Reply to reviewers' comments: egusphere-2022-606

S Robinson *et al*.

Correspondence: ee14s2r@leeds.ac.uk

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

Summary of Changes

Blue text below is our response to the reviewer's comments (reproduced in black). Line numbers refer to the tracked-changes version of the manuscript.

Response to reviewer 1: 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 resultsof 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.

We thank the reviewer for his positive comments recognising the genuine value of our work.

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.

We have revised the manuscript to address this point in accord with the detailed suggestions below, and we thank the reviewer for his thorough comments.

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 RMSerrors of less than about 0.2°C and 0.05 PSU (i.e., about 10–20 times better on that specific metric).

Done: we have added the following text to section 2.2, lines 284-288 to add more context to this evaluation, including a comparison to examples of other models whose data are available to us, namely HadCM3 and MIROC. We would not expect FAMOUS to outperform these two models since it is tuned to HadCM3, and both HadCM3 and MIROC4m have slightly higher complexity and higher resolution. However, this comparison demonstrates that the performance of the *control* simulation is comparable to similar/higher-order models, indicating appropriate model skill, which is useful context for understanding the limitations and advantages of the current study given our pragmatic choice to undertake the Nd isotope scheme development with a fast GCM.

We have not added the HadCM3 and MIROC data to Fig. 1 as we prefer to leave that figure focused on the new model results.

We have also added clarification to the model description (2.1) that HadCM3 is the parent model of FAMOUS, including line 235-237: '*FAMOUS is calibrated to the performance of HadCM3, taking the philosophy that this is the most appropriate evaluation target and it is unrealistic to expect the lower resolution, lower complexity model to out-perform its parent model* (Valdes et al., 2017).

- 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 $\delta^{14}C$).

Having considered it carefully, we have not adopted this suggestion because the control simulations are physically quite different between our simulations, and those of Dentith et al. (2019) and Dentith (2020), who untertook the C-isotope work. The earlier study highlighted that one of the main limitations to FAMOUS's ability to reproduce measured carbon-isotope ratios in the ocean was the over-deep North Atlantic Deep-Water formation and circulation and lack of Southern-sourced water in the abyssal North Atlantic, hence, we adopted a new control configuration. The ocean structure (including AMOC) is very different in the different studies and therefore a comparison to the previous C-isotope results would not be appropriate. To include the C-isotopes in our new simulations would require many months of further model integration time and a large volume of additional analysis, and we do believe there is value in presenting the Nd implementation documented here as a standalone piece of work, also considering that the journals focus is on the description of model development.

- "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)

Done: we have revised the text (section 3.1 line 986-1002) to emphasise the explicit caveat that our experiment design assumes a dominant sediment source based on suggestions that the seafloor sediment is the 'missing' (approx. 90%) Nd source (Tachikawa et al., 2003; Rempfer et al., 2011; Gu et al., 2019; Arsouze et al., 2007, 2009) and the more recent evidence that this is mostly coming out from abyssal seafloor sediment (Abbott et al., 2015b, a, 2019; Pöppelmeier et al., 2020; Deng et al., 2022). Our experiment design is specifically geared towards facilitating a discussion within the Nd community on the appropriate emphasis to place on a benthic flux for solving the Nd paradox, since we know this is an area for intense debate.

To explore more thoroughly the 'top down' versus 'bottom up' paradigm would indeed make a useful area for additional study, though it is beyond the scope of the presented work, since we primarily aim here to present the new version of an Nd isotope enabled FAMOUS and explore the sensitivity of the two main parameters/processes that are currently thought to govern marine Nd cycling and yet have only poor constraints. This is a good opportunity to highlight our companion paper to this manuscript (in discussion: https://egusphere.copernicus.org/preprints/2022/egusphere-2022-937/), where we present an optimized version of the Nd isotope scheme. Exploring the difference in a top down vs bottom up is something to achieve with this optimised model, to extend the work presented in the companion paper, which already begins down that path by assessing the margin vs benthic flux.

(Note, in response to another reviewer's comment, we have rephrased the way we refer to the 'top down' vs 'bottom up' paradigm in this manuscript).

- 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 "× 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.)

Done: units were presented in conventions similar to previous Nd isotope implementation in GCMs. However, we are happy to update this to Gg yr^{-1} for easier reading and have made this change throughout, including figures – maybe it will catch on!

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.

Thank you for highlighting this.

