

Biogeosciences Discuss., referee comment RC1
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Comment on bg-2022-107

Anonymous Referee #1

Referee comment on "Neodymium budget in the Mediterranean Sea: evaluating the role of atmospheric dusts using a high-resolution dynamical-biogeochemical model" by Mohamed Ayache et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2022-107-RC1>, 2022

Ayache et al. present a revised implementation of the marine neodymium cycle in the high-resolution regional ocean model NEMO-PISCES for the Mediterranean Sea that forms the natural extension of previous work with less complex implementations. The authors now consider sediments, rivers, and dust as sources of Nd, and they assess their respective contribution and impacts via a number of sensitivity experiments. Similar to other recent studies, Ayache et al. come to the conclusion that sediments are the major source of Nd to the oceans also in the Mediterranean Sea. They further find that despite contributing only ~5% to the total Nd flux, dust plays a critical role for surface and intermediate Nd concentrations and isotopic compositions substantially improving the mismatch to observations.

Overall, the manuscript is well-written, but the figures require some improvements. The findings are in agreement with previous studies, and the new implementation in such a high-resolution regional ocean model will surely be a good test-bed for future investigations further elucidating the marine Nd cycle. However, some aspects remain unclear and require clarification as I outline below.

Main points

I think it would be helpful to give a brief description of the modern/simulated circulation in the Mediterranean Sea in the introduction, as not everyone may be familiar with the details of it. This would further allow for a more robust assessment of the applicability of Nd isotopes as faithful water mass tracer in the Mediterranean Sea later on that is currently missing.

Why was no tuning of the Nd cycle parameters performed for this study, not even of unconstrained parameters of internal cycling? If this is too computationally expensive, this should be mentioned in the text also highlighting the downsides of an “un-tuned” Nd module with partly only poorly constrained parameters.

There appears to be no impact of riverine Nd to the Nd concentrations even at the surface. Even though in the text it is mentioned that there are small differences in the catchment areas, they are not visible in Fig. 3. I would have expected at least a visible difference close to the estuary of the Nile river. Since previous studies described the Nile as an important Nd source, I think the lack of a clear imprint thereof warrants a more detailed discussion of this.

Why does the dust flux generate a subsurface Nd maximum? If it is related to the subsurface production than this subsurface maximum should also emerge for the other simulations. Further, in line 283 it is mentioned that this subsurface maximum of experiment SedRivDust is also found in the observations, however, the depth profiles of Fig. 5 show a rather constant vertical profile with no pronounced maximum in the subsurface. In addition, as mentioned in line 359, it is paradoxical that such a small Nd flux by dust can increase the Nd concentration by so much. This paradox is not resolved in the text, and requires more in-depth investigation due to its great impact on the Nd cycle. In particular, it is very surprising to me that the riverine flux has virtually no impact on the surface and intermediate Nd concentrations, while the dust flux has a very large impact, while both sources are similar in magnitude (3.7% versus 5.3%).

It is difficult to assess the agreement between simulations and observations based on the figures alone. It would therefore be helpful to also provide a more objective measure, such as the root mean squared error, mean absolute error or another appropriate metric.

Specific points

L26: I think it would be good to also cite more recent modeling work in this context (e.g., Gu et al., 2019; Pöppelmeier et al., 2022; Pasquier et al., 2022).

L64: I believe you are referring to Pöppelmeier et al. (2020), not Pöppelmeier et al. (2019).

L75: Rempfer et al. (2011) and Gu et al. (2019) both also subtracted 70% of the total riverine Nd source not 30%.

L152: 'adsorption onto particles' not 'into particles'.

L161: Following not Flowing.

L162: Do you mean that [Nd] and ϵNd are calculated offline?

L163: Mass-dependent fractionation is corrected for during measurement anyway.

L185: Typo – 'the the'.

L200: The calculation of the sedimentary Nd flux remains unclear to me. How do you calculate the Nd flux from a bulk detrital concentration? Do you assume a constant and uniform dissolution rate? Since this source flux is so important for the Nd cycle, a more thorough explanation is required.

L228: Typos in references.

L240: Experiment not experience, here and elsewhere.

L 273: From Fig. 3 it appears that simulated Nd concentrations fit rather well to the observations in the deep layer and the factor of two difference mentioned in the text appears to be only present at intermediate depths.

L290/291: Remove 'globally' here and elsewhere, since you only consider the Mediterranean.

L 300: The deep layer looks pretty much the same for all three experiments in Figs. 5f and 6

L313/338: Experiment not experience.

L320. A brief comparison also to other global studies would be helpful to set these results into a better context.

L332: Strike 'compared o in-situ observations'.

L335-336: Maybe give river Nd concentrations also in pmol/kg for better comparability to dissolved ocean concentrations.

L360-363: I don't understand how a dust dissolution of 10% can lead to lower surface Nd concentration when the input Nd flux is five times higher. As mentioned in the text higher concentrations lead to more efficient scavenging, but since particle concentrations should remain the same between both experiments, the net effect on the dissolved Nd concentration should remain an increase not a decrease (albeit less than the factor of five).

Figs. 2, 5: Please use the Greek epsilon character in the figures.

Fig. 3: Please note whether these are averages over the mentioned depth intervals. Further, in the caption it is noted that the intermediate layer is from 250 to 600 m while in the text it is 200 to 600 m.

Figs. 3, 4: Please use the same colorbar for both figures to allow for better comparability.

Fig. 6: The tick-labels of the colorbar appear to be rounded thus missing the digit after the comma.

References

Gu, S. et al. Modeling Neodymium Isotopes in the Ocean Component of the Community Earth System Model (CESM1). *J. Adv. Model. Earth Syst.* 11, 624–640 (2019).

Pasquier, B. et al. GNOM v1.0: an optimized steady-state model of the modern marine neodymium cycle. *Geosci. Model Dev.* 15, 4625–4656 (2022).

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