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Biogeosciences Discussions

Interactive comment on "Fueling primary productivity: nutrient return pathways from the deep ocean and their dependence on the Meridional Overturning Circulation" *by* J. B. Palter et al.

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The paper presents an in-depth analysis of a series of global biogeochemical model simulations used already in several previous studies. Main focus of the paper is the role of SAMW in fueling nutrient supply to the low-latitude surface waters of these coarse-resolution models, as postulated by previous studies. By their careful and clever choice of dye tracers and an associated tagging scheme, the authors present a very concise and convincing analysis that in many places confirms earlier hypotheses about the role of the Southern Ocean, and SAMW in particular, in the global nutrient budget.

The paper thus adds new, interesting and useful information, it is generally very well prepared, and it should be published in Biogeosciences once the concerns below have been addressed. I apologize for the long delay caused by my late review.

Concerns:

1. terminology: The study employs the OCMIP biogeochemical model, which does not resolve any phytoplankton or other particulate organic matter. It basically channels all surplus nutrients (surplus wrt observed surface PO4) reaching the surface layer into export or DOM. The biological production simulated by this model is thus very close to new production rather than primary production (as stated several times in the paper, including the title and figure captions). As new production is, in my view, the more relevant property for biogeochemical cycles and presumably also for fisheries, changing "primary productivity" to "new production"might even further strengthen the paper.

2.choice of isopycnals: The dye tracers are defined in isopycnal ranges 26.5-27.1 (SAMW) and 27.1-27.4 (AAIW). The analysis focuses on waters lighter than 26.8, i.e. above the mid-point of the SAMW range (e.g., Table 3, Fig 10). This seems to give a convenient safety zone (26.8-27.1) for dilution of SAMW by AAIW (or deep) waters from below, and it will likely bias the results in favor of a greater importance of SAMW. The inclusion of all surface waters moving north out of the Southern Ocean across the 26.5 isopycnal without being subducted as SAMW might also enhance the contribution of the SAMW tag.

I'm not sure whether a better coincidence of dye tags and analysis isopcynals is possible in a z-level GCM, and I do not suggest that the authors should modify their carefully constructed scheme. It might, however, help to provide some additional discussion of the above caveats, e.g. some quantitative estimate of the sensitivity of their results to the particular choice of the 26.8 analysis isopycnal. The statement on the bottom of p. 4060 that "qualitative results are not sensitive to the density horizon used" seems a bit

weak.

3. preformed vs regenerated: With the above choice of isooycnals and dye tags, essentially all waters upwelled in the Southern Ocean and moving north will be tagged as AAIW or (predominantly) SMAW waters. What is most interesting in terms of carbon uptake or biogeochemical feedbacks such as a possible response of diazotrophs is the fate of preformed nutrients. Regenerated nutrients may have experienced biotic diapycnal fluxes (sinking) and are thus more difficult to interpret than preformed nutrients that exclusively move with the water. I suggest that the authors focus more on preformed than regenerated nutrients (e.g. modify panels b,d,f,h in Figure 11). Even though the contribution of preformed SAMW PO4 is smaller than that of regenerated PO4, this is a very interesting result that could be pointed out more clearly.

I'm afraid I do not understand the concept of negative contributions of remineralized nutrients to primary (new) production (Fig.11,12). I imagine that one can separately diagnose the new production fueled by preformed PO4 and the new production fueled by remineralized PO4 at each grid point and time step. Shouldn't both contributions be non-negative? Does it make a difference whether the regenerated nutrients derive from tagged DOP or other material?

4. diapycnal dye fluxes: In the zonal average (presumably along z levels?) of Figure 9, both SAMW and AAIW tags show systematic shifts towards lighter densities, in the case of SAMW by a few hundred meters on presumed time scales of a few decades (40 year lifetime estimated by the authors). This suggests diapycnal transport velocities of several meters per year (particularly when the downward diapycnal export flux of POM is accounted for), perhaps somewhat higher than the average value of Munk's abyssal recipes (probably OK in the high mixing areas of the Southern Ocean). This is in contradiction to what is shown in Figure 12, which suggests a net DOWNWARD flux of SAMW dye almost everywhere south of 20S. Is there a contribution by convective adjustment not included in the vertical mixing term of the model output? (If so, this should be shown separately or included into the vertical mixing term.) Or is there a

contribution of the GM advective fluxes not yet included in the Figure?

The systematic shift towards lighter density classes is already visible in the Southern Ocean (Fig.9), although dye should be destroyed outside the tagging density range in this area (Fig.4). Presumably, the broader density range of zonally averaged dye is a feature of the zonal averaging procedure (along z-levels?), though I do not understand why this should generate a systematic shift towards lighter densities.

minor points:

abstract, l.6 suggest to avoid the alarmist term "catastrophically" (would you say that production or carbon uptake increase "catastrophically" in other areas?)

p.4055, line 5. Why zero and not 100%?

p. 4055, line 26. "similar" might be just OK, but it might be worthwhile to mention that the observed Si* reaches zero already south of the equator in the Pacific Ocean and only at about 50N in the Atlantic. This Atlantic-Pacific asymmetry is not seen in the models and might indicate problems with resolving the water mass (and Si*) formation in the Sea of Okhotsk.

p. 4065, l.2 "much lower" seems a bit too strong, and I would prefer a more quantitative statement (XX percent in the region...) . Right at the equator of the Atlantic Ocean, one might even diagnose "much higher".

p. 4060, l. 27ff. "independent of the physical model". The insensitivity might be artificially pronounced by the choice of the OCMIP biogeochemistry that limits feedbacks of the circulation on nutrient concentrations. Perhaps phrase this more carefully.

p. 4060, l.21 "noticeable contribution". Suggest to include a more quantitative statement.

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