- L47 (missing "in"):

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

Done.

- 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).

Done.

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

Done.

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

Done.

- 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?

No problem! The choice for scaling Nd fluxes in the code was done to make the amounts of Nd in the model easier for humans to work with due to the very small numbers involved in simulating a trace element in a global model. Such scaling is often done (e.g., salinity and reporting values such as ε_{Nd}). We have edited the text (line 336-339) to make this clearer.

- 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?

Done (yr on the same line and updated to Gg and Tg).

- 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}"?

Done.

- L284–286: It is unclear how the additional constraints on the aeolian ε_{Nd} are applied. It is probably worth expanding/detailing.

Done.

- Fig. 5:

- Panel a: This filled contour map essentially looks bicolor to me. Could a log scale be applied tothe colormap to distinguish different river discharge strengths?

Done.

- 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.

Done: figure caption updated to clarify this: 'Figure 5: (a) Simulated river outflow (RIVER) in FAMOUS, (b) major river ε_{Nd}, (c) major river [Nd], and (d) the resulting riverine Nd source. The coastal grids in (b) and (c) are prescribed following average [Nd] and ε_{Nd} estimates of dissolved river runoff to each of the oceans by Goldstein and Jacobsen (1987; see Table 3).'

- Panel d (missing): Could the authors add a map of the resulting riverine Nd source?

Done.

- Table 1 + Eq. (7): S_{river} units issue. Substituting the units from Table 1 into Eq. (7) yields

  > m^2 yr^{-1}

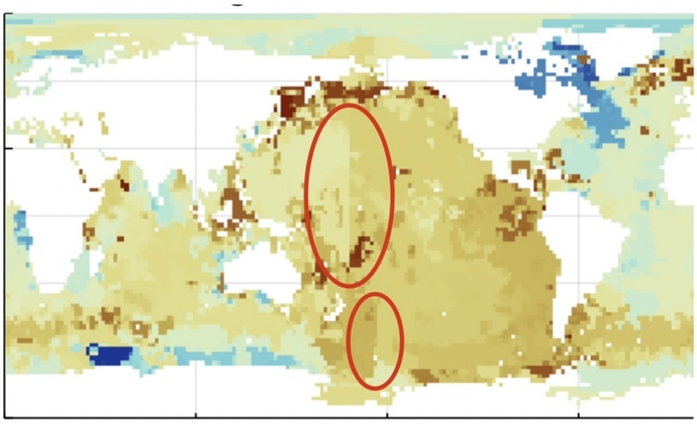
  instead of

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

  This begs the question: Is the equation correct?

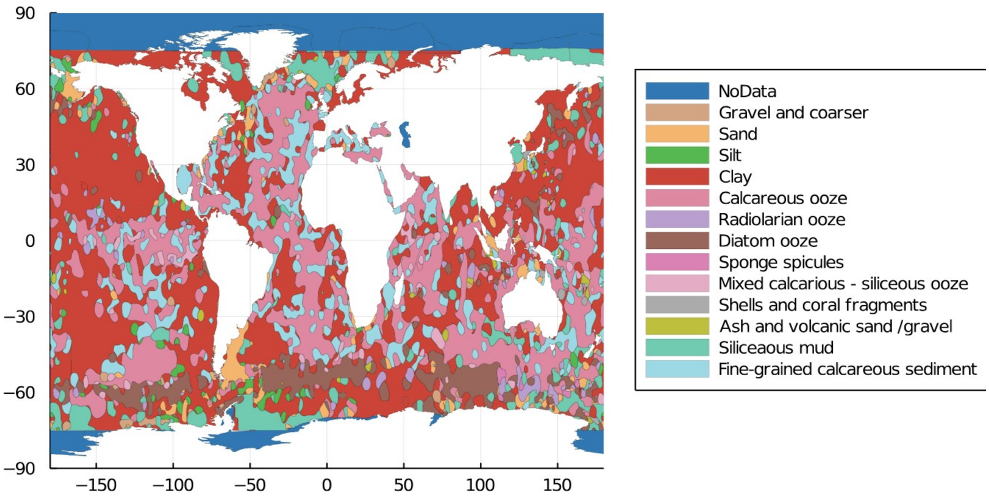
Done: we corrected the RIVER units in Table 1 to g m^{-2} yr^{-1}, and the intext units for the source from the river from kg m^{-3} yr^{-1}. This is the correct unit as used in the code, and yields the correct units for Eq.(7).

