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Biogeosciences Discussions

Interactive comment on "Impact of enhanced vertical mixing on marine biogeochemistry: lessons for geo-engineering and natural variability" *by* S. Dutreuil et al.

Anonymous Referee #2

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General comment

This study addresses the impact of artificially enhanced vertical mixing of biogeochemical tracers on carbon export, CO2 fluxes and further N2O and DMS fluxes to the atmosphere, by means of a biogeochemical model (PISCES) coupled to an ocean general circulation model.

The enhanced mixing is restricted to the biogeochemical tracers, but has no effect on ocean physics. Further, by parameterizing a ‘‘well-mixed box model" of the atmosphere, and because fossil fuel emmissions and biospheric carbon fluxes are used for ‘‘updating" atmospheric CO2 and N2O, the study is limited to the sole effects of ocean

biogeochemistry. A more detailed model, that also simulates the potential feedbacks between ocean biogeochemical processes, the atmosphere, land biota, and ocean physics may be necessary to address comprehensively the consequences associated with the deployment of mechanical pipes. However, within these limitations (some of which are already mentioned by the authors), to my opinion the paper discusses an interesting aspect of the impact of artificially increased mixing on ocean-atmosphere fluxes of climate relevant gases.

Specific comments:

I have just a few concerns or suggestions to make:

Methods section:

Given the detailed consideration of N2O in the results and discussion section, I consider it necessary to describe explicitely the implementation of production and loss of N2O into the PISCES model.

What was the rationale for choosing the three regions for the enhanced mixing experiments? Could these regions be shown in a plot, together with the patchy mixing sites?

Results and discussions section:

This study (model) seems to be based on Aumont and Bopp (2006). In that paper, model results are compared to observations either at the surface, or for vertical sections down to about 5000 m, i.e. the plots do not resolve the simulated profiles in the upper few hundred meters very well. Given the rather short time scale (20 years) considered in this study, I would find it helpful to see gradients and/or profiles of the relevant tracers (e.g. nutrients, N2O, oxygen) in the upper few hundred meters (e.g. like Fig. 3 for DIC and Alk).

Sections 4 and 5:

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PISCES to my knowledge is a rather complex model, and the sensitivity of the model to alterations in the biogeochemical parameters is not addressed in the discussion. Thus, it remains unclear how the model results would look like with a different set of biogeochemical parameters. I would suggest to add at least some discussion about this aspect. Are there any sensitivity studies carried out with the model? (in particular with respect to iron parameterization, light limitation parameters, remineralisation length scales?)

The model seems to show some defiencies in the Southern Ocean (e.g. Fig 1), and I would suggest to add some comments on this in the discussion of the effect of mixing/pipes in this region.

Minor comments:

p. 4, lines 17 to 24: I think the three sentences mentioning the results of the study would better fit into the discussion sections.

p. 10ff: Reading the paper for the first time, I was a bit unsure to what mixing experiment the authors refer to in section 3.2.1. It seems to be the patchy mixing experiment. This could be stated in one sentence.

p. 12, line 7: ‘‘regional nature of the alkalinity profile” - perhaps better: ‘‘regional variability of the alkalinity profile”?

p. 12, line 17: ‘‘to the increased mixing (3.5 umol kg-1)” - I don’t understand this number of 3.5 umol kg-1 - of what?

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