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Comment

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

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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

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

Even more troublesome are some of the biochemical assumptions 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 80s 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). Depend-

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ing on the flexibility of the biological coupling of C:N:P, the biological pump may play a significant role in the sequestration of CO₂.

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. Finally, although the main in situ spatial distribution of sea surface chl a and $\Delta p\text{CO}_2$ 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.

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 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.

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