- 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 meridiansby chance. Maybe these areas could be fused back and the ε_{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!)

> We take the opportunity to respond to this side-note because it is an interesting discussion, but please note that we do not revise the manuscript, or benthic boundary condition, in light of this comment because it mainly pertains to the previous work. When designing the simulations presented here, we thought about the points the reviewer now raises carefully and decided against making further tweaks to the published seafloor dataset. For some background on the methods for that previous paper and to explain our decision not to smooth over this feature at the date-line: the artificial disconnect across the meridian (and the South Pacific) is an artifact of the high-resolution gridded map characterising the major lithologies of seafloor sediments in the world's ocean basins (Dutkiewicz et al., 2015; see 'Seafloor Lithology Map' in Data Availability), which was used to constrain the interpolation of discrete detrital and pore water measurements to create the seafloor ε_{Nd} maps. Here, we adopted the assumption that dominant seafloor lithology types at least partially describe the major sedimentary source and characteristics of detrital ε_{Nd}. This lithology map was, at the time of the paper, the most up to date representation of seafloor lithology. However, limitations of the seafloor lithology map included missing coverage in the polar Arctic region and a disconnect across the meridian. In order to create seafloor ε_{Nd} maps, and facilitate new schemes testing a global benthic flux, an ε_{Nd} signature needed to be assigned to all depositional sedimentary environments. We therefore had to make pragmatic (and often difficult) choices in order to best represent the ε_{Nd} distributions in abyssal seafloor regions with vast areas of no data and factor in boundaries of the map. In the previous work, we went some way towards correcting for these discontinuities in the gridded lithology file around the international date line using manual adjustments in the coastal areas of the Ross Sea (see supplementary: C10 and SF18) and the east Bering Sea (SF4). However, the Pacific seafloor, covering such a vast area but with limited measurements, proved the most challenging region to represent. To 'manually adjust' to correct for this disconnect across the Pacific would have meant either ignoring lithological bounds from the seafloor lithology map and applying a single mean ε_{Nd} across the whole of the abyssal Pacific, which arguably would have imposed just as arbitrary value as using the lithology bounds. As such, and in the interest of transparency, we chose for this first evolution of the seafloor ε_{Nd} map to minimise the manual-tuning. Most importantly, we hope this data driven and easily reproducible map provides a blue-print for how to [re]make the map with new data, and we provide as much information as possible so that every user can apply their own preferred assumptions and adjustments. We particularly highlighted outstanding questions over labile benthic fluxes and we hope that a future influx of seafloor detrital and importantly pore water ε_{Nd} measurements from GEOTRACES as well as from other programs and the wider community will help feed in knowledge to revise the map in a second version. This updated knowledge of the benthic flux would then go hand in hand with a future update to revise the seafloor lithology map, including correcting for the arbitrary bounds across the meridian line.

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

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

or

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exp((z_eu - z) / ...)
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depending on the vertical axis (z) orientation.

Done: corrected the typos in Eq.(10) and (11) corrected to $\exp((z_{eu} - z) / ...)$: thank you.

- Fig. 7: Colorbar units should be all upright (some are italic for some reason).

Done: italics removed for consistency. However, we prefer to keep the colourbar units horizontal and we think this layout is sufficiently clear.

- Eqs. (12) and (14): The sum should be indexing over χ instead of i.

Done.

- 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.

Done: we have updated the description of Eq.(15), which removed the incomplete sentence after the equation. We have also added detail in the text below Table 2 to explain the assumption that $[Nd]_d$ and $[Nd]_p$ are in equilibrium and defined the parameter which describes the ratio ($[Nd]_p/[Nd]_d$), that, based upon these assumptions, is the same irrespective of particle type and determines the scavenging efficiency in the model.

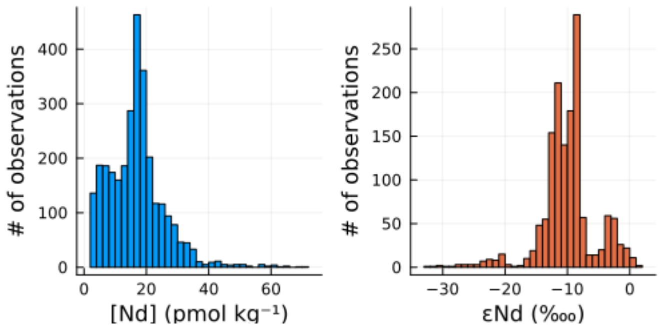
- L480: It took me a while to realize that the authors have used p instead of more usual ϱ (Greek rho)for seawater density. Could they replace p with ϱ?

