

We thank the referees for their insightful reviews and general support for our manuscript “Fueling export production: nutrient return pathways from the deep ocean and their dependence on the Meridional Overturning Circulation.” Our responses to the referees’ suggestions and questions are interwoven with their comments below. When quoting from the referees, the text appears in italics for clarity.

The most noteworthy changes to the manuscript concern two word choices. First, *primary productivity* has been changed everywhere to *export production* including in the title. The choice for the new wording is described in some detail in the response to Referee 2. The second is the choice to use the word *unmodified* to describe nutrients that come directly from a tagging region without being remineralized, following a suggestion from Referee 1. We had been using the word *preformed* to describe these nutrients, noting where the usage was different from a traditional definition of the word preformed. However, we agree with Referee 1 that an abundance of caution should be used regarding this subtle word and have opted to avoid any confusing language.

Response to anonymous referee 1:

Page 4051. The discussion of theoretical considerations of driving mechanisms for the overturning circulation (based largely on Ganadeskian 1999) was potentially a bit long/complicated given the limited direct use later?

While writing, we considered shortening the manuscript by excluding this somewhat lengthy theoretical framework. However, the reasons to include the discussion ultimately outweighed our desire for brevity, as this framework provides both the motivation for carrying 4 models through the paper and the key to understanding their differences. Each model achieves a different meridional overturning circulation, and no consensus has yet emerged from observational studies to tell us which circulation operates in nature (tracers and measurements of diapycnal mixing in the ocean interior seem to favor a Southern Ocean upward branch, yet velocity estimates from inverse models favor a low-latitude upward branch, see Table 2). Therefore, we feel that this framework is essential to understanding the comparison of the results among the models.

Page 4053, line 5. This sentence is a bit awkward, suggest ‘which are just under half as intense...’

We have reworded to: “approximately half as intense.”

Page 4053 and Table 1. Model nomenclature might be a bit confusing to those not already familiar with the MOM3 suite. Although ‘HH’, ‘LL’ ECMWF’ ‘P2A’ are explained clearly, (e.g. Table 1), a non-familiar reader would likely have to keep referring back to this table and interpreting the numbers contained. It might help to put simple descriptors next to the values in the table (e.g. ‘high’ next to diapycnal diffusivity in ‘HH’, ‘low’ next to Hellerman wind stress, rather than simply highlighting certain values in bold?

We agree that any help to the reader in recalling the experimental setup is useful. After experimenting with the placement of various descriptors within the table, we decided that it is least cluttered if they are included in the title of each column.

Page 4053. 'Clockwise' is possibly a bit of a confusing description here. Clockwise only applies in the case where north is represented to the right? Some brief clarification might help.

We have removed the word.

Page 4054. The partitioning of ('new') production into a sinking flux and a source term for DOP is apparently spatially invariant? Do the authors know how robust the current conclusions might be to relaxation of this assumption? e.g. one might expect that the proportion sinking might be higher (lower) in high (low) latitude systems?

Yes, in the OCMIP biogeochemistry model, the proportions of new production that are represented as a sinking flux and a source term for DOP are constant in space (and time). The principal conclusion from the models is that SAMW provides a major source of nutrients to the low latitude pycnocline, but that the size of this contribution is sensitive to the location of the MOC's upward branch. We believe that this conclusion is robust regardless of the uniformity in the proportion of new production that goes into a sinking flux, because it is driven to a large degree by the high surface and subducted nutrient concentrations in the SAMW formation region that are observed in nature, rather than the parameterized sinking flux of nutrients into the layer.

Page 4055. Why are the results presented for the LL-ECMWF after a longer spin up period, presumably this model takes longer to reach near steady-state?

The difference was due simply to a bookkeeping issue. The results for the earlier period of LL-ECMWF were not archived in a format that was convenient for analysis. A note has been added to the text to clarify.

Page 4056, line 10 onwards. I appreciated the authors' careful distinction of the 'pre-formed' and 'remineralised' pools and how these differ from traditional uses of these terms. I additionally wondered whether further clarity could be provided by using a different term, at least for the 'preformed' pool as labelled in the model runs, potentially 'pre-labelled' or 'pre-tagged'?

We have changed all instances of *preformed* to *unmodified* in order to further clarify, as noted above.

