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Interactive comment on "The fate of upwelled nitrate off Peru shaped by submesoscale filaments and fronts" *by* Jaard Hauschildt et al.

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Comment:
This study investigates the effects of filaments and fronts on subduction of nitrate in the Peru-Chile Upwelling System, using a submesoscale permitting regional ocean model coupled with a biogeochemical model. The results of this study found that about 40% of the newly upwelled nitrate near the coast of Peru is subducted before being used by phytoplankton. The results show a more pronounced subduction occurs at submesoscale fronts near the cold filaments, which were underestimated by the previous mesoscale resolving models. The manuscript is well written and includes new insights on the subduction of the nitrate in EBUS. Therefore, I have only several minor comments below.

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Reply:
The authors would like to thank Takeyoshi Nagai for his overall favorable review of our manuscript and his detailed, constructive comments which helped to improve the paper. In the following we will respond individually to the detailed comments.

Comment:
P1L11 “highlighting the additional value of direct rate measurements for model validation. ” Unclear

Reply:
This sentence has been rephrased and now reads as follows:
P1L9-11: “Surface chlorophyll in the filament is a factor 4 lower than at the upwelling front while surface primary production is a factor 2 higher.“

Comment:
P2L13 “citepChavez2007, Gruber2015, Koehn2017, Brady2019. ” Typo
P11L8 “80m (100m)” Spaces are needed before “m”.
P11L29 “30km” A space is needed before “km”.

Reply:
These mistakes have been corrected in the new manuscript.

Comment:
P11L30 “...along-shore position matches with two SST minima (16°C) at the coast...”
I couldn’t find two SST minima 16 degree at the coast in Figure 2. From the color scale

they look like 18-19 degree.

Reply:
We mistakenly described the SST minima of the model simulation instead of the observations in this sentence, the temperature has been changed to reflect the observed values. It now reads as follows:
P16L33-34: "Their along-shore position matches with two SST minima (19°C) at the coast, suggesting that they carry recently upwelled water."

Comment:
P14L2 ". . .is characterized in the following. . ."
"is" should be "are".

Reply:
This mistake has been corrected in the new manuscript. It now reads as follows:
P18L4-5: "The physical structure of the filament and the distribution of the biogeochemical parameters are characterized in the following."

Comment:
Figure 3 and 4 captions
There is no explanation for the broken white lines, which are probably MLD by reading legend, but the information should be in the captions.

Reply:
The broken white lines indeed represent the mixed-layer depth as stated in the legend, the information has been added to the captions as well (Note that these figures are now Figures 4 and 5 due to the addition of Figure 2 for validation purposes following another reviewer's comment).

Comment:
P17L28 “In brief, in situ PP observations based on carbon suggest that a significant portion of the observed offshore reduction in nitrate concentrations is due to uptake by PP. Simulations indicate that approximately 40.6% of upwelled nitrate is subducted and not utilised by PP. ”
Is this because PP estimated in-situ without the effect of subduction of nitrate?

Reply:
Thank you for this interesting comment. The in-situ PP estimate is computed using water samples contained in a bottle (as detailed in section 2.3). At the time and depth the bottle is closed, there is no way to determine whether the sampled water mass is a subducted water mass. To determine PP for a subducted water mass, it would be necessary to follow the water mass before it is sampled and be able to know that it has subducted. This was not done in the Eulerian experimental setup here and would require a Lagrangian approach. We can only assume that the sampled water mass corresponds to the filament and thus likely corresponds to a subducted water mass.

Comment:
P19L32 “While the coincidence of increased subduction and a negative change in nitrate may seem counterintuitive, ...”
To me, increased subduction and reduction of nitrate are not counterintuitive.

Reply:
We agree that to readers who are very familiar with the key concepts of our paper the use of the word “counterintuitive” seems out of place and is not

needed. We rephrased the sentence as follows:
P24L8-10: "The positive change in nitrate offshore together with increased eddy kinetic energy (EKE) in the 1/45° simulation suggests that an increase in the cross-shore nitrate flux takes over once the nitrate is removed from the surface (Fig. 8a,b)."

Comment:
P20L2 "-2 " Is the minus sign here for an increase?

Reply:
The minus sign was a typo and has been fixed, the corrected sentence is: P24L11-12: "At a depth of 300 m to 500 m nitrate increases near the coast (~2 μmol l^{-1}), directly below the strong negative nitrate change in the top 200 m (~$-$2.75 μmol l^{-1})."

Comment:
P20L3 "A plausible explanation for this pattern is that organic matter is able to sediment at shallower depth on the slope in the mesoscale simulation, which permits enhanced local remineralization and nitrate release with respect to the submesoscale simulation. "
I'm confused. If this is the case, remineralized nitrate is higher in 1/9 degree simulation with negative value in Figure 7b in below 300 m near the coast?

