

## Referee #1

I appreciate the new Fig. 6 that shows detailed eNd comparison between model and data for different basins. Considering this new observation, I would suggest that the authors revise the text corresponding to the performance of simulation. The data-model comparison rather indicate that the BE is not sufficient to reproduce the modern Mediterranean seawater eNd distribution. The decoupling is significant, not only for LIW but also in the Alboran Sea. I agreed with the authors' answer to my comments ("the LIW layer gave an isotopic signature of almost  $-7 \pm 1$ ; Tachikawa et al., 2004; Henry et al., 2004, and from P. Montagnia, in prep.") but the revised version is not always consistent with their answer.

I strongly recommend that the following parts would be modified before the final acceptance of this work.

P. 4, line 8 and throughout the text, as well as the figure caption. The compilation by Tachikawa et al. (2004) does not include Vance et al. (2004). This reference should be cited systematically for the data-model eNd comparison.

P. 4, line 21, "extrapolate". For the eNd compilation of margins, I expect that both extrapolation and interpolation were applied.

P.10, line 13, about correlation coefficient shown in Table 3. The coefficients shown in the text (0.71 and 0.61) do not correspond to the values indicated in Table 3. Please check and correct them.

P. 10, line 14, "dashed line" that is not shown in Fig. 4. I had already mentioned this point in my previous review. Please correct it.

P. 10, lines 15-16, "a slight overestimation of eNd between 0.3 and 1 eNd unites". According to the new Fig. 4b, the size of the offset should be larger (ex. the offset seems to be -2 at around 200m). Please check.

P. 10, line 18, "reasonable East-West gradient of eNd". What does "reasonable" mean? It is necessary to change this ambiguous expression.

P.11, lines 5-6. The eNd overestimation at the intermediate water depths is not limited for the Sicily Strait and Tyrrhenian sub-basins. The Alboran Sea presents a large offset (Fig. 6). Also "but the lack of observations prevent us to assess their consistency" is not appropriate and inconsistent with the authors answer ("the LIW layer gave an isotopic signature of almost  $-7 \pm 1$ ; Tachikawa et al., 2004; Henry et al., 2004, and from P. Montagnia, in prep.").

P.11, line 15. Please clarify "any specific isotopic signature".

P. 11, lines 20-22, "Overall the model capture correctly...". This part should be revised taking into account the observed offset between the simulated and measured seawater eNd distribution.

P. 12, lines 20-21, "The high resolution... in the Med Sea". This part should be nuanced.

P. 12, line 30, "especially in the EMed". The statement is not totally correct because of the strong offset in the Alboran Sea.

P. 13, lines 7-13. "The LIW layer is ...in the whole basin". This part should be modified as I suggest at the beginning of this review.

Figure 4. Label "a" and "b" is missing. On the x-axis of Fig. 4b, "epsilon" is not correctly shown.

Figure 5. I am not sure that data from Henry et al. (1994) and Vance et al. (2004) are

shown. Please check and correct, if necessary.

Figure 6. I would put the Alboran result on the left, the Levantine result in the middle because of their east-west position in the Mediterranean Sea. "Tachikawa et al., 1983" should be "Tachikawa et al., 2004". Add also the other references (ex. Vance et al., 2004; Henry et al., 1994) when necessary.

Figure 7a and 7b. The cyan curves show Levantine eNd variation, not "Aeg".

#### Reference

Vance, D., Scrivner, A. E., Benecy, P., Staubwasser, M., and Henderson, G. M.: The use of foraminifera as a record of the past neodymium isotope composition of seawater, *Paleoceanography*, 19, doi:10.1029/2003PA000957, 2004.

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#### Referee #2

##### General comments

This is a revised manuscript of Ayache et al. Along with the comments give by the referees, the authors seem to significantly improve the manuscript. I think, however, a couple of issues are still remained to be addressed before final publication.

##### Specific comments

After I read this manuscript, the following idea came to my mind: I wonder what is the aim of this study?

As written in the first sentence of discussion section, one important finding of this study is that the main features of the Nd IC distribution in the Mediterranean Sea are generated by assuming BE as the only Nd oceanic source term, which has been already demonstrated for the global ocean and the Atlantic basin. This fact confuses me a lot, because the authors have already found that their approach could be applied to much wider oceanic regions than the Mediterranean Sea. Furthermore, the Mediterranean Sea is a semi-closed basin and seems to be much easier system for modeling study than the global ocean. Therefore, I am not quite sure why they studied this oceanic region at this moment, which should have been done much earlier stage. I admit that more detailed and precise geological information might be available around the Mediterranean Sea than the global ocean, and this would lead to facilitate a more accurate simulation on Nd IC distribution. In reality, however, according to this manuscript, some problems (more radiogenic values in some areas etc.) still remain to be solved for a realistic simulation. Therefore, I recommend the authors to emphasize what is the merit to do modeling work for the Mediterranean Sea comparing with the global ocean. Otherwise, this paper would only deal with a case study of a limited oceanic region.

##### Technical corrections

P4 L28; What is "rive"? Please correct.

P7 L28; "(Ludwig et al., 2009)" should be "Ludwig et al. (2009)".

P14 L15; "Our next step is" seems to be much proper than "Our next step was".

Figs 4 and 5; Although the authors show new figures in the documents for replying referees' comments, those figures presented in the revised manuscript are the previous ones. Please replace to the new ones.