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Biogeosciences Discussions

Interactive comment on "Reconstructing the Nd oceanic cycle using a coupled dynamical – biogeochemical model" *by* T. Arsouze et al.

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Dear Dr. Joos,

First of all, we would like to thank you, for handling the editing process, and the three reviewers, for their useful comments, about our submitted manuscript "Reconstructing the Nd oceanic cycle using a coupled dynamical – biogeochemical model" by Arsouze T., Dutay, J.-C., Lacan, F. and Jeandel, C.

Sentences from reviewers start with the ' (RC) ' symbol (Reviewers Comments), whereas author's reply sentences start with the ' (AR) ' symbol (Author's Reply).

Please find joined with this present Reply to Reviewers, an annotated corrected version of the manuscript.

Referee #1: Mark Siddall

General Comments:

(RC) The English in this paper needs quite some work. Although the overall structure of the English is generally good there are many peculiar ways of saying things that make it hard to read in places and in other places the tone of the comments seems out of place. Please consult with a collaborator who is a native speaker to help sort this out.

(AR) The corrected version has been read and corrected by an English native speaker. We hope this helps making the manuscript more readable.

(RC) MISSING – most of the figures are given for both EpsNd and Nd concentration the horizontal maps in Figs. 7 and 8. These must be shown.

(AR) The two horizontal concentration maps have been added (Fig. 9 and 10).

(RC) MISSING – characteristic profiles for each basin. The vertical signal hard to detect in the coloured contour plots. I think you need to follow the example of Jones et al and Siddall et al and show specific depth profiles for each of the ocean basins.

(AR) Some profiles for Atlantic and Pacific basin have been added to the figures 3 and 5, in the same way as Jones et al. And Siddall et al.

Specific Comments:

(RC) P5551, L14 – Nd is not widely sampled compared to other tracers and there are special deficiencies such as the particulate component.

(AR) ' widely ' has been replaced by ' recently '.

(RC) P5552, L19 - The Siddall et al study already showed that the water-mass effect does not exclude vertical cycling

(AR) This sentence aims to introduce the notion of ' Nd paradox ', stating that ' at first sight ' (this has been added in the text), the water-mass tracer property is not compatible with vetical cycling. Also, we refer P5553, L23 that indeed, Siddall et al study showed that the water-mass tracer property does not exclude vertical cycling.

(RC) P5552, L28 - I suggest that BE best represents genuine exchange (i.e. no net input, see P5553, L11) and the Boundary Input/Output or similar is used to represent Input and Output. Using BE will certainly be confusing to the broader community

(AR) Lacan and Jeandel (2005), as well as all the papers referencing BE (including the present one, P5552, L25-28), define BE as both a source and a sink. P5553, L11, we refer that Arsouze et al. (2007) parameterized in their model both the sources and sinks of the BE as a genuine exchange. However, you are definitely right in the sense that we sometimes mix up the exchange process and the Boundary Input, by naming both ' Boundary Exchange '. We corrected that in the manuscript, by using ' Boundary Source ' for the BE source, and ' Boundary Scavenging ' for the BE sink, so as to avoid any confusion.

(RC) P5553, L29 – maybe add ', as these authors acknowledge,' - I would just state that these authors acknowledge the need for further work and tone down some of this criticism. Perhaps this is just a subtle question of tone that needs to be sorted out by a native speaker.

(AR) The sentence has been modified, and clearly states the authors acknowledge the need for further work.

(RC) P5554, L5 - I disagree - one could imagine a number of sensitivity experiments to explore paleo scenarios with these models. I would remove this statement or modify it to read 'limiting any potential paleo-applications'. Note the advantage was that we were able to do vital sensitivity experiments (which you are prohibited from doing because of your cumbersome model).

(AR) The sentence has been modified according to the proposition.

(RC) Section 3 – I think you need to explain how you justify these choices of scavenging coefficients

(AR) Few words have been added in the text about the choice of these scavenging coefficients.

(RC) Section 5.2 and elsewhere – if particle size is so important then why do the Siddall et al simulations do a better job than your simulations? The Siddall et al simulations do not include the particle size effect explicitly.

(AR) It is hard to compare two different models with two different parameterizations of the Nd cycle, and two different pools of particles. Distribution of particles in our model is much less satisfactory than the observation-based one used by Siddall et al., and the fact that these authors obtain better results than we do does not mean that they would not obtain even better results using different particle size. The important issue is that using our model, we need to take big particles into account to reproduce the Nd cycle in a more accurate way. We try to moderate our conclusions concerning the particle size limiting them to the use of our model.