Page 4057, line 24 onwards and Fig. 7. Presumably the authors would argue that the better agreement between 'P2A' and the radiocarbon distribution suggests that this model contains the most realistic representation of the overturning circulation (i.e. dominance by the Southern Ocean). However they don't really return to this point when later discussing the contribution of SAMW nutrients. E.g. would they argue that the importance of this pathway in the real ocean is

likely towards the upper end of the values in Table 3 and on page 4063 they would presumably argue that within the real ocean, Southern Ocean mixing/upwelling dominates nutrient resupply?

We have added the following sentences at the end of Section 3, which explains our stance on this issue:

“There is currently no consensus on where and by what mechanisms the upwelling branch of the MOC is achieved: the distributions of tracers such as $\delta^{14}\text{C}$ are consistent with upwelling predominantly in the Southern Ocean (Fig. 7) as are the low coefficients of diapycnal diffusion measured in the ocean interior (Gregg and Sanford, 1980; Ledwell et al., 1993; Ledwell et al., 1998), while some observationally-based inverse models of overturning suggest a greater role for upwelling at low latitudes (Table 2). This controversy is the motivation for including 4 different models in this analysis, and the range of the results is thought to span the envelope of likely possibilities for the nutrient return pathways occurring in nature.”

Page 4058, line 6. This sentence was a bit confused, both the HH and the LL models have the same winds, they only differ due to differences in K_v and A . Suggest, ‘. . . Not surprisingly the LL model, which is also forced with. . .’

The sentence has been changed to:

“Not surprisingly, the LL model, which is also forced with sluggish Southern Hemisphere westerly winds but has weaker diapycnal diffusivity and GM coefficients, has precisely the opposite bias: an underventilated deep ocean with a low radiocarbon bias, and corresponding radiocarbon trapping in the upper ocean.”

Page 4065, line 25. Within the formation region of SAMW, ample fixed nitrogen in surface waters will also restrict nitrogen fixation.

We agree that when there is ample fixed nitrogen at the surface of the ocean, the energetically-costly process of nitrogen fixation is discouraged (Ganeshram et al., 2002; Deutsch et al., 2004). However, the root cause of the high fixed nitrogen concentrations both in the SAMW formation region and along its export pathways is likely a lack of iron (eg. Moore et al., 2009). To keep this causal relationship clear, we do not include a discussion of the high concentrations of fixed nitrogen.

Figure 13. From the graph presented, a contribution of half the nutrients from the preformed pool looks high? (the grey bars all appear to be <half the length of the remineralised bars). What is the actual value?

This is a good observation: the proportion is between 30 and 35%. The caption has been changed to “Approximately a third of the SAMW nutrients sustaining low latitude productivity are from the *unmodified* pool.”

Response to A. Oschlies (Referee 2):

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.

As noted in above, we have adopted the term 'export production' throughout the manuscript including in the title. The OCMIP biogeochemical model allows for the production of dissolved organic phosphorus (DOP) and its remineralization to phosphate everywhere in the water column, including at the surface. Hence, the resultant productivity in the model, the J_{prod} term, can be thought of as total primary productivity including productivity resulting from the drawdown of remineralized nutrients (see Fig. 3). Because export production is a constant fraction of total production, the fraction of export production sustained by each tag is equal to the fraction of total production sustained by each tag. The same cannot be said of new production.

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 isopycnals 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.

We agree that the details of our results are sensitive to the choice of the vertical averaging domain. However, we argue that the lessons of the experiments remain largely unchanged regardless of this choice. The 26.8 isopycnal was chosen as the base of the ventilated pycnocline, and is used in the introduction in our calculation of the nutrients lost from the seasonally-accessible layer at low-latitudes. To address the reviewer's comments, we repeated the calculations that were used to make Table 3, with the base of our averaging layer ranging from 26.5 to 27.4. As expected, averaging dye and phosphate concentrations to increasingly dense layers enlarges the proportion of the *deep low latitude phosphate* comprising the pycnocline. However, even when the 27.3

isopycnal is chosen as the base of the averaging domain, the relative rank of each tagged phosphate with regard to its contribution to the total phosphate in the layer remains the same. We have added the following language to reflect this point:

“This density horizon is chosen to represent the base of the ventilated pycnocline, but the qualitative results are not sensitive to the density horizon used, as long as it is above the 27.4 isopycnal below which the *deep low latitude* tag is assigned. Indeed, the relative rank of each tagged phosphate with regard to its contribution to the low-latitude pycnocline phosphate remains the same when averaging above the 27.3 isopycnal as the 26.8 isopycnal.”

