

We thank the reviewer for the helpful comments on our paper. Our responses to the reviewer's comments are as follows:

The submission by Demuynck 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 processes 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). Demuynck et al. 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 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.

By definition a model can only be a representation of a real system. It is therefore important to check the sensitivity of (model)results by applying changes to certain parameters and see how it affects the results. The model includes biology and physics.

We included already some explorations of sensitivity to simplifications in the physical model. Firstly, upwelling velocities were increased by 50% in one model run and decreased by 50% in another. Secondly, we acknowledged that in reality the northwards transport is not completely restricted to the ML but rather takes place partly in the SSL as well. We explored the sensitivity of results to this. For one model run 80% of the total northward transport was made to occur in the ML and 20% in the SSL. Results were essentially the same in all altered models. As already stated in the MS, the results in Fig. 14 show that *“applying these changes one by one to the model does not greatly affect the final result in terms of the primacy of physical processes (entrainment) over biological processes in driving nutrient patterns in the surface ocean. For each altered model it remains true that the silicate gradient (the south-to-north gradient in the ML concentration) is more strongly affected by making the bottom boundary condition constant than it is by removing biology from the model.”*

In a revised MS we will include more exploration of possible limitations of the physical model and we will calculate the impacts on results where possible.

However, the main concern of the reviewer seems to go to biology and whether the role of biology is underestimated in the model on shorter timescales. A main weakness in this regard is the fixed deep water concentration (as a boundary condition for the model). We acknowledge that in the MS, more time and effort must go to the exploration of the effect of having a fixed boundary condition. We also refer to the second comment of the reviewer and our answer to that comment.

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 (70 μ M difference across latitude in control run, 30 μ M 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...”.

In the standard run the silicate gradient from 65 to 40°S is, as noted, about 70 $\mu\text{mol kg}^{-1}$. When biology is removed, the gradient is reduced to about 30 $\mu\text{mol kg}^{-1}$. When the effect of upwelling is removed, it is more like 10 $\mu\text{mol kg}^{-1}$. Our original statement “biological processes are not necessary to reproduce a surface macronutrient gradient” is therefore correct because a gradient persists when biology is removed from the model. Only a very small gradient persists when physics acting on a subsurface horizontal gradient is removed. However, it is also true that the gradient is reduced when biological processes are removed and we will modify the text to acknowledge this, including revising the statement “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 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.

The reviewer makes an interesting point. It is difficult without using a completely different model to be sure exactly how quickly the removal of biology would cause the horizontal gradient to disappear. However, we will carry out the extreme model experiment the reviewer suggests and modify claims accordingly.

The direct impact (via remineralisation) of biological fluxes on deep nutrient concentrations is minor over one or a few years. This is because the annual remineralisation fluxes at depths of several hundred meters are very small compared to the ambient nutrient concentrations. The lower boundary of the SSL is fixed in the model at 200, 300 or 500m for different stations (Table 1 of the MS). In order to understand how rapidly remineralisation could alter nutrient concentrations at these depths, we calculate remineralisation rates using a Martin curve to calculate the attenuation of the particle flux (F_z) and the associated remineralisation rate (R_z) as a function of depth:

$$F_z = F_{100}(z/100)^b$$

and so:

$$R_z = (F_{100}/100^b) * [(z + 1)^b - z^b]$$

Plugging in an estimated average export flux for the Southern Ocean of 30 g C m⁻² y⁻² at 100m depth (Schlitzer et al., 2002; Henson et al., 2011; Siegel et al., 2014) and a standard 'b' value of -0.8 yields carbon remineralisation rates at depths of 200 and 500m of between 0.01 and 0.07 g C m⁻³ y⁻². These can be converted to nitrogen remineralisation fluxes (units of μmol N kg⁻¹ y⁻¹) by multiplying by (106 / 12 / Redfield C:N), i.e. multiplying by 12.5 (assuming Redfield C:N ratio of 106/16 = 6.67). The exact numbers used in the calculations are not so important because it is clear that however they are calculated the rates are very low – annual remineralisation rates from this calculation are < 1 μmol N kg⁻¹ y⁻¹ at all depths between 200 and 500 m. Annual nitrogen remineralisation rates are thus much lower than the nitrate concentrations below the SSL (10-30 μmol kg⁻¹).

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 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.

We agree with the reviewer's point - the time that it takes for the mode waters to flow beneath the surface to low-latitude upwelling sites is indeed measured in decades/centuries rather than years. This means that it will take a long time before any anthropogenically-induced effects on mode water composition have consequences for surface waters at low latitudes. The paper describes how the 'steady state' nutrient distribution of the Southern Ocean (in particular, the meridional gradient in upper-ocean nutrients) is set up. The message is that biology sets the deep-ocean distribution of nutrients over long time scales of many decades to centuries (through e.g. the different remineralisation depths of N and Si) and that physics communicates this deep boundary condition to the upper ocean on short time scales of years. Thus, if one wanted to change SAMW and AAIW nutrients at low latitudes, changing the physics would be the quickest way to do this, as physics operates over years. Then of course one would have to wait decades for that signal of change to be propagated to low latitudes. It is likely, therefore, that the short-term pre-eminence of physical processes will be less important in terms of far-field effects, as the reviewer suggests. We will amend section 1 to reflect this.

References

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