(RC) P5569, L17 and elsewhere – you need to state that this number is highly tentative in the absence of any sensitivity tests. Your other simulations show a residence time between 125 and 760 years – why are you suddenly so confident to state 360 years in the light of this information?

(AR) This statement has been moderated in the text.

(RC) Table 1 – include the K values and res time for the Siddall et al simulations for comparison.

(AR) The K values and residence time of Siddall et al. (2008) have been added in Tab. 1.

(RC) All figures - bigger font needed in the figures and on the axes

(AR) All the figures have been modified with a bigger font and increased resolution.

(RC) Fig2 – too low res and axes/text too feint

(AR) This has been modified, so that Fig. 2 should now be easier to read.

(RC) Fig 6 and in text – you need to discuss why these comparisons are so poor ? It is hard to believe you are really making a big step forward when the concentrations are so poorly modeled. Why are the Siddall et al simulations getting this correct when yours are not?

(AR) Performances of our simulation have been discussed in the text and the shortcomings in small particle concentrations are considered to be the main cause in our modeling difficulties. Having no access to Siddall et al. simulations, it is not possible to derive specifics diagnostics to explain the different performance of the two studies. Moreover, they are completed with different protocols (forcing, source of travers,etc..) that preclude any rigorous inter-comparison

(RC) Fig. 8 and text – it makes no sense to integrate over all of the interesting water masses and lose a lot of the signal. I suggest to integrate say 3000 to 4000 m

(AR) We now integrate between 2500 and 4000m depth.

(RC) Fig. 9 – very nice but you could discuss the implications of this more. What if the sink were to increase or change size during sea-level ïňĆuctuations?

(AR) A sea-level variation of 120m (as during LGM) might not influence that much Nd Be, that not only acts at surface but also at greter depth. Rather, changing in erosion rates and ocean circulation are most likely to influence Nd distribution. This has been discussed in Arsouze et al. 2008.

Referee #2: Anonymous referee

General Comments:

(RC) 1. I think the results of the model are not as satisfactory as the authors suggest. For example, none of the simulations reproduce the observed concentrations of Nd very well (Fig. 6). I think the evaluation of the results should thus be adjusted.

(AR) Evaluation of the results has been moderated in several parts of the text.

(RC) 2. In my opinion the explanation of methods should provide more details (see spe- cific comments).

(AR) We tried to answer satisfyingly to all specific comments concerning how the method has been implemented.

(RC) 3. The manner in which results are presented is sometimes superficial instead of being detailed (see for example specific comments 16,17,18,19).

(AR) We refer to the answers to the specific comments. We tried to modify the description of the results so as to make this part of the manuscript clearer and more detailed.

(RC) 4. The term "Boundary Exchange" should be defined clearly. In their study Arsouze et al. denote the burial of particles (and thus particle-associated Nd) in the sediments as "Boundary Exchange", which is in fact rather part of internal cycling processes (parametrized by reversible scavenging). As I understand it, the "Boundary Exchange" used in this study does not really include a sink, but only a source (i.e., ïňĆux across the sediment-water interface) and the sink is rather provided by the internal cycling.

(AR) As stated by specific comment #3 of referee #1, ' Boundary Exchange ' is defined as both source and a sink (P5552, L25-28). To make this BE / source / sink notion clearer, we defined the ' Boundary Source ' and ' Boundary Scavenging ' as the associated BE source and sink respectively. In this study, Boundary Source is explicitly parameterized (margin sedimentary source), while Boundary Scavenging is provided by the internal cycling.

(RC) 5. "Sensitivity tests" are mentioned throughout the paper but it is not clear whether this expression refers to EXP1-5 or to additional experiments that are not shown (e.g., p.5559 L.16, p.5568 L.19, p.5568 L.28). It would be helpful, to make that clear (e.g. add cross-references to Table 1), and to provide further details about what has been tested in the additional experiments which are not presented in the paper.

(AR) Indeed, this was a bit messy. We tried to cross-reference whenever possible, and clearly state when additional tests were performed but not presented in the paper.

(RC) 6. The comparison of results with observations is not quantitative enough. It would be nice to have a measure of quality for each run (like that used for EXP5 (p.5565 L.20), or e.g. the root mean square deviation of model results from observations). If mentioning 71% in case of EXP5 it would be helpful to provide this measure of quality also for the other experiments (there seems not to be a big difference between EXP2 and EXP5 in Fig. 4).

(AR) Some quantitative results have been added in the text and in Tab. 1.