We also agree that the inclusion of surface waters moving north out of the Southern Ocean that are not actually subducted as SAMW will lead to a greater role for SAMW than if the SAMW tag were somehow constrained to move only in the ocean interior (as is the case for all the high-latitude tags). However, the surface advective supply of SAMW tag goes to zero almost everywhere south of 40°S. Thus, the conclusion that SAMW formation and subduction are critical for restoring nutrients to the pycnocline should hold despite the northward spreading of the tag at the surface.

3. preformed vs regenerated: With the above choice of isopycnals and dye tags, essentially all waters upwelled in the Southern Ocean and moving north will be tagged as AAIW or (predominantly) SAMW 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.

We have remade Fig. 11 as suggested, with the right hand panels showing the contribution of *preformed* (now called *unmodified*) tagged phosphate to the total new production. The new figure is appended to the end of this document. We have also changed the text accordingly:

“The *unmodified phosphate* of any variety fuels less than 30% of the total productivity (Figure 11, right column), and low latitude productivity draws primarily from the *remineralized* phosphate pool. Thus, nutrients reaching the euphotic zone from any source region are utilized at least once, if not many times, before entering a new tagging region. However, in all models but HH, *unmodified SAMW phosphate* sustains more new production near the equator and in the low latitudes of the North Atlantic than any other *unmodified* tagged phosphate pool.”

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?

Theoretically, it should be possible to diagnose new production sustained by each tagged nutrient exactly, as described in this comment. However, the model was not configured to archive this diagnostic. Instead, the productivity sustained by each tagged remineralized phosphate (J_{PROD}^{tag}) must be diagnosed from source/sink terms, J_{PO4}^{tag} and J_{DOP}^{tag} . By summing equations 2 and 6 from Figure 3 above 75 m, we arrive at the expression:

$$J_{PROD}^{tag} = \frac{J_{DOP}^{tag} - J_{PO4}^{tag}}{0.33}$$

Using this expression, we calculate the productivity associated with each remineralized phosphate tag J_{PROD}^{tag} and divide it by the total J_{PROD} , which is archived by the model. According to this expression, a sink of PO_4^{3-} (i.e. negative J_{PO4}) is due to new production, while a sink of DOP (i.e. negative J_{DOP} , due to the remineralization of DOP to PO_4^{3-}) is associated with negative production. The J_{PROD}^{tag} can be negative if the tagged DOP is remineralized to PO_4^{3-} at a greater rate than the tagged remineralized PO_4^{3-} fuels productivity. We have expanded the explanation of this subtlety at the end of Section 2.3 on Experimental Design.

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.

The shift to lighter densities appears even when the dye is plotted as a function of density. It is most simply explained by the interplay of advective-diffusive spreading and the tagging protocol. The SAMW tag is destroyed at densities greater than 27.4; above this isopycnal, it is only mixed with the other tags, as is reflected by the gray shading in the Figure 4 schematic.

Thus, the downward advection and diffusion of the SAMW tag is offset by its destruction at heavier densities. The net result is the asymmetric shift to lighter densities.

This explanation is also related to the counterintuitive (but robust) result that is at the heart of the reviewer's other inquiry: how is it that the SAMW tag generally spreads to lighter densities in the Southern Ocean even as the net flux of the tag is directed downward at the 26.5 isopycnal? The explanation appears in the following text, which was incorporated into the manuscript:

