

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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Referee comments

During the public discussion of your manuscript two referee comments have been posted, to which you have replied in a particularly timely manner – thank you!

While both referees appreciate the thorough and rigorous validation of the model, they also emphasize the lack of originality in the developments reported. They find that the manuscript brings

- no significant improvement of the understanding of the Nd oceanic cycle;

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- no improvement of Nd cycle modelling;
- no intercomparison of Nd modelling with different oceanic models.

As summarized by Referee #1, the current version of the paper merely reproduces with the CESM what Rempfer et al. (2011) have done before, including exactly the same sensitivity tests and diagnostics, and generally reaches similar conclusions.

Both referees nevertheless make constructive suggestions about how to adapt the manuscript in order to move it away from the level of a “technical internal note” (Referee #1) and to make it a valuable reference for future studies involving Nd and its isotopes with the CESM. Referee #1, e. g., explains that the submitted version is lacking

“[...] 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.”;

Referee #2 suggests to focus on the parameter tuning procedure which could be

“[...] realized separately for different oceanic basins (Atlantic and Indo-Pacific separately and Southern Ocean as [a] buffer zone to ensure the continuity, for example). The upper layers affected by dust and river water will be treated separately from and the lower layers.”

Referee #2 furthermore outlines ways to address the impact of the assumption of homogeneous margin Nd fluxes (which has been questioned in the earlier work of Rempfer et al. (2011), i. e., by several of the co-authors of this paper) and even provides references to relevant recent papers that could be used to define meaningful sensitivity tests to make progress on the problems related to these margin Nd fluxes.

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Finally, Referee #2 singles out the biotic vs. abiotic module as the most original contribution of the reported development works, but finds that only a small part of the text is devoted to it.

I have now reread your paper in the light of the referees' comments and looked up the relevant literature cited in the manuscript and in the referees' comments. In conclusion, I concur with the referees. They have certainly provided critical, but above all, care-, thoughtful and constructive reviews, for which I thank them.

Replies to referees

I have also reread the replies to the Referees and I find that the referees' major concerns and suggestions are, unfortunately, not fully addressed. The reply to Referee #1 does not mention the advisable comparison of this model's results with results obtained from other models.

As stated in GMD's *Manuscript type* information page, "[d]evelopment and technical papers usually include a significant amount of evaluation against standard benchmarks, observations, and/or other model output as appropriate." Obviously, there are no benchmarks available in this case and the model-data comparison merely reproduces the results of the Bern3D implementation described by Rempfer et al. (2011). The implicit recommendation of Referee #1 seems thus rather natural: please present some in-depth comparison with the previous implementation (or with the results of Arsouze et al. (2009), if you prefer to compare with a model of similar complexity than CESM).

Most unfortunately though, the concrete and outstandingly constructive suggestions of Referee #2 are not considered at fair value: the first one (tuning procedure) is relegated

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to "future work" while the second one (margin Nd fluxes) is declared "out of scope."

It should be possible to adapt the Nd cycle implementation in the model without too much hassle so that different parameter values can be used in different ocean basins and depth layers, as suggested by Referee #2, without having to call upon a Kalman filter method to tune these parameters. Designing one or two meaningful sensitivity tests for the margin Nd fluxes should also be feasible on the basis of the provided reference papers!

In the replies to referees, I read that the document "[...] is a technical paper, which describes and documents a new feature of the CESM, which fits the scope of GMD." It is certainly correct that this paper a priori fits into the scope of GMD. However, although GMD *Model description papers* are expected to put a stronger focus on technical details than papers in other, less model-centric, journals, GMD papers must not reduce to technical reference notes only. We clearly expect that papers "present significant advance" (see Editorial 1.1, section 2.3) in the scientific research area that they contribute to. Furthermore, this paper presents *incremental model development*: new functionality is added to an existing model. We explicitly encourage submission of such papers to GMD, but they "must include a tangible and potentially useful advance related to model development." (Editorial 1.1, Introduction). As it currently stands, the expected tangible advance is not sufficiently developed in this paper. I believe that target could nevertheless be easily reached if the referees' comments and suggestions were pursued.

Decision

Referee #1 states in his/her report that he/she cannot recommend the paper for publication in GMD; Referee #2 concludes that more efforts are required if the paper is meant to become a reference for future studies using Nd isotopes in the CESM, and

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accordingly recommends a complete overhaul and resubmission. In their evaluation reports, the two **referees unanimously recommend to reject the manuscript** in its present form.

In its current version, **the manuscript cannot be accepted for publication**. The amount of work required to address the concerns expressed by the referees and to take their most important suggestions into account – as well as the referees' unanimous advice – precludes “major revision” at this stage, I am afraid.

I do, however – just like the referees – see good potential in the manuscript and I strongly recommend to **revise your paper** following the recommendations made by the two referees **and submit a completely new version**. Please provide readers with a more systematic intercomparison with another Nd enabled biogeochemical model (the Bern3D or another one) and amend the developments along the lines laid out by Referee #2.

Regarding the abiotic vs. biotic module, which has been singled out as the most original contribution in this paper, I would like to add the following suggestion: why not include a simple sensitivity test, with some prescribed (possibly hypothetical) change, such as a reduced AMOC or modified export production in order to illustrate the potential of the approach. I also find the denomination “abiotic” somewhat misleading, as the underlying particle flux distribution includes a biogenic contribution, although a fixed one. Please take also advantage of the other comments and recommendations provided by the referees.

I am looking forward to reading your fresh manuscript.

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References

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