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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.

S. Dutreuil et al.

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We would like to thank both reviewers for their comments that we think help to clarify and improve the manuscript. We detail here our responses to anonymous Referee 2 and to Ricardo Letelier (Referee 1). In addition, and in response to Katja Fennel's comment, we have included references to Karl and Letelier (MEPS, 2008) and Fennel (MEPS, 2008)

———————— Comments by Referee 2: 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.

>> Done - We have added a few more lines of description (including equations) to be more explicit for the N2O parameterization. The text now reads:

Finally, we also include here the module of the oceanic N2O proposed by Suntharalingham et al. (2000) to compute N2O seawater concentrations and air-sea fluxes. N2O is treated as a non conserved tracer, where the tracer equation is as follows:

The first three terms on the right-hand side represent advection, vertical and lateral diffusion, JN2O represents the biological sources of N2O and FN2O is the flux of N2O across the air-sea interface. We follow Suntharalingham et al. (2000) and we assume no N2O production in the euphotic zone (JN2O = 0). In the aphotic zone, N2O is produced when ammonium is oxidized to nitrate (nitrification pathway) and when organic nitrogen is converted to N2O at low O2 concentrations (denitrification pathway). Again, we follow Suntharalingham et al. (2000) and parameterize the N2O source term to be

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a function of the O2 concentration and O2 consumption. JN2O is given by

where alpha and beta are scalar multipliers and f(O2) a function of [O2] such that N2O production is maximum at [O2] = 1 micromol/L (denitrification pathway). Values for the scalars alpha and beta have been taken from simulation OX.5 of Suntharalingham et al. (2000). N2O is lost to the atmosphere via gas exchange, the gas exchange coefficient, kg, being computed using the relationship from Wanninkhof (1992) and the Schmidt number for N2O.

——————— 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?

>> We chose our 3 regions as case studies to examine the response to artificially elevated vertical mixing when restricted to a given geographic area. As such, the 3 regions were chosen to highlight the contrasting impact of regional mixing that might be associated with either artificial or natural mixing. They highlight the interacting effects of the changes in carbon export, DIC and alkalinity in governing the response (in terms of the rate of change in atmospheric CO2) to vertical mixing well. We decided not to include the regions on the plots, so as not to complicate/mask the readability of the results. But we chose to restate their broad geographic locations where necessary in the text. The text now reads:

In order to examine the impact of restricting mixing to a larger, but geographically constrained, region we performed regional mixing experiments in idealized locations (sub-Arctic Pacific, Southern Ocean, and equatorial Pacific).

——————— 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

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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).

>> We agree with the reviewer and have added the requested panels (O2, NO3, Fe, N2O, DMS) to figure 3 (DIC and Alk already shown).When available (DIC, Alk, NO3, O2), we also show the gradient of the relevant tracer deduced from available climatologies. The new figure 3 is also mentioned in the text when it helps to discuss the impact of enhanced mixing.

———————— Sections 4 and 5: 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 reviewer is correct to note that the sensitivity of the model parameterizations is an important aspect. However, we do not feel that this manuscript is the place to include such an exhaustive analysis, which would require many additional model runs in order to test the full range of possible outcomes for the fullest range of model parameters (e.g., iron scavenging, remineralization, uptake terms for the various nutrients and the photosynthetic parameters for instance). We are actually currently using assimilation studies conducted at JGOFS stations to better understand and constrain our models parameters (Kane et al., in prep). Nevertheless we do include a brief discussion of how they might alter/modify the model results. For example, the weak response of export in the Fe limited southern ocean will be linked to how we parameterize the Fe/C ratio to vary with light. However, this would only modify the response in terms of its magnitude and not its direction. In other regions, the local variability in the DIC and alkalinity gradients is the main driver of the response (in terms of pCO2, see table 1) and as such, sensitivity to biological parameters would be of second order. We have

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added an additional paragraph in the Caveats section to address these points (as well as those of R. Letelier). This new paragraph reads as such:

PISCES is a relatively complex OGCBM in terms of the biological processes accounted for. As such, there are certain parameters that are necessarily unconstrained. Although we did not perform an exhaustive sensitivity analysis during this particular study, the analysis in Table 1 shows that the predominant drivers of the pCO2 response are DIC and alkalinity. However, PISCES does not represent variability in C/N/P ratios during the production and remineralisation of organic matter. Any resulting C/N/P variability might therefore be important in governing the CO2 response to enhanced vertical mixing in oligotrophic regions that are limited by N and P. That said, variability in C/N/P demands arise from complex cellular processes and adaptation (e.g., Klausmeier et al., 2004) and their inclusion in a global 3D OGCBM is not straightforward at this stage. On the other hand, PISCES does represent variability in Fe/C ratios (as observed by laboratory studies, e.g., Sunda and Huntsman, 1997). Accounting for this phytoplankton physiological response (variable Fe/C ratios) results in a weak export response to the increased Fe concentrations (arising from enhanced mixing) in the Southern Ocean. Finally, we also note that as more similar studies are performed using alternative OGCBMs, we will be in a position to better estimate the overall uncertainty associated with the particular modeling strategies adopted.

