

Interactive comment on “Spatial Variations in Silicate-to-Nitrate Ratios in the Southern Ocean Surface Waters are Controlled in the Short Term by Physics Rather Than Biology” by Pieter Demuyndck et al.

Anonymous Referee #1

Received and published: 5 May 2019

The submission by Demuyndck et al. explores the mechanisms that maintain nutrient concentrations and stoichiometry across the polar frontal zone of the Southern Ocean – a critical region for nutrient supply to low latitude ecosystems. The traditional view is that biological process exert a dominant control on nutrients in this region, drawing down silica faster than other nutrients as waters advect northwards towards the formation region of Antarctic Intermediate Water (AAIW). Demuyndck challenge this view using an idealized model that connects a series of upper ocean boxes each containing mixed layer and subsurface layer, and resolves various physical exchanges between

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them. They show that in fact, surface nutrient concentrations and ratios mostly mirror the subsurface waters and are maintained by physical supply from below, rather than biological uptake. This is an interesting finding, and I like the approach of using an idealized model from which simple insights can be distilled. Overall, I am therefore supportive of this paper. However, I think there is still a little work to do in exploring the limitations of the physical supply mechanism, before the paper is ready for publication.

Ultimately, it seems clear that biological uptake must be responsible for the drawdown in nutrients and change in surface nutrient stoichiometry across latitude. The authors acknowledge this and focus their discussion on “short timescales”, on which the physical supply dominates. However, I feel like the paper may still underrate the role of biology for a few reasons that I’d like to see addressed.

First, on page 21 it is stated that “biological processes are not necessary to reproduce a surface macronutrient gradient”, referencing a sensitivity test in which uptake is “switched off”. In fact, Fig. 12b shows that in this experiment the surface silicate gradient weakens more than 50% when biology is removed (70uM difference across latitude in control run, 30uM difference when biology removed). It think that this degree of weakening, even when the Si concentrations supplied from below are held constant with a very strong gradient (Fig. 5b) suggests a very important role for biological processes even on short timescales, which is not reflected in the paper. I would either like to see some discussion around why the authors don’t think this evidence for strong biological control, or for them to remove strong statements such as “biological processes are not necessary. . .”.

Second, because the model holds the nutrient concentrations in the deep layer constant, it is impossible for the authors to test the timescales on which physical supply versus biological uptake control surface nutrients. They state that uptake may become important on timescales longer than decadal, but it’s not clear that it wouldn’t be even shorter than this. Removing biological processes would soon impact the deep ocean

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boundary condition in the real ocean, both because organic matter remineralization is important in maintaining deep concentrations (which the authors acknowledge), but also due to mixing. The weakened surface nutrient gradient in the absence of biological uptake would soon start to impact the subsurface layer (through detrainment) and from there the deep layer due to diffusive mixing within the timescale of a year. Therefore, if deep water concentrations were not clamped at constant values, it seems that the surface gradient would be even further weakened the very next year due to a weaker supply gradient, and so on and so forth until the gradient very quickly disappears. Ideally, the authors would put forward a test to determine how quickly this feedback dilutes the nutrient gradient once biology is removed. I don't immediately see how to do this without entirely restructuring the model, but am open to any demonstration that the authors can design. I suppose the maximum speed of the feedback (fastest flattening of the gradient) could be quantified by simply resetting the deep boundary condition to the subsurface concentrations once per year. If such a demonstration is not possible, then I think the authors need to acknowledge that the nutrient gradient might vanish quite quickly without uptake (maybe even in a year so) if the boundary condition were not held constant.

Finally, the authors motivate the paper by discussing the connection of Southern Ocean nutrient concentrations and stoichiometry to low latitude ecosystems through AAIW and SAMW. Towards the end of the paper, they suggest that physics rather than biology may modulate this connection on short (decadal) timescales, because Southern Ocean surface nutrients are set by physical supply from below on those timescales. Even if one accepts the dominance of physics on this timescale (but see above), it is not clear that there would be much impact on the low latitudes. This is because AAIW and SAMW are already a few hundred years old by the time they reach tropical upwelling zones, and this long transport timescale would likely buffer the nutrient content of those watermasses against the decadal scale physical variations the authors postulate. In other words, the nutrient content of those waters seems like it must be controlled by the biological processes that ultimately control Southern Ocean surface

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nutrients. The authors should either refute this, or again better acknowledge the role of biological uptake in setting properties of SAMW and AAIW that are communicated to low latitudes.

I think these three issues need to be addressed before the paper can be published, but would reiterate that I like the overall approach of the paper and find it quite insightful.

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2019-120>, 2019.

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