Answer to Referee #2, Josep L. Pelegrí

We thank Dr. Josep L. Pelegrí for his careful review of our manuscript and for his thoughtful comments that will surely help to improve its quality. We tried to address all his comments and we include below a detailed answer to all the questions.

Answers to Major comments

Major comment nr.1:

(1) Evaluation of the model's performance

This is a very critical aspect and the authors dedicate a significantly long section, including an appendix, to evaluate the performance of the model. They compare the numerical output with observations using different datasets: the near-surface seasonal circulation as inferred from surface drifters; the annual-mean sea surface height, sea surface temperature (SST) and sea surface salinity; the annual-mean mixed layer depth (MLD); the annual-mean surface chlorophyll; and the annual-mean and seasonal-mean net primary production (NPP).

I value this effort very much but, honestly, at the end of the Evaluation section I have important doubts on how good the model's performance is. Throughout this section the authors recognize the existence of substantial differences between model and field data, and also talk about model bias.

Answer to MC1:

As stated by Dr. Pelegrí, we have invested quite some effort to carefully evaluate many aspects of our model. As a result, we feel that we are well aware of its strengths and its limitations. While there are clearly some issues, the results of our model evaluation are in line with most state of the art models – in many respects the fidelity of the model simulated fields is even better than that of most models. However, it is clear that models are never perfect, so the question we have to answer is to what degree biases and other types of errors will affect the results and the conclusions drawn from them. Our overall assessment is that, despite the biases, that the performance of our model is more than adequate to answer the main scientific question regarding the magnitude and the importance of the long-range lateral fluxes of organic carbon. Thanks to the information provided by a detailed model comparison with observations, we are also able to discuss in the paper how our results are affected by the observed biases, especially in the southern subregion of the Canary Upwelling System (CanUS), where we see the largest and most relevant differences from the observations. We address the different elements of this first main comment in sequence:

A) *In Figure 2 they show the spatial distribution of model-data differences for several surface fields. The differences are not negligible at all, as clearly seen by the range of values in the mean fields and the differences, e.g. SST (range of values is 12◦ C and range in deviations is 4◦C) and MLD (range of valuesis 100 m and range in deviations is 60 m).*

The SST biases are clearly significant but actually quite a bit smaller than implied by Dr. Pelegrí's comment, i.e., ±2°C. The SST plot (Figure 2b) shows that differences between model and observations lay in the interval [-0.75ºC,1ºC] in the large majority of the domain, with a large fraction of this bias having a range of only ±0.5ºC. Larger differences are confined to a very narrow coastal band. The region located south of Cape Blanc has the extensive bias. But also here, the (positive) bias has a range of only [0.5ºC,0.75ºC]. This warm bias is accompanied by a positive bias in salinity of about 0.5 (Figure 2c), leading to a near complete compensating with respect to their impact on density. Overall, we consider these biases to be small relative to the spatial and temporal variations. They are also too small to affect substantially primary production or the lateral export of organic carbon. Therefore we expect that these SST and salinity biases have a minor impact on our study. In response to this comment, we will discuss the SST and salinity biases and their impact on the study more explicitly.

The biases in the mixed layer depth are likely more relevant for our study. As highlighted by Dr. Pelegrí, while the modeled distribution agrees overall reasonably well with the observed one based on Argo-floats, our modeled MLD shows sharper gradients than the observed pattern resulting in rather large differences in the northern nearshore and the central offshore region. This could be a true bias of our model, but we also note that the Argo DT-0.2 MLD product was gridded on a relatively low-resolution 2ºx2º grid and that it has a rather limited coverage in the nearshore areas. As a result this product may not be able to properly capture strong gradients and overly smooth distribution relative to reality in regions with strong variations, such as ours. Given our MLD bias structure, it is feasible that some fraction of it could be attributed to biases in the Argo-based product.

In response to this comment, we will extend our already existing discussion in the Results and Discussion sections with a more in depth analysis. In particular, we propose to add a short paragraph to the discussion section to assess the potential impact of these biases in more detail. We also plan to add some material with regard to the impact on productivity (e.g., alteration of the light limitation) and lateral carbon transport.

B) *I particularly miss a comparison between the depth distribution of the modelled and observed particulate organic carbon (POC), which is of capital importance for this study. The seasonal results (Figure 5) show very large differences, possibly too large.*

We agree with Dr. Pelegrí regarding the necessity of having a comparison of modeled and observed POC and we thank him for this suggestion. To this end, we have conducted an evaluation of the modeled POC using 2 datasets: 1) the MODIS satellite estimate of surface POC (S-POC in the diagram); 2) the cruise POC measurements from AMT, ANT and Geotraces in the upper 200 m (POC in the diagram) located in the 0km-2000km offshore range of our analysis domain. Most of the in-situ data were collected in fall, especially in October, often in the far offshore region of our domain (cf. Figure MC1-1).

