egusphere-2022-606 Review

Title: Simulating marine neodymium isotope distributions using ND v1.0 coupled to the ocean component of the FAMOUS-MOSES1 climate model: sensitivities to reversible scavenging efficiency and benthic source distributions

Reviewer: Benoît Pasquier

General comments

This is a welcome and timely manuscript describing a new marine Nd-cycling model embedded in a fast GCM that is well-suited for future exploration and optimization. The authors also present preliminary results of the sensitivity of their model to varying two important parameters of the Nd cycle, which already offer new insights into our understanding of the global Nd cycle and its isotope signature.

The model skill is thoroughly examined through quantitative metrics and expert assessment of the tracer distributions. To the best of my knowledge, the science supporting the model is sound, the context and references are properly presented, and many of the potential caveats of the model are presented.

Although I have not attempted to reproduce the scientific results myself, I commend the authors for making available what seems to be all the necessary code files and data for running the simulations.

The manuscript is structured well, the presentation is clear and easy to follow, and the figures are of high quality.

My biggest issue with the current manuscript is a small one and lies within the angle or sometimes plain omission of some necessary discussions around the caveats of the model.

Overall, I would recommend the publication of this manuscript after minor revisions.

Below is the list of minor suggestions and comments,

Specific comments

- **On the model's ocean circulation,** I have a few suggestions that the authors might or might not want to consider.

  At the end of the model description (Section 2.1), the authors explain that their choice of the older MOSES version was driven by the bad ocean circulation of the more recent version ("collapsed Atlantic Ocean convection and strong deep Pacific MOC"). Obviously, no ocean circulation model is perfect, and I commend the authors for detailing their choice of ocean circulation model in the following section (2.2), but I think a bit more could help there.

  - Suggestion 1: Add some discussion about how the quality/skill compares to other GCMs. This could be illustrated by "simply" adding other GCMs to Fig. 1. I believe this would help the reader assess the author's model choice

    Note this suggestion comes from my perspective as a data-assimilated ocean circulation user. RMS errors of about 2°C and 0.9 PSU for temperature and salinity, respectively, seem like large biases, given, e.g., the "old" OCIM1 circulation model (*DeVries and Primeau*, 2011) and its RMS errors of less than about 0.2°C and 0.05 PSU (i.e., about 10–20 times better on that specific metric).

  - Suggestion 2: Discuss how well the selected FAMOUS model does in reproducing other circulation tracers (e.g., those mentioned by the authors, $\delta^{13}C$ and $\delta^{14}C$).

- **"bottom-up" vs "top-down"**. The model presented is a "bottom-up" model, which means that roughly 85% of the Nd tracer is injected at the bottom of the ocean. Although that fraction varies from 66% to 89% in their experiments, discussions on the possibility of a potential "top-down" model (where said fraction would go much lower) are sparse. I think a more thorough examination of the possibility that baking-in strong sedimentary fluxes can be a caveat in itself and discussing alternatives in a more balanced way would strengthen the manuscript. (More details in the line-by-line points below)

- **Unit suggestion**. Throughout, maybe by convention or already established precedent, the authors express quantities that I believe could be simplified for clarity. For example, fluxes are expressed in g yr^{-1} but are of the order of 10^9 g yr^{-1}. This begs the question: Why not use Gg yr^{-1}? This would remove many "$\times 10^x$". Alternatively, since [Nd] is expressed in pmol kg^{-1}, maybe the authors could expres sources and sinks in Mmol yr^{-1} instead of g yr^{-1}. (These are just suggestions.)

Line-by-line suggestions, comments, typos, etc.

- Eq. (1): While there is a mountain of established precedent publications that have adhered to this ε_{Nd} notation, the authors might be interested in checking *Coplen*, 2011 (doi:10.1002/rcm.5129) for ε_{Nd} notation and unit), which argues for writing it in δ notation, without the superfluous 10^4 constants, and expressing it in parts per ten thousand (‱):

  > $\delta_{Nd} = IR_{sample} / IR_{CHUR} - 1$

  For what it is worth, in *Pasquier et al.* (2022), we opted to keep the ε_{Nd} symbol but expressed the equation without a unit (i.e., without the 10^4). No change is required here, just pointing at some potential improvements.