————————- 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.

>> As already mentioned for surface chlorophyll in Aumont and Bopp (2006), in the Southern Ocean, the simulated chlorophyll standing stock seems to be too elevated during the growing season. A possible explanation may be the overpredicted middepth iron concentrations which lead to an excessive accumulation during winter.

This is only part of the explanation: all tracers (see gradients on Figure 3) show similar

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discrepancies when compared to observations in the S.O. Deficiencies in the representation of the S.O physics by the OPA model may be responsible for the mismatch between model and observations. In particular, because of its coarse resolution, OPA does simulate an intense open-ocean convection in winter that acts to flatten the tracer gradients as seen on Figure 3. A new version of the OPA model, now including a new parameterization to better represent the formation of dense waters along bathymetry has been released recently and will help to better represent biogeochemical tracer concentrations in the S.O.

Nevertheless, in terms of the impact of mixing/pipes in this region, we argue that because DIC and Alkalinity show very similar discrepancies, the effect of the mean-state mismatch would be dampened.

———————— 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.

>> While we understand the reviewer position, we feel it is of interest to mention the broad results of our study at this stage. Nevertheless, should the reviewer or editor feel strongly about this then these can be moved to the discussion.

———————— p. 10: 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.

>> Agreed - We have modified the first sentence of the paragraph to clarify this point. The text now reads: As expected, the artificial enhancement of ocean mixing in the patchy mixing experiment promotes an increase in biological productivity and export of carbon (Figure 2).

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

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>> Changed - The sentence now reads: Thirdly, the spatial variability of the alkalinity profile (which can be either positive or negative, Figure 3) can either reduce or increase pCO2, depending on whether more, or less, alkalinity is provided to surface waters&.

———————— 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?

>> Agreed - We have removed this number from the text that now reads: In the tropical Atlantic, DIC mixing increases pCO2 by 10 ppm, but biological productivity responds greatly to the increased mixing and lowers pCO2 by approximately 5 ppm.

————————- R. Letelier (Referee)

In the present manuscript, Dutreuil, Bopp and Tagliabue apply a general circulation and biogeochemistry model (OGCBM), as described in Aumont and Bopp (2006), and enhanced with DMS and N2O modules, to assess the effect on atmospheric CO2, N2O and DMS resulting from enhancing the upwelling of 200 m deep water in selected oceanic regions. This analysis appears in response to the recent suggestion made by Lovelock and Rapley (2007), among others, that ocean fertilization and a concomitant anthropogenic carbon sequestration by the biological pump could be achieved by enhancing the upwelling of deep nutrient rich across the thermocline into the surface layers of oligotrophic regions. The conclusions reached by Dutreuil et al. indicate that, although particle carbon export increases as a result of artificial ocean mixing, there is a net decrease in the ocean CO2 uptake and an apparent increase in DMS and N2O fluxes from the ocean into the atmosphere.

In its present form, I have serious concerns regarding some of the conceptual constructs driving this model. From a physical perspective, the authors seem to equate the upwelling of 200m deep waters to an increase of the mixed-layer depth down to 200m. However, the concept of using pipes to bring deep water into surface layers of the ocean does not necessarily imply an increase in the mixed-layer depth. As suggested by Letelier et al. (2008), the mixed-layer will deepen only if the rate of cooling of

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surface waters resulting from the mixing with deep cold water is greater than the solar radiant heating. Hence, we should not assume a priory a decrease in photosynthesis resulting from a decrease in light availability due to an increased mixing-depth.

>> We agree with the reviewer that in the current version of the manuscript we seem to equate an upwelling of 200m deep waters to an increase of the mixed layer depth down to 200m. We also agree that the concept of using pipes does not necessarily imply an increase in the mixed layer depth.

What we have done to mimic the concept of pipes is to mix all biogeochemical tracers from the surface of the ocean down to 200m (as stated in the submitted version of the manuscript), but without explicitly modifying the mixed layer depth. We have artificially increased the vertical diffusion coefficient to 1 m2/s in the first 200m for all passive tracers, which would be broadly similar in terms of net transport to adding both an artificial upwelling and downwelling velocities of 0.1 m/s.

Hence, we do not simulate any change in the mixed layer depth and do not impose any decrease in light availability driven by an increased mixed layer depth. Thus we do not assume any a priori decrease in photosynthesis. The text has been corrected accordingly in the few places where it was unclear.

