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 community comment: Tristan Vadsaria

The following comment of the preprint research article entitled "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" by Robinson et al. has been motivated by the next implementation of the Neodymium oceanic cycle in the *i*LOVECLIM model.
This article describes the modelling implementation of the Nd oceanic cycle in the fast global climate model (GCM) FAMOUS. After selecting the most appropriate reference simulation in terms of oceanic variable (temperature, salinity, AMOC pattern and strength), the authors scrutinized in detail the result of the implementation based on two decisive parameters. These parameters, being the scavenging coefficient and the Nd flux from the sediment, are known to be still poorly constrained by observational studies. The modelling work provided by Robinson et al., in line with the recent Nd modelling studies performed with GCM, helps to provide more insight on these processes with the "own" physics and parameterization of the FAMOUS model.

I found that the paper is overall very well written with good descriptions of the results which is very important to understand the relative importance of both the scavenging coefficient and the Nd flux from the sediment on the global oceanic distribution of Nd and εNd. I particularly like the effort put into reaching the most "appropriate" simulation regarding ocean physics in order to reduce the associated bias for the Nd and εNd interpretation. I also join the authors about the need of a shared protocol for an Nd modelling intercomparison project.
Below are some (minor) comments to 1) have some clarifications on some points of the manuscript, 2) suggest some modifications if it is feasible in time and in resources for the authors.
Because my field of expertise is mostly climate modelling, my comments will not focus on the geochemistry part of the paper.
Best regards,
Tristan Vadsaria

We really appreciate the time taken for the community comments given and the interest in the presented study.

River forcings
For this point I am not sure at 100% that my comment is pertinent, sorry if I misunderstood the manuscript in this regard. As far as I understood from other Nd modelers, the river forcings in terms of (dissolved) Nd concentration and εNd come indeed from Goldstein and Jacobsen (1987) but is often updated with recent observations such as coming from Blanchet (2019). As I assumed that the authors are using the most updated data to force the model, would it not be better to indicate this in the manuscript (especially for the caption of figure 5)?

We can clarify this here: in our presented scheme the riverine Nd source only refers to the dissolved riverine flux, and hence we used the estimated dissolved Nd concentration and isotope ratios from Goldstein and Jacobsen (1987). Recent schemes have updated the riverine source with particulate riverine samples from Blanchet (2019) and Robinson et al (2021). However, as we explain in lines 471-476, the ε_{Nd} of dissolved and particulate river inputs is highly variable, and combining them is non-trivial. We therefore chose to keep the dissolved riverine source separate, and the sediment flux imposed (which includes the most recent compilation of published ε_{Nd} from river sediment samples deposited on the continental shelf and slope) encompasses at least in part the ε_{Nd} from a river particulate flux.

What about the seafloor Nd concentration forcing?
This comment is not really a suggestion nor a critic since I think it's beyond the scope of the study but rather an open question, I think it would need more discussion from and for the Nd modelling community.
For the Nd sediment source, I understand that this study is in line with the previous scheme initiated by Rempfer et al. (2011) that "[…]do not make any assumption regarding the nature of the Nd boundary source" and "[…]do not assume spatial variation...", later followed by Gu et al. (2019) with the same depth limitation and also by Pöppelmeier et al. (2020) but without the depth limitation. This approach is indeed convenient for tuning "fsed" which is today still not very well known. However, how far is it reasonable to apply the same scheme while considering the whole seafloor, i.e., to not consider the spatial Nd concentration of the seafloor into the calculation of the sediment source (question also valid for Pöppelmeier et al., 2020)?

While the Nd sediment source was "confined" to the continental margin in Rempfer et al. (2011) it seemed more "reasonable" to make their assumptions especially regarding the horizontal resolution of the Bern3D model. However, now, without the depth limitation, I would guess that the spatial variations of the Nd concentration of the seafloor would have an impact on the deep and bottom Nd dissolved seawater distribution, don't the authors think so?
In Arsouze et al. (2009), "Fsed is then determined for both ^{143}Nd and ^{144}Nd isotopes by multiplying this sediment flux to the concentration along the margin" and they fixed the sediment flux to only one value, without the ability to make a lot of simulation to tune this flux because of the resolution and the time consumption of their model. My question is: would it be possible to have an intermediate approach between Rempfer et al. (2011) and Arsouze et al. (2009), e. g., tuning the flux while applying widely the Nd seafloor concentration (obtained from the recent data of Robinson et al. 2021 for instance)?