- L47 (missing "in"):

  > he measured Nd isotope composition of seawater is not actively involved ***in*** marine biological cycling

- L56 (and all other occurrences) the year of our *Pasquier et al.* publication should be 2022 instead of 2021 (and a DOI should be added).

- L156: The *Jones et al.* (2008) citation should be removed (because it is not about the FAMOUS model).

- Fig. 1 (Taylor diagrams) is missing units.

- L258: Not that it is important, but I am curious, as this flew over my head:

> This technique minimises the mathematical error associated with carrying small numbers.

What is the reason here? Isn't FAMOUS written in Fortran and doesn't it deal with floating point arithmetic correctly for small numbers?

- Table 1:
    - The exponent of the yr unit is shoved to the next line (for several rows), slightly reducing readability.
    - As per the specific comment above, maybe better unit choices can improve clarity?
- L275 (and throughout): The context makes it clear that f_{dust} is in grams of Nd. Maybe remove the "(Nd)" in "g(Nd) yr^{-1}"?
- L284–286: It is unclear how the additional constraints on the aeolian ε_{Nd} are applied. It is probably worth expanding/detailing.
- Fig. 5:
    - Panel a: This filled contour map essentially looks bicolor to me. Could a log scale be applied to the colormap to distinguish different river discharge strengths?
    - Panels b and c: While the *Goldstein and Jacobsen* (1987) reference is given, it is unclear how the prescribed riverine ε_{Nd} and [Nd] gridded datasets are created.
    - Panel d (missing): Could the authors add a map of the resulting riverine Nd source?
- Table 1 + Eq. (7): S_{river} units issue. Substituting the units from Table 1 into Eq. (7) yields