(RC) 7. The paper shows some deficiencies in the use of English. The quality and clarity of the paper would therefore benefit from professional editing.

(AR) cf. reply to Referee #1 comments. The manuscript has been read and corrected by an English native speaker. Hope this helps with the clarity of the paper.

Specific comments:

(RC) 1. It is necessary to better explain the boundary conditions which are applied in the model. It is not obvious why the authors need to apply a map of ÏţN d if already a map of Nd concentration is used. I think Arsouze et al. should clearly state how the global map of ÏţN d (Fig. 2a) at the continental margins is applied to the sediment-water ïňĆux.

(AR) As stated in the description of the Reversible scavenging model (section 2.3) and p5560, l.17, we use Nd concentration and Nd IC to model the two 143Nd and 144Nd isotopes concentrations. Both isotopes concentration are calculated using average

isotopic composition in Nd concentration and εNd definition. Information has been added in the text.

(RC) 2. As far as I know, Jeandel et al, (2007) did not publish a global map of Nd concentrations (Fig. 2b) at continental margins but did publish a global map of ÏţN d at continental margins. It would be impor tant to provide more information about how the map in Fig. 2b was created.

(AR) The Nd concentration map has not been published in Jeandel et al. (2007), but the database including the Nd concentration information was published as complementary material of this paper. The Nd concentration map has been generated in the same way has the εNd map. This has been added in the text.

(RC) 3. Goldstein and Jacobsen (1987) did not publish concentrations of individual Nd isotopes in river runoff (Fig. 2e), but concentration of Nd and the ratio of [143]Nd/[144]Nd. Therefore it is not clear how Arsouze et al. obtain the required concentration of each individual isotope

(AR) This comments refers to specific comment #1. Concentration of each isotopes is determined using average concentration of each istopes in Nd concentration and εNd definition.

(RC) 4. Jeandel et al. (2007) did not publish a global map of the neodymium isotopic composition of dust, nor do Grousset et al (1988, 1998). It would be helpful to have some additional information about how the map in Fig. 2f was created.

(AR) Original data from Grousset et al. provide some direct information about Nd IC of dusts in certain regions, but not a global map. In the missing areas, we applied Nd IC value of the source region of the dust from Jeandel et al. (2007) compilation. This statement has been rephrased and emphasized in the text.

(RC) 5. Not enough information is provided on what is going to happen to remaining particle-ïňĆuxes at the bottom of the water column. Presumably, all the particle associated neodymium leaves the model at the bottom. I think this should be mentioned.

(AR) Indeed, all the particle associated neodymium leaves the model at the bottom. This statement has been emphasized in the text.

(RC) 6. How is dissolution of particles treated in the model? As this is important for the effect of reversible scavenging on Nd concentrations, I think this should be mentioned in the paper as well.

(AR) The degradation rate of POMb and POMs, and of POMs in DOM depends on temperature with a Q10 of about 1.9. This has been added in the text.

(RC) 7. p.5552 L.22: As far as I know, Tachikawa et al. (2003) were the first to pro- pose continental margins as an additional source and should thus be cited in this context.

(AR) This reference has been added.

(RC) 8. p.5557 L.21: The reversible scavenging model was already applied to Nd by Siddall et al. (2008).

(AR) This reference has been added.

(RC) 9. p.5558 L.15: Regarding equation 2 I think it would be helpful to have some more information, about which equations are being transformed and inserted into each other in order to obtain equation 2.

(AR) Equation (2) is obtained via applying equation (1) to each particulate pool. This has been added in the text.

(RC) 10. Subtitle 2.4 says "Description of Nd sources". As the sink term is also treated in this section (p.5561 L.27), it should be mentioned in the subtitle as well.

(AR) This has been added in the text.

(RC) 11. The calculation of maskmar should be better explained (p.5560 L.8).

(AR) Some more explanation about this maskmar has been added in the text.

(RC) 12. p.5562 L.22: Preferential scavenging is mentioned only once within the paper and there is no reference given. I think this expression requires some further explanation or at least a reference.

(AR) The part about preferential scavenging has been emphaiszed in the text.

(RC) 13. The authors should explain why and how they chose the corresponding values of K and provide a citation for "available data" (p.5563 L.7).

(AR) Some citations for ' available data ' are already provided p 5562, L12. Few words have been added to explain the choice of these corresponding values of K.

(RC) 14. It should be mentioned that the number of 2.3 pmol(Nd)/kg refers to [Nd]model (p.5564 L.13), and in turn, that [Nd]model corresponds to the mean concentration of the global ocean as mentioned later on (p.5565 L27). In general, I would suggest an additional table containing symbols and abbreviations used in the paper.