Small edit: As I kept thinking about the previous point, I realized that Nd seafloor data was indeed scarcer than εNd and that extrapolating a wide Nd seafloor map would be less relevant especially in the Pacific Ocean (cf attached figure in supplement using Nd data provided by Robinson et al., 2021). Anyway, would it be possible to imagine a regional seafloor Nd (or basin-scale) signal such as used for the dust εNd?

These are interesting discussion points that do warrant thorough discussion in the community, so we are glad that our manuscript has people talking about this.

First, we return to the start of the comment. Our study primarily aimed to present the new Nd isotope scheme in FAMOUS and explore key model parameters under the context of our current knowledge of marine Nd cycling. The reasoning for the globally uniform sediment flux applied across the seafloor was to explore the emerging benthic flux hypotheses that suggests that a widespread benthic Nd flux, of similar flux magnitudes, and across diverse sedimentary environments may dominate the marine Nd cycle. In this study we wanted to focus on where this is and where this isn't the case before making assumptions on what environmental conditions drive spatially diverse fluxes, especially since pore water data is currently scarce. We think that this is an important first step towards tackling the big open questions that the comment poses.

In response here, and to another reviewer's comments, we have added more discussion in the manuscript surrounding the spatial variability of elevated Nd fluxes and regions that may act as Nd sinks.

To explore in more detail the spatial variability in Nd sediment sources, although out of the remit of this study (as highlighted in the comment), will be very useful and we agree that greater community focus on describing the environmental regions which drive elevated benthic fluxes is necessary to achieve this. A regional mask to explore elevated benthic fluxes is a great suggestion for future work, for example building upon the work in Pöppelmeier et al. (2020) where core top-bottom water offsets were used to create a simple regional elevated benthic flux map.

Less sensitive response of Pacific εNd to reversible scavenging efficiency - deep seafloor wide sediment source

Concerning that point, the authors said that their results "[...] contrast with results from Rempfer et al. (2011) ..." (line 751) and "attribute this difference primarily to the spatial variation in the sediment Nd flux" (lines 752-753) due to the different modelling scheme used for the Nd sediment source.

Even though that explanation seems obvious, I would like to see a simulation output to confirm what the authors are suggesting (i.e., simulations governed by deep seafloor wide sediment source) with the "own" FAMOUS ocean dynamics: A set of simulations similar to the scavenging coefficient sensitivity simulations (first part of the result) but with the "initial" depth limitation of 3000km would be the best (but maybe too much for that purpose). What about retaining the best simulation ("EXPT_RS4" as far as I understood) for the scavenging coefficient and run a parallel simulation with the depth limitation of 3000km for the sediment source? I think that putting only the result (a couple of 2d maps) of this new simulation in the supplementary compared with "EXPT_RS4", while keeping the original text in the main paper would be enough. Taking this comment into consideration is obviously up to the authors regarding its feasibility.

We highlight our companion paper to this manuscript (in discussion: https://egusphere.copernicus.org/preprints/2022/egusphere-2022-937/), where we first present an optimized version of the Nd isotope scheme. In this companion paper, which builds on the present manuscript, we begin to explore spatial variations in our optimised scheme (rather than in EXPT_RS4) by assessing the 'margin' vs 'benthic flux' as suggested in the comment.

Wrong residence time value?
As explained in the main manuscript, the residence time is equal to the Nd inventory divided by the total Nd flux (line 598). Following table 4 (and 5), it corresponds to (column 5*1000/column 4). If I apply this, I found the same results as the authors for all the experiments except for "EXPT_RS1" which should be (10.6*1000/5.27) = 2011 years, am I right? Anyway, it does not change anything to the results description and the conclusion since it is still the simulation with the highest residence time.