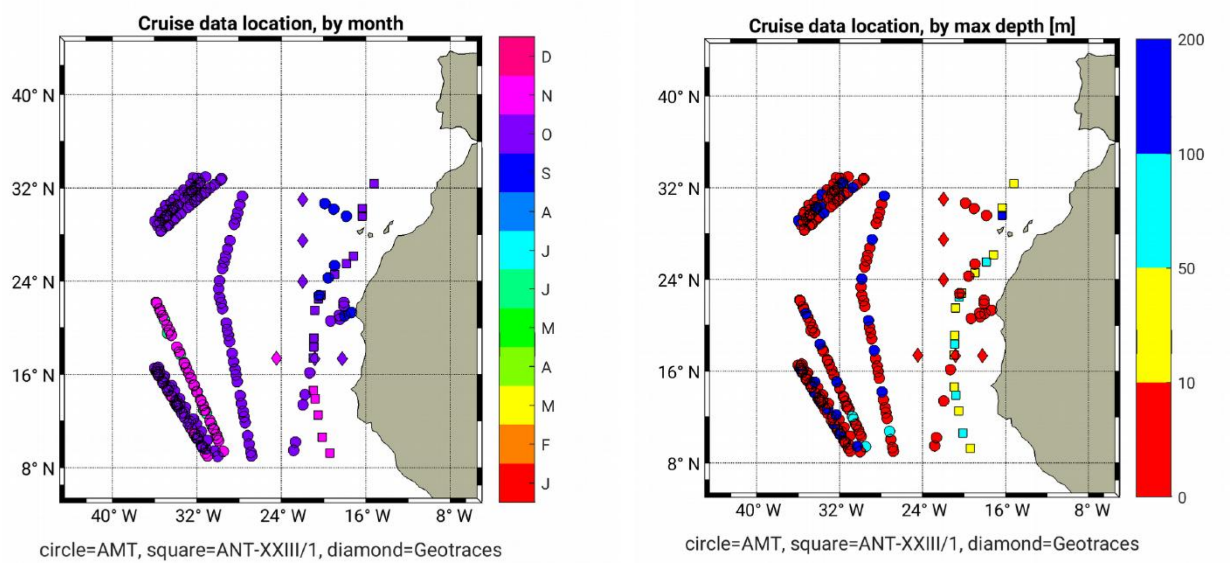

Figure MC1-1: Retrieved cruise POC measurements in the region of analysis corresponding to the Budget Analysis boxes. Data points are colored by sampling month and by maximum depth of the measurement. Circles=AMT (15-23), Squares=ANT, Diamonds=Geotraces.

Modeled POC and data were co-located in space and time using a daily ROMS climatology for the same 6 years. As visible from the resulting plot (Figure MC1-2), the magnitude of modeled and observed POC is the same and the vertically-integrated POC in the first 100m also corresponds. Due to our coastally-confined production (largely discussed in the model evaluation) combined with the fact that cruise data are mostly located offshore, and due to the deepening of the chlorophyll maximum in the southern productive subregion, we observe a deeper-than-expected POC maximum in the model. As also discussed in the paper, this may mean that if anything our model may underestimate the offshore transport in the CanUS (and especially in its southern sector), therefore implying that the already large magnitude of the offshore transport that we find may be a low estimate.

In response to this comment, we will include in the Taylor diagram to a comparison with POC (see Figure MC1-3) to both satellite estimates and in situ measurements. We will also highlight that in our paper we already provide a plot (Figure 7) of the mean vertical profile of the total organic carbon for the whole Canary Upwelling System.

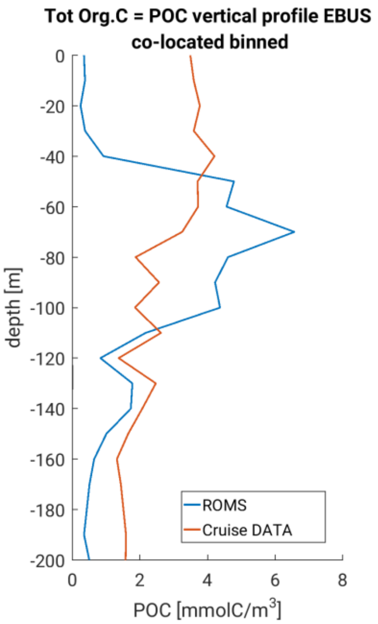

Figure MC1-2: mean POC profile in the CanUS compared to cruise data, from co-located POC, binned in depth to 10m depth intervals.