(AR) This has been mentioned in the text: ' (average global value of [Nd]model = 2.3 pmol(Nd)/kg,. . . '.

(RC) 15. p.5565 L.9: A cross-reference should be added to support this statement.

(AR) A cross-reference to Tab. 1 has been added in the text.

(RC) 16. p.5565 L.17: This is not obvious for AABW (which is one of the "main" deep-water masses in the Atlantic).

(AR) We moderated the results obtained in the Atlantic basin, especially concerning AABW.

(RC) 17. p.5565 L.22: This is not obvious for AABW in the North Pacific (where ÏţNd is too low and EXP3 and EXP4 seem to do a better job) and therefore the question arises whether the inter-basin gradient of deep-waters is reproduced very well. Please provide an additional figure, or Fig. 8 with modified depth resolution to support this statement (see also comment 30).

(AR) Figure 8 has been modified. ÏţNd is now averaged between 2500 and 4000m.

(RC) 18. p.5565 L.23: In my opinion Fig. 7 does not contain any information about inter- mediate depths (but about surface layers).

(AR) ' intermediate ' has been removed.

(RC) 19. p.5565 L.24: The authors should be more specific here (e.g. mention that con- centrations in upper layers are still too low).

(AR) We now mention that concentration in the upper layer are too low.

(RC) 20. p.5565 L.26: "sediment remobilization process" should be changed to Fsed to make it clear that it is adjusted manually.

(AR) Fsed has been added in the text.

(RC) 21. p.5565 L.28: If there is some reason to consider a residence time of 360 years to be more realistic than one of 640 or 760 years, it should be mentioned in this context.

(AR) ' more realistic ' has been removed from the text.

(RC) 22. p.5567 L.11: As the authors draw here one of their major conclusions, I think a reference dealing with the role of submarine groundwater inïňĆow (in particular concerning the depth in which this process is of impor tance) should be inserted here.

(AR) A refe rence has been added in the text.

(RC) 23. p.5568 L.9: I disagree with the statement that there is a "remarkable" agree- ment with the data in EXP4 as AABW is not ver y well represented in the Atlantic basin (Fig. 3).

(AR) ' remarkable ' has been removed from the text.

(RC) 24. p.5568 L.13: The upper panel of Fig4 in Siddall et al. (2008) shows a relatively good match of model results and data, without considering different particle sizes. I

am wondering why the authors are so confident that particle size plays a big role in reproducing ÏţNd and Nd concentration if they do not consider particles aside from POMs and litho in EXP1-EXP3 (but POMs, POMb, BSi, CaCO3 , and litho in EXP4-5). Could the better match of model results and data in EXP4 and EXP5 not possibly be explained by particle type, rather than particle size?

(AR) Referee #1 did a similar comment. We refer to the author response for the first part of the question.

(RC) 25. p.5570 L.10: The authors state; "We simultaneously simulated both Nd IC and concentration...". In contrast on p.5558 L.8 it is written that isotopes are simulated and IC is calculated afterwards.

(AR) This comment refers to both minor comments #1 and #3, i.e. to the way Nd IC and concentration were defined based on 143Nd and 144Nd concentrations. We could not model explicitly ÏţNd has this parameter is not conservative in the ocean. Rather, we modeled both isotopes 143Nd and 144Nd (that are conservative in the ocean), and solved a 2 equations – 2 unknowns system to calculate Nd IC and concentration. All this has been added in the text p.5558 L.8. To us, their is no contradiction between p.5570 L.10 and p.5558 L.8.

(RC) 26. Fig.3: For the purpose of comparison of upper and lower panels, application of the same color scale would be helpful. Labeling is partly in French. Please provide coordinates of the Atlantic transect, or show its location within a map.

(RC) 27. Fig 5: I think the color scale should be changed here, as the observed gradient of concentrations within the water column is hardly visible in some of the sub figures.

(RC) 28. Figs. 3,5: Labeling of the y-axis is missing in these figures.

(RC) 29. Figs. 4,6: I think a legend explaining the meaning of the symbols used (Atlantic, Indian, Pacific) would enhance clarity.

(AR) to (RC) #26 – 29 : All figures have been re-created, in higher definition, with the suggested modifications. Concerning (RC) #27, several color scales have been tried out, but final decision was to get two different colorbars for the two basins so as to evidence the intra-basin gradients rather than the inter-basin gradients (cf. Fig. 7 and 8 for inter-basin comparison).