“How do nutrients subducted in the SAMW layer at densities greater than those in the subtropical euphotic zone come to influence productivity in low latitudes? To help answer this question, we examine the vertical flux of the SAMW tag at the depth of the 26.5 isopycnal (Fig. 12), which is regularly ventilated in the subtropical North Atlantic. However, before discussing how the SAMW rises to the shallower isopycnals, we must first address the fact that the tag appears to move downwards at the depth of the 26.5 isopycnal just north of the tagging region. This net downward flux is a consequence of the vertical gradient in SAMW concentrations at the depth of the 26.5 isopycnal just north of the tagging region. In this region, SAMW concentrations decrease with depth across the 26.5 isopycnal because northward transport supplies the tag at the surface while the tagging protocol destroys the tag at depth. The downwelling and vertical diffusion acting on this gradient serve to transport SAMW tag downward. Downstream of this downwelling domain, the SAMW is transported upwards at the depth of the 26.5 isopycnal. The upward transport is primarily achieved by vertical advection in tropical regions, along the eastern fringes of the Southern Hemisphere subtropical basins, and in the South Atlantic western boundary current off Brazil (Figure 12). Advected and mixed to shallower depths, the SAMW can be transformed by surface buoyancy forcing and diapycnal mixing and may no longer carry the characteristic temperature, salinity and potential vorticity signature it had upon formation and subduction. However, our tags trace the water mass fraction and its nutrients downstream of the tagging regions even as the water mass itself is destroyed by such transformations.”

Finally, in the original manuscript we did exclude the convective adjustment in the diffusion term (along with a few other mixing components), though the GM stirring was included (as part of the explicit diffusion in the right hand panels of Fig 12). Because all diffusive terms should be included for a comprehensive view of where the tag moves upward at the 26.5 isopycnal, we have updated Figure 12 to include all of these terms. However, the figure does not noticeably change due to the inclusion of the additional mixing terms. The new figure is appended to the end of this document.

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?)

The word has been replaced with the word significantly.

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

Thank you for catching this typo. It has been changed.

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.*

We agree that this suite of coarse MOM3 models does not resolve many of the complex water mass formation processes thought to be important in the Sea of Okhotsk, including tidal mixing, which was not parameterized in this generation of MOM. The absence of these processes may be a potential cause of the reduced Atlantic-Pacific asymmetry in the models' SAMW tag relative to the distribution of the negative Si* tracer indicative of SAMW in nature. A statement in Section 2.3 has been added to the text to acknowledge this possibility. However, we do note that the models produce a slight Atlantic-Pacific asymmetry, more apparent in the maps of the SAMW nutrients (Figure 10) than in the SAMW dye (Figure 6).

p. 4056, 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".

We validated this statement by differencing the concentrations in the HH model from the others. The differences are seen largely in the Indian Ocean, the equatorial Atlantic and western tropical Pacific where the HH model (Fig. 6b) has 20-40% less SAMW than the others. The text has been changed to include this more quantitative language.

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.

We have now rephrased for clarity:

“The high *unmodified SAMW phosphate* concentration is determined by the OCMIP biogeochemistry, which sets surface nutrients at their climatological values, and is therefore not sensitive to the differing physics in each model.”

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

The wording has been changed to:

“Spreading of the *unmodified SAMW phosphate* is stronger in the P2A and LL-ECMWF models, contributing up to 20% of the total PO_4^{3-} above the 26.8 isopycnal in the subtropical North Atlantic (Figure 10, top row).”

References:

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Figure captions for the attached figures:

Figure 11: The zonally-averaged fraction of new production fueled by phosphate (*unmodified* + *remineralized*) from each of the tagging regions (left column) and by the *unmodified* component of each tagged phosphate between 30°S and 30°N (right column) for the four models described in Table 1. Note the panels on the left show the full latitude range of the model ocean, while the panels on the right show only the low-latitudes. The contribution of the *remineralized* components of tagged phosphate to sustaining low latitude productivity is the difference between the contributions of the total tagged phosphate (left) and the *unmodified* component of the tagged phosphate (right). Negative values reflect the method used to diagnose the new production fueled by each tagged nutrient using the source/sink terms for DOP and phosphate, as described at the end of Section 2.3. The lines refer to phosphate from the North Pacific (red solid), North Atlantic (red dotted), Deep low latitude (green solid), SAMW (black solid), AAIW (black dotted), and Southern (magenta solid) regions (refer to Figure 4 for tagging protocol). Note the different horizontal and vertical scales for the left and right panels.

Figure 12: Vertical fluxes of SAMW dye (in fractional concentration per $\text{m}^2 \text{ s}$) at the depth of the 26.5 isopycnal. Upper row is for the HH model and lower row for P2A. The left hand panels are the vertical advective flux (wC , where w is the vertical velocity and C is the concentration of the SAMW dye tag). The right hand panels are the vertical diffusive flux ($\kappa_v dC/dz$, where κ_v is the coefficient of diapycnal diffusion). The diffusive flux includes mixing due background diffusion, convection, and stirring due to the GM transport. The colorbar to the right of panel b applies to all maps, and positive numbers represent an upwards flux. The line plots are the zonal means of the maps (black for vertical advection and blue for vertical diffusion).

