Nd in the CESM ocean model in 2009, but we cannot find any references regarding this. We have no information on this from our NCAR collaborators either.

In addition to the documentation purpose, we do have some other points to make. First, we follow the methods in Rempfer et al., 2011 since it is the most comprehensive survey of Nd cycle. We use their method, but we are implementing in a different model. Bern3D is an intermediated complexity model, but CESM is a much more sophisticated model. It is not obvious that the two independent models have to produce similar results. In addition, we use a more completed data set to tune model parameters (line 97-104), which yields similar parameters as in Rempfer et al., 2011. A totally different model, as well as double the amount of available observations, give similar results. In some sense, our work is a confirmation of the robustness of Rempfer et al., 2011.

Second, different from Rempfer et al., 2011, we implemented the abiotic Nd module, which uses fixed particle fields as Rempfer et al., 2011, as well as a full biotic Nd module, which is coupled with active marine ecosystem calculating particle fields simultaneously. The abiotic and biotic Nd shows identical results under present forcing because the particle fields used in abiotic Nd module is the climatology of the particle fields in the marine ecosystem. Therefore, in principle, when equilibrium is reached, abiotic and biotic should produce the same climatological Nd fields (there are some seasonal variations, but the comparison showed in the paper are all decadal mean). However, if we use this module to do paleo simulation, in which both circulation and particle fields are changing, these two versions of Nd have the advantage that we can separate the two effects. But this kind of simulation is beyond the scope of this study and we are working on extensive paleo simulations and will be addressed in ongoing work.

In the following, we have addressed all comments, with the original review quoted.

"The authors consider a +/- 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."

Thanks for pointing this out. Yes, we should mention this limitation in the text. This +/- 3 εNd for validating is from Rempfer et al., 2011, we use their measurement as a benchmark, therefore we can compare with their results.

"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 ?"

We should state this clearly. All the results and figures are based on the latest ten years average (decadal mean) in each experiment.

"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."

Thanks for pointing this out. Maybe we should explicitly say that Rempfer et al. 2011 is based one previous works. We also cited Arsouze et al. 2009 and Tachikawa et al. 2003 where similar methods are involved (For example, line 185, line 237, line 245).

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

In Siddall et al. 2008, they pointed out that the Nd paradox can be explained by the combination of both lateral advection and reversible. We should also include lateral advection here. Any tracer is subjected to ocean transport, but the reversible scavenging is unique.

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

Boundary Exchange, which is Nd exchange between water and sediment, is more related to the source-sink for Nd cycle. Nd paradox is the decoupling of Nd and εNd. We think reversible scavenging and lateral advection are more important in explaining 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 ?"

CESM has different components: atmosphere, ocean, land, ice and river. Different components communicate through coupler. These components can be run together as fully coupled and also can be run alone with other components as data. For example, in this study, we run ocean alone experiment (for the purpose of reducing computational cost): ocean is active, but other components are data (data-atmopshere, data-land, data-ice and data-river). Ocean component does not know whether the fields passed through coupler are data or active. Ocean component is active. It is not offline.

CORE dataset (Large and Yeager, 2008) is based on NCEP reanalysis and satellite observation and provides a method to run ocean model without a fully coupled GCM. This method has been widely used in ocean alone simulations. It has two options: one is “normal year forcing”, which is repeating seasonal cycle and no interannual variability in this forcing; another is the interannual forcing.

"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 ?"

We use the 3 degree ocean as in Jahn et al., 2015. This Nd module will be eventually validated in fully coupled CESM (active atmosphere, land and etc.) at 1 degree resolution along with all other isotopes (e.g. carbon isotope, water isotope) in the future. However, we think the results should be similar. The reason is that our results (optimal parameterization, model sensitivity to parameters) are similar to Rempfer et al. 2011, which is from a totally different model (Bern3d, intermediate complexity) at much lower resolution.

"l154: you need to tell a little bit more about the particle fieldsÂËŸ a: 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) ?"

The differences between “abio” and “bio” are stated in line 107-110, line 155-159, line 268-272. CESM have active marine ecosystem module, which can simulate particle fluxes online. The climatology under normal year forcing is shown in section 2.2. The exactly difference between eco and abio is that eco uses particle fluxes simultaneously computed by ecosystem, while abio uses particle fluxes fixed at prescribed values, which is the climatology of the same model (CESM) under the same forcing (normal year). Eco and abio Nd can be turned on separately or together during the model set up steps. So they can be run under the same model setup. (All experiments in this study are under the same model setup: active ocean under normal year forcing).