(RC) 30. Fig.6: What do the lines of $\pm$ 10pmol/kg mean? They are not mentioned in the text.

(AR) Fig. 4 and 6, the $\pm$ 3 εNd and $\pm$ 10pmol/kg represent the range used to characterize a good agreement between the model and the data. This has been added in the caption of the figure.

(RC) 31. Fig.8: I think averaging across a depth range between 800 and 5000 m is not ver y helpful here. Why not averaging across a smaller depth range (e.g. mean depth of NADW)?

(AR) Fig. 8 is now averaged between 2500 and 4000m.

Referee #3: Anonymous referee

Comments:

(RC) One of the shortcomings of the input parameterization at the margins is my opinion the assumption that the input is similar in amount at every margin. There is clear evidence that shelves underlying oxygen minimum zones serve as a stronger source than low productivity margins (cf. Haley et al., 2004). This should be explicitly mentioned and the authors may want to include an estimate of this source in their input function based on the global fraction of such suboxic/anoxic shelves.

(AR) We are already stating p. 5560, l. 4-7, that some factors acting on the Boundary Source might lead to large differences in the flux of the source. However, we currently do not know what are the processes involved in BE, and the amplitude of variation of the flux. O2 concentration, but also temperature, sediment discharge, turbulence or

current velocity are all parameters (among others) that have a potential to influence the Boundary Sources. We do agree that this assumption of similar input at every margin is crude, but we do believe that it is a necessary first step in the Nd cycle modeling. Trying to parameterize BE and set the source a geographically variable is a matter of another dedicated study. This has been added in the text in the conclusion part, as a perspective for future works.

(RC) It is obvious that the model needs to be extended to different types of particles which most likely will have very different distribution coefficients (Kds) for Nd (similar to the modeling approach by Siddall et al., 2005 for Th and Pa). For this exercise however, a dedicated study of the Kds for REEs needs to be carried because the information on particle type affinities for Nd and the REEs is quite scarce, as far as I am aware.

(AR) As far as we know, indeed, particle type affinities for Nd and REEs are unavailable. That's the reason why we did not do any sensitivity test on these parameters, even if we are aware that, indeed, there are likely important differences in Kds. In the manuscript, P.5562, l. 20 – 26, we mention this problem.

Minor Comments:

(RC) Page 5562, line 12: What is Ap/Ad? I guess it is particulate over dissolved Nd concentrations but the authors need to say so.

(AR) Ap/Ad was indeed the particulate over dissolved concentration. This has been replaced everywhere in the text by Ndp/Ndd.

(RC) On top of page 5567, the authors claim that Nd concentrations were simulated "an order of magnitude lower". Lower than what? I don't see that when looking at fig. 5 where the Nd concentrations for Exp 2 and 5 are quite close to the real values.

(AR) The sentence here was incomplete. Concentrations simulated in EXP5 are still one order of magnitude lower than the available data, at surface and intermediate depth. This has been added in the text.

(RC) In figures 3 and 5, the authors need to provide latitudes of their sections or at least provide it in the captions. These figures, as well as figs. 7 and 8 need to be provided at high resolution because otherwise it will be ver y difficult to identify any details, which is important to distinguish and to compare the results of the experiments. The complete list of references for all data used in these figures needs to be provided in the captions of the figures. There are many data missing from crucial areas, such as for example Spivack and Wasserburg (1988) or Stordal and Wasserburg (1986) for the Atlantic and Amakawa et al.'s surface and deep water data in the Pacific and Indian Ocean. There is also a new and important Nd isotope data set for the eastern Atlantic, in par ticular the deep southeastern Atlantic Basin (Rickli et al., 2009), which provides strong evidence for continental contributions from Africa and which needs to be included in figures 7 and 8.

(AR) All the figures have been re-generated in higher resolution, and Fig. 3 and 5 now provide latitude to the sections. All the data, except Ricki et al. (2009), where part of the data collection of Lacan and van de Flierdt, and so include the references you mention (Wasserburg 1988, Stordal and Wasserburg 1986 or Amakawa et al.). In Fig 3 and 5, two maps have been added in the graph so as to locate the two vertical sections, and the location of the vertical profiles.

(RC) For figures 4 and 6 the authors need to provide correlation coefficients. It is virtually impossible to judge the agreement between data and model on the basis of only visual comparison of the cross plots and the correlation lines.

(AR) Some complementary information to Fig. 4 and 6 about data / model comparison has been added in Tab.1.

Interactive comment on Biogeosciences Discuss., 6, 5549, 2009.