Figure 11

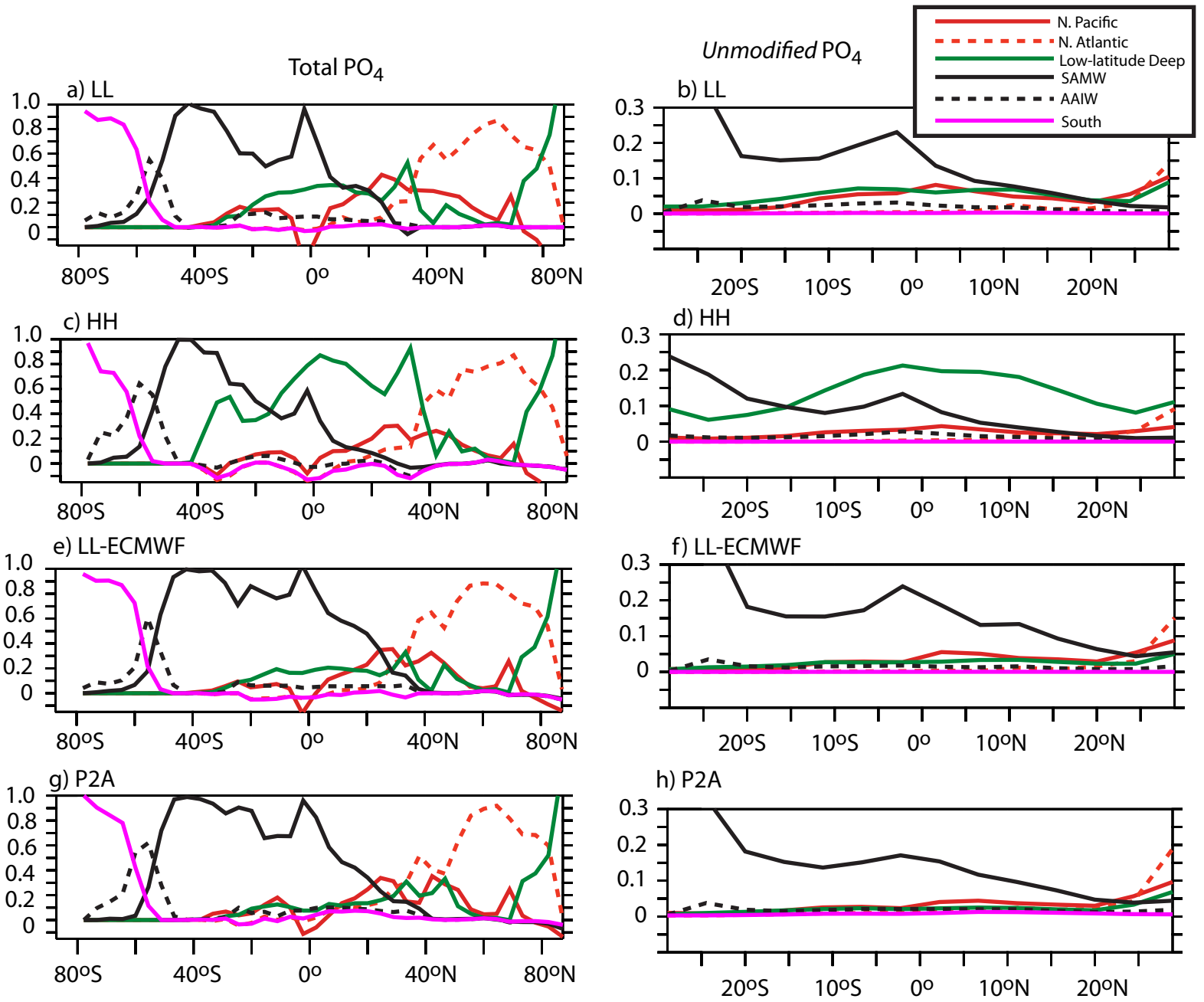


Figure 12