Done.

- 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.)

  I just stumbled upon this GMD highlight paper on MAE vs RMSE that the authors may find useful: Hodson, T. O.: Root-mean-square error (RMSE) or mean absolute error (MAE): when to use them or not, Geosci. Model Dev., 15, 5481–5487, https://doi.org/10.5194/gmd-15-5481-2022, 2022.

  My understanding from that paper is that MAE should be used for [Nd] (exponentially distributed) and RMSE should be used for εNd (normally distributed). I would still recommend reporting both MAE and RMSE however, to facilitate comparisons with past and future models, and also because the distribution assumptions are not exactly satisfied with the GEOTRACES IDP21 data:



Done: We now report the RMSE (Eq. 18) in Table 3 alongside MAE.

For some context, in the study we considered whether to report MAE or RMSE by exploring the pros and cons of each global metric. The study focusses on the new Nd isotope scheme in FAMOUS and exploring how the simulated distributions are influenced by systematically varying model parameters (reversible scavenging efficiency and the magnitude of the sediment flux), leaving the optimisation of the scheme for future work. As such, we wanted the global performance metric to be used as a quick indicator of model skill, and for comparison with other schemes. In this study, we wanted to focus more on the detailed exploration of the spatial distributions of [Nd] and ε_{Nd} to investigate what the scheme (and our understanding and assumptions about the marine Nd cycle) is currently representing, what it is not capturing, and what this can tell us about the large questions surrounding the marine Nd cycling.

RMSE penalises outliers more (it gives a relatively high weight to large errors and means the RMSE should be more useful when large errors are particularly undesirable), MAE is a linear score which means that all the individual differences are weighted equally in the average. We considered the physical biases in the model, model resolution and the resolution of boundary conditions, assumptions of the scheme and our priority in producing a scheme that represents broadly the global marine Nd cycle (which will not represent highly localised and often more extreme features), and from this, how we value/penalise model errors. Our choice for MAE was based upon the following:

- If the scheme did not capture, for example, a very radiogenic value from a localized seawater sample taken near a volcanic region with an ε_{Nd} of +10, but the surrounding sediment and seawater has a value of -7, then this model-data mismatch would be penalised heavily in RMSE, despite the model representing the general/largescale ε_{Nd} distributions
- RMSE has a tendency to be increasingly larger than MAE as the test sample size increases- this can be problematic when comparing RMSE results calculated on different sized test samples, which is the case for comparing global Nd isotope schemes using different observational databases for validation.
- MAE, as a very simple global model skill metric, is reported in previous Nd isotope schemes in GCMs (Rempfer et al., 2011; Gu et al. 2017, Poppelmier et al. 2020), so we wanted to report our values in a way that could be quickly compared to other schemes.

The plot showing the distribution of [Nd] and ε_{Nd} measurements is very useful for visualising the distributions, and we agree it is insightful, in this instance, to report both values as a combination of simple global metrics to explore model performance, and both metrics give a quick insight into model performance. We note that the cited paper suggests that RMSE is optimal for normally distributed errors (yet the distribution of ε_{Nd} shows a skew, and so cannot be assumed to be fully under a normal distribution, and that MAE does not only apply to uniformly distributed errors. The paper also suggests that MAE is more robust, yet argues there are better metrics. Overall, we consider our choice for MAE is indeed a reasonable metric.

To echo the issues highlighted by the reviewer, we propose that it would be very useful for future model-intercomparison efforts and isotope scheme development/optimisations to have a broad discussion across the community with aim of establishing best practice for model performance metrics and reporting model skill (including discussing the nuances of how applying different metrics may be more useful for model skill in [Nd] compared to ε_{Nd}).

- L563: missing minus in exponent: yr^{-1} instead of yr^{1}.

Done.

- L564 and throughout: Notation suggestion: Probably clearer to write

> $(1.5\text{-}6) \times 10^9$ g yr^{-1}

than

> $1.5 \times 10^9\text{-}6.0 \times 10^9$ g yr^{-1}

but again,

> (1.5-6) Gg yr^{-1}

would be even better in my opinion.