Reply:
We agree that the formulation in the first manuscript version was confusing. We now describe changes in the submesoscale simulation relative to the mesoscale simulation. The key concept that was missing from our previous process

description is that due to increased offshore transport in the submesoscale simulation sinking organic matter sediments at deeper shelf areas, out of reach of the upwelling source waters. This results in a downward redistribution of nitrate similar to the "nutrient leakage“ described in Gruber et al. 2010, a reference that we also added at this point in the manuscript. We rephrased our explanation:
P24L12-P25L4: “A plausible explanation for this pattern is that due to increased offshore transport in the submesoscale simulation sinking organic matter sediments on deeper shelf areas, out of reach of the upwelling source waters. This would result in a downward redistribution of nitrate similar to the "nutrient leakage" described in Gruber et al. (2011). However, small differences in the alongshore mean circulation along the continental slope (not shown) may also modify the nitrate content of the water masses. Disentangling these processes is beyond the scope of the present work.”

Comment:
P23L7 “Along-shore variability of PP in the simulations is closely related to chlorophyll concentrations, while the observed relationship of PP and chlorophyll is clearly more complex (Table 1). Investigating the reasons for this discrepancy is beyond the scope of this study and left for future work ”
Is this because PP estimated in-situ without the effect of subduction to dark layers?

Reply:
As explained in the reply to a previous comment, our experimental setup is not able to determine accurately whether the sampled water mass is a subducted water mass or not. So there is a possibility that the measured PP corresponds to the PP in an upwelled water mass or a subducted one. Another plausible explanation for these differences might be that despite the already high complexity in a model like PISCES, the parameterisation of primary production is

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still relatively simple. For example, changes of the phytoplankton physiology or species composition of the phytoplankton community are not modelled. Another missing process is the aging or health of the phytoplankton population, since in the real ocean phytoplankton does not change its state from "alive and containing chlorophyll“ to "dead organic matter without chlorophyll“ instantly. Some intermediate stage where the phytoplankton population may be hardly growing on average but still contains some chlorophyll seems more realistic, but it is not represented in this version of the PISCES model (note that a new version, PISCES-quota (Kwiatkowski et al., 2018) will be able to represent more elaborated phytoplankton stages in future studies). We have also incorporated this point into the discussion:

P28L30-P29L3: “Along-shore variability of PP in the simulation is closely related to chlorophyll concentrations, while the observed relationship of PP and chlorophyll is less clear (Table 1). Investigating the reasons for this discrepancy is beyond the scope of this study and left for future work, but we can speculate that it is related to the relatively simple parameterisation of primary production in PISCES (see section 2.5). For example, changes of the phytoplankton physiology or species composition of the phytoplankton community are not modelled. Another missing process is the aging or health of the phytoplankton population, which could slow down PP while chlorophyll concentrations remain high and thereby decouple both parameters. Note that a new version, PISCES-quota (Kwiatkowski et al., 2018) will be able to represent more elaborate phytoplankton stages in future studies.”

Ref:

Kwiatkowski, L., Aumont, O., Bopp, L., & Ciais, P. (2018). The Impact of variable Phytoplankton Stoichiometry on Projections of primary production, food quality, and carbon uptake in the global ocean. Global Biogeochemical Cycles, 32(4), 516-528.

Comment:
P23L13 “Near-surface stratification in the model is by a factor of 2-3 lower than in the observations ”
What mixing parameterization used in this study is missing in the manuscript. Is it KPP or two-equation type turbulence closures?

Reply:
The KPP parameterization is used. We added this information in the model description section:
P6L29-30: “The nonlocal K-Profile Parameterization (KPP) scheme is used to handle unresolved processes related to vertical mixing.”

Comment:
P23L18-31 “However, the fact that nitrate concentration...”
I think that PISCES includes ammonium. Therefore, the authors can see the fraction of ammonium for the PP.

Reply:
The PISCES model indeed includes ammonium. However, the sentence in question refers to our rate measurements which do not distinguish between new production and regenerated production. Since we have no observations to evaluate / compare the regenerated production in the model, we decided to only report total primary productivity. We rephrased the sentence slightly to make it very clear that this statement refers to observations only.
P28L13-18: “Besides, our observations of PP rates in the filament do not distinguish between regenerated production from ammonium and new production from nitrate (Dugdale and Goering, 1967). Fernandez et al. (2009) show that regenerated nitrogen can locally account for as much as 50% of surface primary

production in southern Peru. However, nitrate concentrations measured in the filament 150 km offshore are only 20 - 50% of those measured near the coast and nitrate appears to decrease with offshore distance on transect CROSS (Fig. 4). This is an indicator that a substantial amount of PP in the region of the filament survey near 15°S is fuelled by newly upwelled nitrate."

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