The difference between climatology abio and climatology bio results is quit small if we compare the magnitudes in Figure 12 with the magnitudes in Figure 5, 9 and 10. This is intuitive as the physical circulation is the same, the particle fields are one with seasonality (eco) and another is climatology (abio). But if we run paleosimulations, the particle fields produced by the active ecosystem module will be different from the present day condition. In that case, we anticipate there will be much larger difference between abio and eco results.

We will apply this optimal parameter tuned for present day to paleo studies since we don't have a better option. First of all, our knowledge of past Nd sources and εNd are limited and paleo observation is much less compared with present day. It is not practical to tune the model parameters under paleo condition (for example, Last Glacial Maximum). Secondly, as we show in Table 2, EXP1 and EXP2 are slightly different parameters from CTRL, but the results are similar. Model sensitivity is quite small around the parameter setting in CTRL (Fig.3). And the particle fields in our model tuning process is different from Rempfer et al. 2011, although the general patterns are the same, we get similar optimal parameters. Thirdly, as shown in Figure 14 and Figure 16, we double or half those two parameters, but changes in εNd are very small. εNd is the proxy widely used in paleo studies. Therefore, we feel justified to use the parameters in paleo studies. Also, as Rempfer et al., 2012 pointed out, substantial changes in εNd is smallNd sources are required to generate large-scale changes in deep water εNd comparable to deglacial εNd reconstructions. But this is definitely a drawback of the model and should be kept in mind.

"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 ?"

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Discussion paper

The AMOC strength is our simulation is about 16.6 Sv, it is in the range of present day estimation. Therefore, AMOC is not sluggish in our model. Profiles in North and South Atlantic (Figure 10, profile 5 and 7), the less radiogenic NADW core is around 3,000 m.

Figure 10 profile 4 show model-data mismatch: observation show a very large vertical gradient for over 5 εNd unit within 3,000m, while model show a much less vertical gradient and more homogenous εNd at this location. This is deep convection region, therefore, the watermass should be well mixed, as shown in the [Nd]d profile in Figure 9 profile 4. Probably the very unradiogenic value near the surface in the observation have some surface input which our model don't have. It is impossible for the model to simulate every point consistent with observation, especially considering our simplifications on Nd sources. For example, the εNd field prescribed for dust source (Figure 2c) only have a large scale gradient and a homogeneous value in each basin; We use a global uniform boundary source magnitude (fboundary). Our goal to capture the general picture of the distribution of [Nd]d and εNd.

Our model resolution is improved from Rempfer et al. 2011, but our 3° model is still rather coarse. We haven't tried a higher resolution model. We can take a look if this is improved when our 1 degree run with all other traces is ready, but we don't think it will make a large difference. Our εNd fields prescribed for the boundary, river source (Jeandel et al., 2007) and dust source (Tachikawa et al., 2003) is also very low resolution. Therefore, if we use a high resolution model but with low resolution sources, we don't think it will help a lot. But if in the future, we have much higher resolution observations about Nd sources, it will definitely help to improve regional model-data inconsistency. Again, our point is that our model is able to capture the big picture and resolve small scale regional model-data consistency is out of the scope of this study. It may be improved in the future by using some regional model, or using a ensemble Kalman filter to have better parameterization in different regions(Liu et al., 2014).

"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."

These sensitivity tests have been run during the parameter tuning process. Here we show these results to confirm we have the same sensitivity as in Rempfer et al. 2011.

"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."

We follow the method in Rempfer et al. 2011 by applying a boundary source at the continental margins above 3,000 m. Figure 2b show the grids above 3,000 m in our model. The εNd prescribed for the boundary source is extrapolated from (Jeandel et al., 2007) for a global coverage. The boundary source is 3.57*10-5 g/(m2s) (fboundary/Total_area). The average sink in this bridge in Pacific is 4.2*10-12 g/(m2s) (Nd_p*w*thickness of the grid). Therefore, it is a local source in our model.

"Fig8: should put a colorbar here"

Thanks for suggesting this. Putting a colorbar will help the readers. Colors here refer to different depth range.

"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."

Figure b and d shows the vertical difference. The maximum differences between “abio” and “eco” are near the surface, but the magnitude of the maximum differences is still very small compare with the magnitude in Figure 5.

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