Done: Gg yr^{-1} notation adopted.

- 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).

Done: and we thank the reviewer for spotting this. We had actually calculated the mean % rate of change over the final 100 years. Accordingly, the table below shows the % rate of change in the final 100 years for each sensitivity experiment in the study. We have corrected the definition of equilibrium in the text, line 837: '<0.02 % change per 100 years (where simulations XPDAI, XPDAD and XPDAG) have not yet reached equilibrium'.

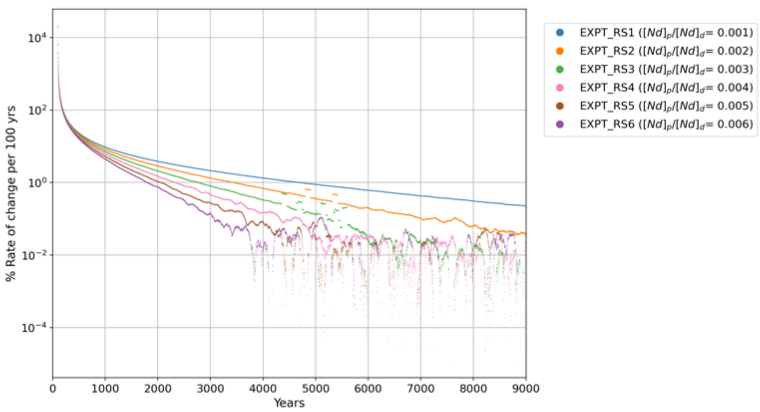
RS_TUNE	**% change in last 100 yrs**
xpdai	0.1631
xpdad	0.0223
xpdah	0.0100
xpdae	0.0055
xpdaf	0.0123
xpdag	0.0674
F_SED	**% change in last 100 yrs**
xpdal	0.0047
xpdam	0.0059
xpdah	0.0100
xpdan	0.0112

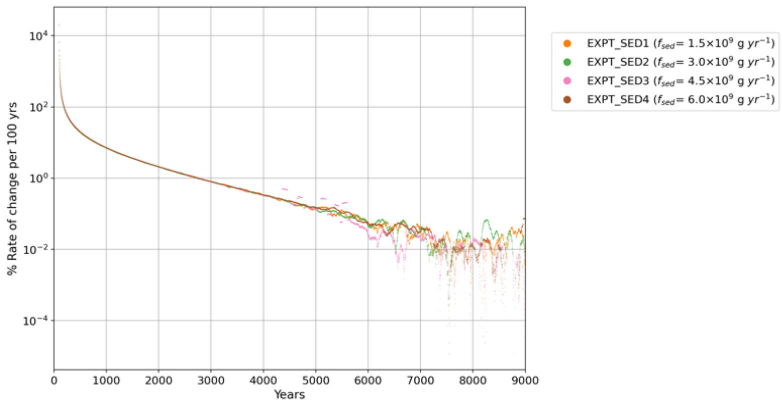
- Fig. 9:
  - Maybe a *y*-axis log scale instead of the broken axis?

Done.

  - 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.

Done: However, based on broad consultation, we think that the plot of global Nd inventory over time is the most intuitive to understand. We have therefore kept this as the figure in the main text, but have created (and now cite in the main text) supplementary figures of the Nd inventory rate of change over time for both ensembles of sensitivity simulations in the study (shown below), so that the detail of the temporal evolution can be examined directly by an interested reader. If the editor prefers differently, then we can add the supplementary plots of tendency into the main text.





- Table 4:
  - would benefit from a smaller font.

Done: see comment below regarding combining Tables 4 and 5 into Table 3.

  - The residence time of the first row (EXPT_RS1; 3037yr) does not match the formula:

> residence time = Nd inventory / total Nd flux

Done: Typo corrected in the table and text line 887 ([Nd] inventory for EXPT_RS1 is 16 Tg, yielding a residence time of 3036 years).

  - A suggestion: Move the columns for flux, inventory, and age to Table 3, Done: also combined with Tables 4 and 5 – see new Table 3 and note removal of old Table 4 and 5 to avoid repetition.

> 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 etal*, 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).

