Biogeosciences Discuss., 6, C1008–C1014, 2009
www.biogeosciences-discuss.net/6/C1008/2009/

Biogeosciences Discussions

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

Anonymous Referee #2

Received and published: 3 July 2009

The paper of Arsouze et al. deals with the modeling of the neodymium cycle in the modern ocean. The authors present a new and more comprehensive approach (compared to previous studies) which combines processes that have in conjunction not yet been examined. By using a coupled dynamical/biogeochemical model Arsouze et al. are also able to explicitly simulate particle fluxes within the water column.

In their paper they show results of five model experiments in which they apply different sources, varying magnitude of sources as well as different particle fluxes and varying equilibrium scavenging coefficients. Evaluation of model performance is achieved by comparison with observational data.

Arsouze et al. show that riverine and dust-associated fluxes of Nd into the ocean cannot explain the global pattern of ϵ_{Nd} and of Nd concentrations within their OGCM.

Instead, the authors underline the importance of Nd-flux across the sediment-water interface in order to better represent observational data and highlight the necessity of taking into account internal cycling for explaining the "Nd paradox".

The paper of Arsouze et al. is a step forward in modeling and understanding the neodymium cycle in the ocean. This is needed for the interpretation of past and present records of Nd concentrations and isotopic composition in sediments and seawater, respectively.

General Comments:

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.

2. In my opinion the explanation of methods should provide more details (see specific comments).

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

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., flux across the sediment-water interface) and the sink is rather provided by the internal cycling.

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.

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).

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

Specific comments:

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 ϵ_{Nd} if already a map of Nd concentration is used. I think Arsouze et al. should clearly state how the global map of ϵ_{Nd} (Fig. 2a) at the continental margins is applied to the sediment-water flux.

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 ϵ_{Nd} at continental margins. It would be important to provide more information about how the map in Fig. 2b was created.

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.

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.

5. Not enough information is provided on what is going to happen to remaining particle-fluxes 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.

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.

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

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

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.

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.

11. The calculation of mask$_{mar}$ should be better explained (p.5560 L.8).

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.

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).

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.

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

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

17. p.5565 L.22: This is not obvious for AABW in the North Pacific (where ϵ_{Nd} 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).

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

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

20. p.5565 L.26: “sediment remobilization process” should be changed to F_{sed} to make it clear that it is adjusted manually.

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.

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 inflow (in particular concerning the depth in which this process is of importance) should be inserted here.

23. p.5568 L.9: I disagree with the statement that there is a “remarkable” agreement with the data in EXP4 as AABW is not very well represented in the Atlantic basin (Fig. 3).

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 ϵ_{Nd} and Nd concentration if they do not consider particles aside from POMs and litho in EXP1-EXP3 (but POMs, POMb, BSi, $CaCO_3$, 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?

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.

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.

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.

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

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

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

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

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