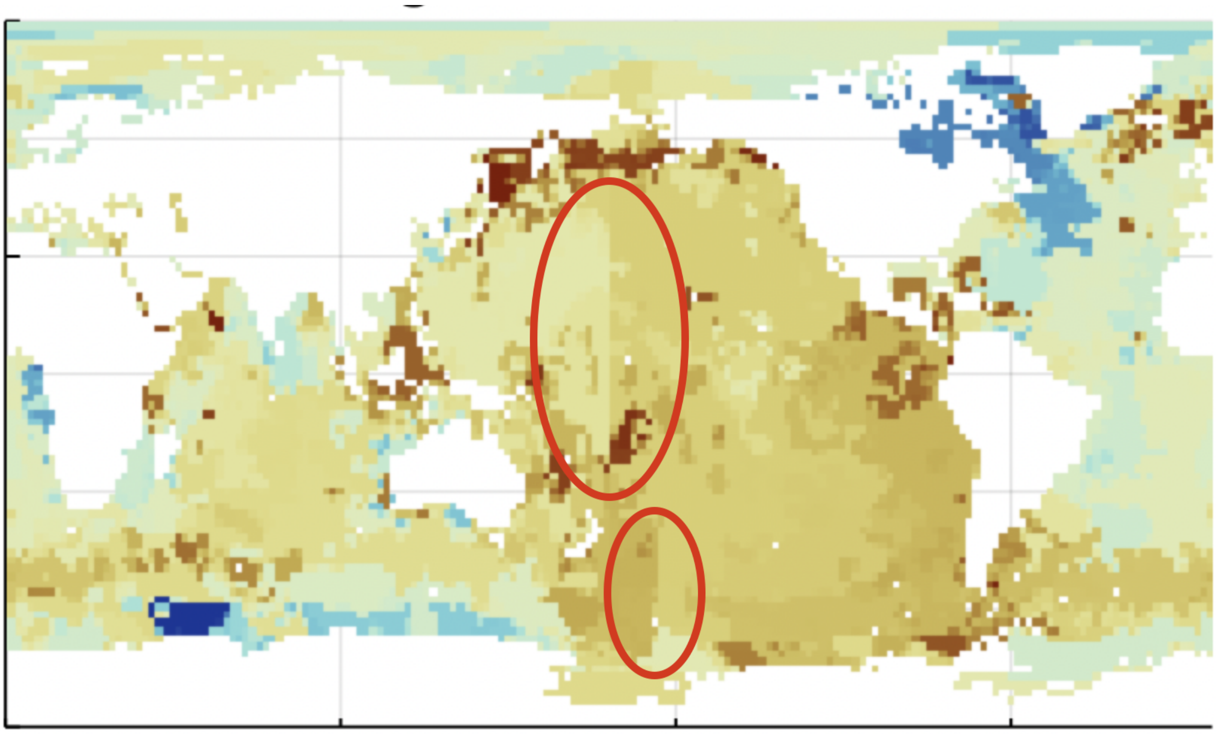
> m^2 yr^{-1}

instead of

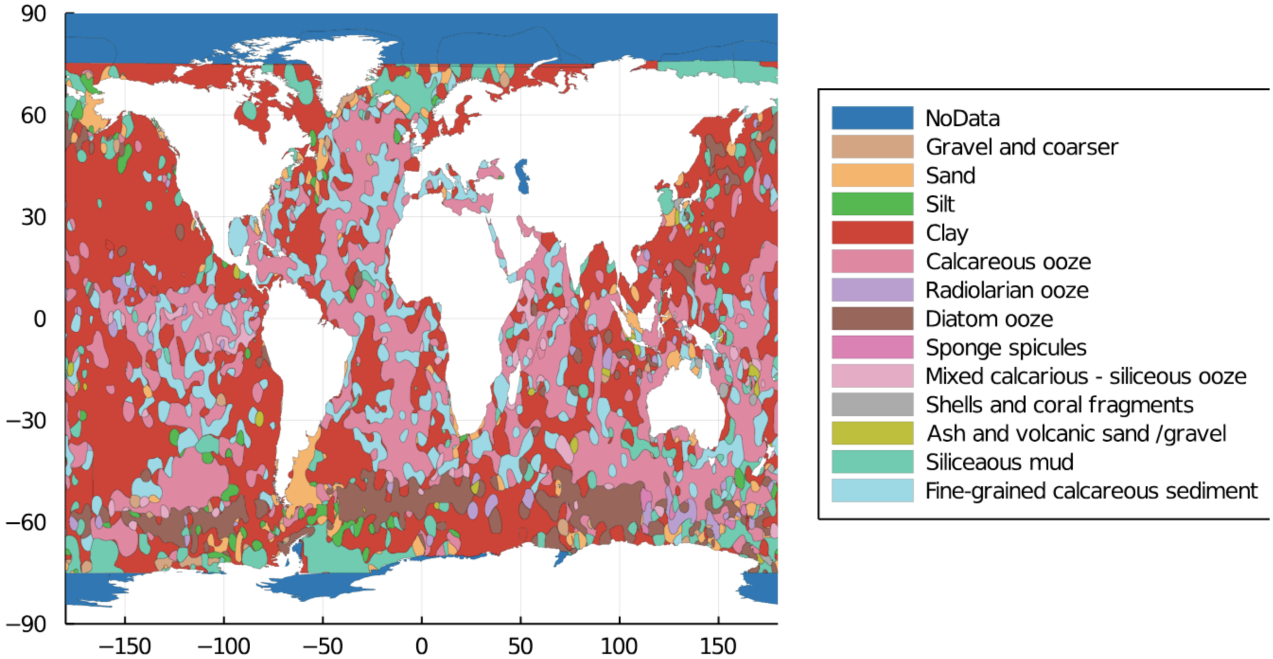
> kg m^{-3} yr^{-1}.

This begs the question: Is the equation correct?

- L396: Side note (not necessary for this manuscript, but could be a nice upstream fix): In *Pasquier et al.* (2022), one of the reasons for capping the north Pacific values of sedimentary ε_{Nd} was because it appeared as if the source dataset from *Robinson et al.* (2021) had used disconnected seafloor areas during production, with a particularly visible jump along the 180° meridian. Another oddly aligned frontier also appeared in the South Pacific around 165°W:



These disconnected areas probably originated from the lithology type dataset used:



It would be oddly coincidental for those lithology areas to have frontiers that coincide with meridians by chance. Maybe these areas could be fused back and the $\varepsilon_{\mathrm{Nd}}$ seafloor dataset updated? (This is not a big critique by any means and I would like to emphatically commend the authors for making such a map/dataset available in the first place!)