We understand this suggestion, but have chosen the original presentation very carefully for a number of reasons, which we explain here: this information as a global model-data skill indicator is included in the table for summary purposes (and e.g., comparison to other model studies). We specifically do not include the suggested scatter plots here because this presentation of the results hides the detail of the modeled spatial behavior compared to observations and can also contribute towards bias in model evaluation based on the density/sparsity distribution of observations, although we agree a joint distribution density plot goes some way to overcome this. Most importantly, we do not want to distract from the intended emphasis of *this* manuscript: to use sensitivity studies to explore what happens physically in the model when the two key parameters are changed in order to understand *how* and *where* they govern Nd distributions. This is a substantial undertaking and a first important step to robustly understanding our model behaviour before creating a model structure that produces the best match to observations – we don't want to 'jump the gun'. Now that we have understood the behaviors, our next paper presents an optimisation of the model scheme, and there we conduct a more detailed evaluation of model performance with respect to observed Nd distributions (https://egusphere.copernicus.org/preprints/2022/egusphere-2022-937/); we think it is less appropriate to do this in the uncalibrated version of the Nd scheme presented here.

- L626:

> ~~demonstrates~~ illustrates?

Done.

- L628: What about:

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

Done.

- 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

Done: updated Figure 10 and corresponding Figure 14 caption.

- 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.

Done: this point is important to highlight, and we have edited the text (see lines 986-1002) to include a wider discussion.

- 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)?

Done: examining these aspects is indeed a main motivation for our study and we have edited the text (lines 1028-1051) to include both discussion points.

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

> closer to -1 ~~ε_{Nd}~~.

Done.

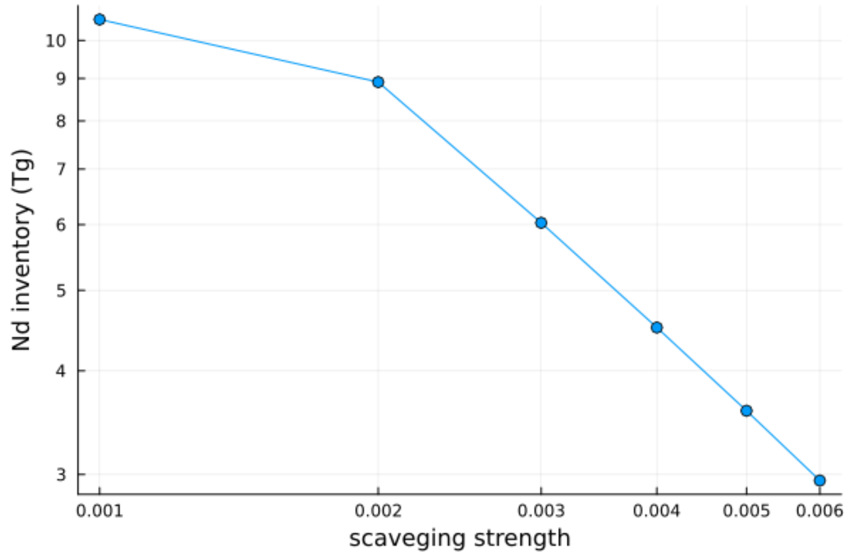
- L687–689: Maybe I missed this: Could it again be a case of surface sources instead? In the Pacific, itis not only that the ε_{Nd} values are too low, but the vertical [Nd] profile also suggests a lack of surface-originating Nd (Fig. S7). Maybe this is another manifestation of too strong scavenging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

Done: we have revised the text to include discussion of how surface and marginal sediment regions may pose a larger in magnitude and more distinct radiogenic Nd source into the Pacific compared to an open ocean abyssal benthic source. This related to a point raised by Ed Hathorne (addressed below), e.g., that red clays in the Pacific are likely large Nd sinks and so future evolution of the scheme could explore spatial variation in Nd fluxes from sediment regions.

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 isclear what is similar? (It cannot be the profiles!)

Done: when originally referring to the 'depth profiles' in Fig. 11, we were pointing towards the simulated $[Nd]_d$ with depth shown in the surrounding sub-panels at different locations. We have clarified this in the in text (line 1067).

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

Done.

- 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.

Done.

- L728: Where is "here"?

Done: we updated the sentence to explicitly point to the sub-tropical North Atlantic.

- 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 tendenciesto accompany the existing plot.

Done: see our earlier response regarding an updated definition of equilibrium calculation and revised calculations, our improved Fig. 9 (log-profile for Nd inventory on the y-axis) and the new supplementary figures showing the rate of change over time for the sediment flux sensitivity studies.

- L837: What about:

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

Done.