The paragraph in the Methods section has been modified accordingly and now reads: “For all the mixing experiments and in an attempt to mimic the impact of ocean pipes on biogeochemistry, we have increased the vertical diffusivity coefficient, Kz, for all biogeochemical tracers, down to 200m (which is in line with the proposed dimensions of mechanical pipes, (Lovelock and Rapley, 2000)). Accordingly, all passive tracers are mixed permanently from the surface to at least 200m, which would be broadly similar in terms of net transport to adding both an artificial upwelling and downwelling velocities of 0.1 m/s. All the other physical/dynamic forcing variables are unchanged (in particular, we do not simulate any change in the mixed layer depth).

——————————— Even more troublesome are some of the biochemical assumptions

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embedded in the model. For example, having a fixed C:N:P stoichiometry for the production of organic matter and its remineralization within the water-column constrains artificially the potential role of the biological pump in the sequestration of atmospheric CO2. This was one of the main arguments used in the 80’s by marine geochemists to argue that the biological pump did not play a role in the sequestration of carbon because there is a tight coupling between the nutrients brought to the surface through upwelling and the amount of DIC in the upwelled waters (the remineralization of organic matter through respiration generates nutrients [N and P] and CO2 in the same ratio than needed for the generation of organic matter through photosynthesis). In other words, in its present form, the OGCBM only rediscovers what was originally stated by geochemists and expand on the role of Fe as a limiting micronutrient. However, many laboratory and field studies since JGOFS indicate that there can be significant uncoupling between C:N:P in the production and remineralization of organic matter (i.e. Michaels et al. 1994, Karl et al. 1997; Christian et al. 1997, White et al. 2006, Karl and Letelier 2008). Depending on the flexibility of the biological coupling of C:N:P, the biological pump may play a significant role in the sequestration of CO2.

>> We concur with the reviewer that our assumption of constant C/N/P stoichiometry during both the production and remineralization of organic matter is a simplification. However, complex 3D ocean general circulation and biogeochemistry models (OGCBMs) necessitate compromises between reality and efficiency. In this case, the compromise is that N and P are coupled to C in the production and remineralization of organic matter. That said, we do note that N and P are decoupled in the PISCES OGCBM by denitrification and N2 fixation. Parameterizing flexible C/N/P stoichiometry in 3D OGCBMs is a challenge that should be addressed in newer versions of our (and other contemporary) OGCBMs. Nevertheless, PISCES does include variable Fe/C and Si/C ratios (see: Aumont and Bopp, 2006) that vary as a function of the phytoplankton group and environmental factors (such as light, biomass and external nutrient concentrations). Indeed, we note that changes in the Fe/C ratio that are driven by the increased Fe concentrations induced by the artificial mixing is of key im-

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portance in producing a weak response (in terms of export) to upwelling in Fe limited regions. Addressing how enhanced mixing would impact C/N/P ratios would require us to parameterize the effect of light, temperature, species composition and specific geochemical processes (e.g., N2 fixation) on the C/N/P ratio and how it might change in response to the environmental changes produced by increased mixing, as well as any adaptive and/or acclimatory responses by the resident phytoplankton (in terms of adjusting their C/N/P ratio in line with the modified environmental conditions). At this stage, addressing these processes at the global scale is beyond the scope of this work. As such, we have amended the text (in the caveats section) to account for our OGCBMs simplification of this issue. We hope to be able to improve our models ability to account for these potentially important processes in the future.

This is discussed in the new caveats paragraph and reads thus: PISCES is a relatively complex OGCBM in terms of the biological processes accounted for. As such, there are certain parameters that are necessarily unconstrained. Although we did not perform an exhaustive sensitivity analysis during this particular study, the analysis in Table 1 shows that the predominant drivers of the pCO2 response are DIC and alkalinity initial concentrations which are independent of the response of the biological activity to enhanced mixing.

In addition, although PISCES is a relatively complex OGCBM, it does not represent variability in C/N/P ratios during the production and remineralisation of organic matter. Any resulting C/N/P variability might therefore be important in governing the CO2 response to enhanced vertical mixing in oligotrophic regions that are limited by N and P. That said, variability in C/N/P demands arise from complex cellular processes and adaptation (e.g., Klausmeier et al., 2004) and their inclusion in a global 3D OGCBM is not straightforward at this stage. On the other hand, PISCES does represent variability in Fe/C ratios (as observed by laboratory studies, e.g., Sunda and Huntsman, 1997). Accounting for this phytoplankton physiological response results in a weak export response to the increased Fe concentrations (arising from enhanced mixing) in the

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Southern Ocean. Finally, we also note that as more similar studies are performed using alternative OGCBMs, we will be in a position to better estimate the overall uncertainty associated with the particular modeling strategies adopted.

———————— There are also some other minor points that should be addressed by the authors if a new version of the manuscript is presented.