- Eqs. (10) and (11) typo: It should be either

> $\exp((z - z_{\mathrm{eu}}) / \ldots)$

or

> $\exp((z_{\mathrm{eu}} - z) / \ldots)$

depending on the vertical axis (z) orientation.

- Fig. 7: Colorbar units should be all upright (some are italic for some reason).
- Eqs. (12) and (14): The sum should be indexing over χ instead of i.
- L479 seems to start a new sentence right after the equation but does not. It is also unclear how $[Nd]_p/[Nd]_d$ is a tunable parameter. (It does not explicitly appear in Eq. (15).) Maybe this is an equation typo? Unsure what fix the authors would want.
- L480: It took me a while to realize that the authors have used p instead of more usual ρ (Greek rho) for seawater density. Could they replace p with ρ?
- Eq. (17) Suggestion: Maybe the authors could also report RMSE (root mean square error, as done by *Sidall et al.* (2008) and *Pasquier et al.* (2022)) along MAE. (Also as a suggestion for the future work mentioned elsewhere: squared differences, like the mean square error (MSE), generally work well as the objective function for optimization routines, owing to their quadratic shape.)
- L563: missing minus in exponent: yr^{-1} instead of yr^1.
- L564 and throughout: Notation suggestion: Probably clearer to write

  > (1.5–6) × 10^9 g yr^{-1}

  than

  > 1.5 × 10^9–6.0 × 10^9 g yr^{-1}

  but again,

  > (1.5–6) Gg yr^{-1}

  would be even better in my opinion.
- L575: I am likely wrong but I am unconvinced that all (any?) of the experiments fit that criterion. Back of the envelope calculation means a $(0.0025\% / 100yr)^{-1}$ = 4Myr stability timescale for the global mean [Nd] tendency. Maybe Figure 9 could also show the (centennial) tendencies of the mean [Nd], and prove me wrong (see Fig. 9 point 2 below).
- Fig. 9:
  - Maybe a y-axis log scale instead of the broken axis?
  - Maybe plot the tendencies in a separate panel below? It is sort of expected that the global inventory scales inversely with the scavenging strength. Therefore the only new information I am looking for at a glance in Fig. 9 is how quickly the system equilibrates. But then plotting the tendencies directly would be more straight to the point.
- Table 4:
  - would benefit from a smaller font.
  - The residence time of the first row (EXPT_RS1; 3037yr) does not match the formula:

    > residence time = Nd inventory/total Nd flux

- A suggestion: Move the columns for flux, inventory, and age to Table 3, and turn the "mismatch" columns into plots. Better would be detailed scatter plots of every model vs observation data point, for [Nd] and ε_{Nd} (it can be a simple scatter with transparency or, even better in my opinion, a joint distribution density plot as was done in, e.g., Fig. 7 of *Pasquier et al*, 2022). I suggest this because only the "mismatch" columns (the last four) are conveying new information while the other columns are either constant, redundant (with Table 3 or Fig.9), or simple divisions (the residence time formula).

- L626:

  > ~~demonstrates~~ illustrates?

- L628: What about:

  > ~~the efficiency of vertical cycling~~ the scavenging efficiency

- Figure 10: These are not

  > Global volume-weighted distributions of [Nd] (left) and ε_{Nd} (right) (...) split into four different depth bins (...)

  Instead, these are maps of

  > [Nd] (left) and ε_{Nd} (right) (...) vertically averaged over four different depth ranges

- L652–661: What about too strong a sedimentary source? While I agree with all the potential caveats listed in this paragraph, the authors should clarify why they don't consider an overestimate of the benthic source as the potential culprit for an overestimate of deep Nd.

- L662–668: Conversely to the preceding paragraph/point, my first impression is that a potential culprit is not discussed: What about too-weak surface sources? The simulated surface [Nd] underestimates observations beyond the coasts, particularly in the Atlantic (visible in the surface map of Fig. 9 but also in the profiles of Fig. S7). Larger surface point sources combined with a slower scavenging scheme can supply this missing surface Atlantic Nd. But a stronger dust source can, too. (That is what happens in our preliminary parameter space optimization in *Pasquier et al.* (2022): the dust solubility parameter is increased —to unrealistic levels— to better fit the observations.) Otherwise, could it be too small a (vertical) supply by the ocean circulation model? Maybe the authors can discuss these hypotheses (and rule them out)?