Done: typo corrected in the table and text ([Nd] inventory for EXPT_RS1 is 16 Tg, yielding a residence time of 3036 years. Thank you!

Very minor suggestion (1)
"The total global flux of river sourced dissolved Nd to seawater (friver) is 4.4 $\times 10^8$g(Nd) yr^{-1}": this is the value that comes from the simulated runoff in FAMOUS combined with the prescribed Nd river concentration, isn't it (as confirmed by looking at Table1)? In that case, I would suggest adding "in the model" to the sentence to enhance clarity.

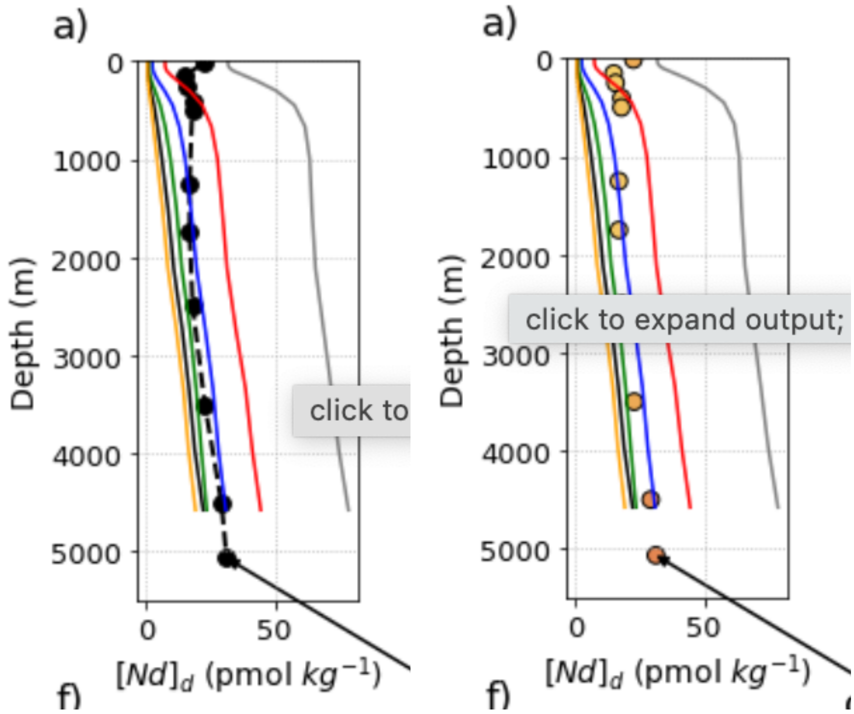
Done.

Very minor suggestion (2)
In my opinion, the use of "pronounced" in line 642, 894 and 992 to describe the behavior of the vertical gradient of [Nd]d is not very descriptive, but I may be wrong since I am not a native English speaker.

Done. Updated throughout text to '*overestimate the vertical [Nd] gradient with depth, where simulated [Nd] is too low in the surface and too high at depth compared to seawater measurements.*'

Very minor suggestion (3)
Figures 11, 12, 15 and 16 are overall very nice but I would suggest, regarding the depth profiles, for more clarity and visibility, to not match the color of the observational data with the color bar of the central 2d map. Maybe rather use a unique color, also with the dots connected together, to reduce the confusion with the color of the simulated profiles (but at this stage it's really a matter of taste).

These plots display a lot of information of the spatial and vertical distributions of simulated and measured Nd. We explored updating the plot to have a single colour and joined scatter (see left panel in the plots below) for the discrete observational data, but find the extra detail makes the plot harder to read compared to our original (see right panel). We have chosen not to update the plots in this instance. Furthermore, although the modelled data can be depicted as continuous profiles we did not want to impose a linear fit between the discrete measured data points especially for ε_{Nd}.



Very minor suggestion (4)
Why not merge Table 2 and Table S3? I think that Table S3 would be very informative in the main text.

Done.