For example, one of the main outcomes in the present study deals with the increase of N2O as a result of ocean fertilization using upwelling pipes. However, there is no clear description in the paper or the literature cited describing this model component and its parametrization.

>> Done - We have added a few more lines of description (including equations) to be more explicit for the N2O parameterization. The new paragraph is pasted below: “Finally, we also include here the module of the oceanic N2O proposed by Suntharalingham et al. (2000) to compute N2O seawater concentrations and air-sea fluxes. N2O is treated as a non conserved tracer, where the tracer equation is as follows:

The first three terms on the right-hand side represent advection, vertical and lateral diffusion, JN2O represents the biological sources of N2O and FN2O is the flux of N2O across the air-sea interface. We follow Suntharalingham et al. (2000) and we assume no N2O production in the euphotic zone (JN2O = 0). In the aphotic zone, N2O is produced when ammonium is oxidized to nitrate (nitrification pathway) and when organic nitrogen is converted to N2O at low O2 concentrations (denitrification pathway). Again, we follow Suntharalingham et al. (2000) and parameterize the N2O source term to be a function of the O2 concentration and O2 consumption. JN2O is given by

where  and  are scalar multipliers and f(O2) a function of [O2] such that N2O production is maximum at [O2] = 1 mol/L (denitrification pathway). Values for the scalars  and  have been taken from simulation OX.5 of Suntharalingham et al. (2000). N2O is lost to the atmosphere via gas exchange, the

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gas exchange coefficient, kg, being computed using the relationship from Wanninkhof (1992) and the Schmidt number for N2O. “

————————- Finally, although the main in situ spatial distribution of sea surface chl a and deltapCO2 are reproduced in the model, there are significant differences in the absolute magnitudes. Could the authors use these differences to assess the sensitivity and uncertainties of the model? It bothers this reviewer that in the present manuscript the authors do not attempt to provide error or uncertainty estimates to any of the budgets or fluxes derived from the model.

>> We concur with the reviewer that an estimate of the uncertainties associated with our modeling approach is missing in this manuscript (as it is often the case for modeling studies using general circulation models!). Different ways to obtain such an estimate are possible, all of them would be very CPU-time consuming. So far, most of the uncertainty estimates in the “modeling world” arise from the comparison of several modeling studies that have followed the same simulation protocol (see results and uncertainties for climate projections in the last IPCC report, see results and uncertainties for the climate-carbon feedback (Friedlingstein et al. 2006), see results and uncertainties for the effect of iron fertilization on atmospheric pCO2 (discussion section in Aumont and Bopp, 2006). In this manuscript, we present the first estimate of the effect of enhanced mixing on global marine biogeochemistry (C-sequestration, N2O and DMS air-sea fluxes). We argue that as more similar studies are performed using alternative OGCBMs, we will be in a position to better estimate the overall uncertainty associated with the particular modeling strategies adopted. The following sentence has been added in the Limitations section: Finally, we also note that as more similar studies are performed using alternative OGCBMs, we will be in a position to better estimate the overall uncertainty associated with the particular modeling strategies adopted.

———————— In summary, I believe that the authors are trying to address an important issue and reach interesting conclusions. However, the assumptions used in the model, specially the fixed elemental stoichiometry, do not allow developing a realistic

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representation of the potential effects of deploying pumps in oligotrophic environments with the objective of increasing primary and export production in order to sequester anthropogenic CO2. As Wally Broecker used to say, without a flexible C:N:P stoichiometry the biological pump cannot play a significant role in the sequestration of carbon. The results of Dutreil et al seem to confirm this fact.

>> We note that we are not only trying to address the impact in oligotrophic gyres (where C/N/P changes might be important), but also in high northern and southern latitudes, where Fe limits production and Fe/C changes are important in governing the export production response and where regional variability in the DIC and alkalinity gradients also play a large role in dictating the subsequent impact on CO2 fluxes (contrast the Southern Ocean and the sub-Arctic Pacific for example).

In a brief response to the reviewers point about flexible C/N/P ratios being the only way to significantly sequester carbon, we broadly agree with this assessment regarding C/N/P (notwithstanding the absence of any quantitative OGCBM experiments to assess this), but note that the flexible C/Fe stoichiometry present in the PISCES OGCBM is actually responsible for REDUCING the sequestration of carbon, whereas if C/Fe ratios were fixed, then sequestration would be enhanced (i.e. opposite to that proposed for C/N/P).

———————— In addition, and in response to Kaja Fennel's comment, we have included references to Karl and Letelier (MEPS, 2008) and Fennel (MEPS, 2008) in the Introduction. We have added the following sentence: ...potential modifications of N2-fixation that have been discussed recently and for which the net effect of an artificial upwelling remains unclear (Karl and Letelier, 2008, Fennel, 2008).

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