- L675 ε_{Nd} should not denote both the value and the unit. Thus, for consistency, I would remove it there:

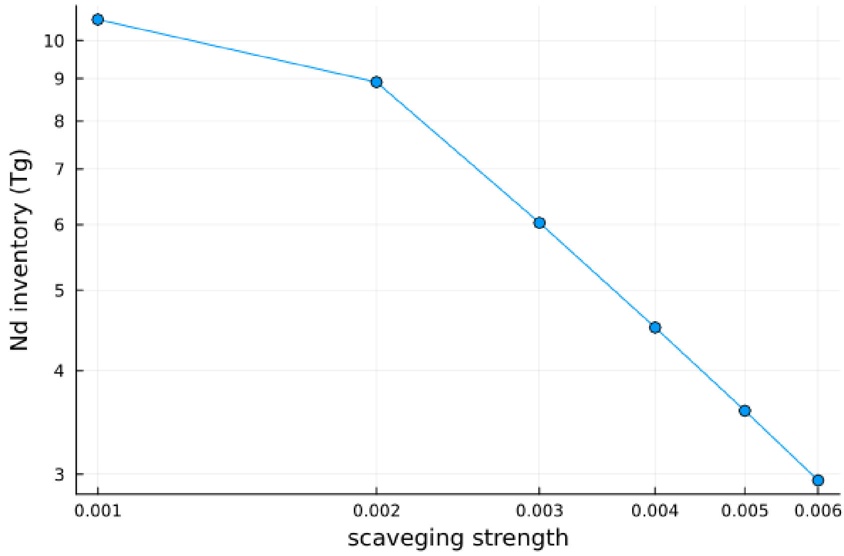
  > closer to -1 $\cancel{\varepsilon_{Nd}}$.

- L687–689: Maybe I missed this: Could it again be a case of surface sources instead? In the Pacific, it is not only that the ε_{Nd} values are too low, but the vertical [Nd] profile also suggests a lack of surface-originating Nd (Fig. S7). Maybe this is another manifestation of too strong scavenging near pointwise sources near the coast (a large number of observations in the North West Pacific make it hard to see the simulated field underneath)? Or maybe the model is missing a radiogenic Pacific surface source?

- L690: I am probably missing something here, but

> Simulated $[Nd]_d$ depth profiles in all the reversible scavenging sensitivity experiments (Fig. 11) generally (though not always) exhibit similar depth profiles to the observational data

seems like an impossible achievement. The Nd inventory precisely scales inversely with the scavenging strength (data from Table 4):



Although there are some variations in the spatial distributions, [Nd] generally does the same. This means if experiment A "exhibits similar depth profiles to the observational data", then the other experiments cannot all also match the data. Could the authors rephrase this paragraph so that it is clear what is similar? (It cannot be the profiles!)

- L708: What about using "suggests" instead of "demonstrates"? (Some, like me, usually assume "demonstrates" means "proves".)

- L716–725: What about a mention of the fact that increased scavenging efficiency, which means more local trapping of Nd, also means inter-basin separation? That is, the inter-basin ε_{Nd} gradients are favored by strong scavenging and a short residence time, as confirmed by the relationship between MAE(ε) and scavenging strength.

- L728: Where is "here"?

- L818:

> By year 6,000 all f_{sed} sensitivity experiments have reached steady state (< 0.0025 % change per 100 years).

Is this correct? Looking at EXP_SED4 around 6000yr still shows a slope

that I estimate to be a change of about 0.5% over 1000yr, i.e., 0.05% / 100yr, which is 20 times more than the advertised threshold. At this stage, I am sure I am missing something therefore I hope that the authors can clarify this! I would again suggest a semilog plot of the total Nd inventory tendencies to accompany the existing plot.

- L837: What about:

> varying f_{sed} drives relatively ~~discrete~~ ***small*** changes in Nd ***spatial*** distributions