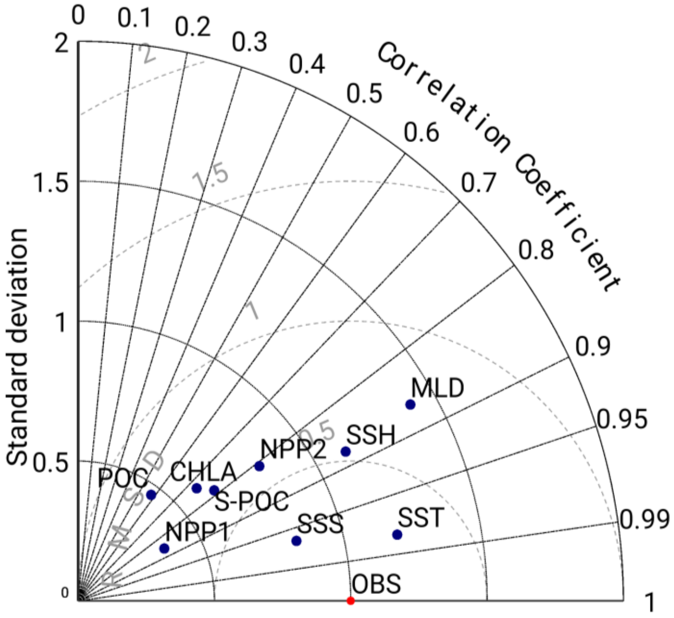

Figure MC1-3: Annual mean Taylor diagram including:
1) an evaluation of surface particulate organic carbon (**S-POC**) using SeaWiFS satellite estimates;
2) a comparison with depth profiles of **POC** from cruise data through co-location of ROMS output in space and time.

For additional discussion of the implications of having a shallower POC distribution we refer also to our answer to Anonymous Referee 1, in which we discuss the results of some sensitivity studies in terms of both transport and impact on NCP.

C) *The authors end this section referring to a Taylor diagram presented in Appendix B (Figure B3), concluding that there is a "good correlation between the modelled and observed fields both in the annual and in the seasonal means." They show the Taylor diagram for the annual-mean results and for the mean of the seasonal results. The authors argue that the Taylor diagram shows results comparable to other studies for upwelling systems. Rather than comparing with other studies, it would be better to look at the statistics and discuss whether the results are convincing or not. For the annual-mean, for example, SST, CHLA and MLD respectively have a (normalized) standard deviation of about 1.2, 0.6 and 1.4, and a (normalized) root-mean-square difference of 0.3, 0.7 and 0.8. The authors should discuss whether these values are reasonable or not.*

Following Dr. Pelegrí's comment, we will be more specific in the description of our Taylor diagrams in the Evaluation section. Regarding our Taylor diagrams included in Figure B3 and here in Figure MC1-3 and Figure MC1-4, all the variables show a correlation of 0.7 or higher with the observations in the annual mean (except cruise data POC) and 0.68 or higher in the seasonal. Among all variables considered, the values of the normalized standard deviations are particularly high for annual mean MLD (1.5), due to the too sharp gradients and high peaks discussed in paragraph (A) of this document. Low values of the normalized standard deviations are instead observed for chlorophyll (0.65) and for NPP1 (0.35) that corresponds to NPP compared to the SeaWiFS VGPM product. This is a consequence of the fact that, even though the representation of the pattern of the variable in the model is close enough to the observations, the magnitude of the modeled blooms is not as intense. Interestingly, if modeled NPP is compared to NPP from the SeaWiFS CbPM product (shown in the Taylor diagram as NPP2), a normalized standard deviation of about 0.75 emerges. This implies a rather large differences in the estimated NPP in the two products.

D) *I am particularly confused by Figure B3b: how is this calculated, just an average mean? What is the meaning? Wouldn't it be much better to show all four seasonal diagrams? It would also help to include, as supplementary materials, diagrams for each subregion.*

The Taylor diagram in Figure B3b is calculated as the simple mean of the seasonal Taylor diagrams. However, as suggested by Dr. Pelegrí, we have decided to substitute this figure, and explicitly include in the appendix of the paper the four seasonal Taylor diagrams, here visible in Figure MC1-4. We have now included surface POC (S-POC) compared against the SeaWiFS satellite estimates also in these diagrams.

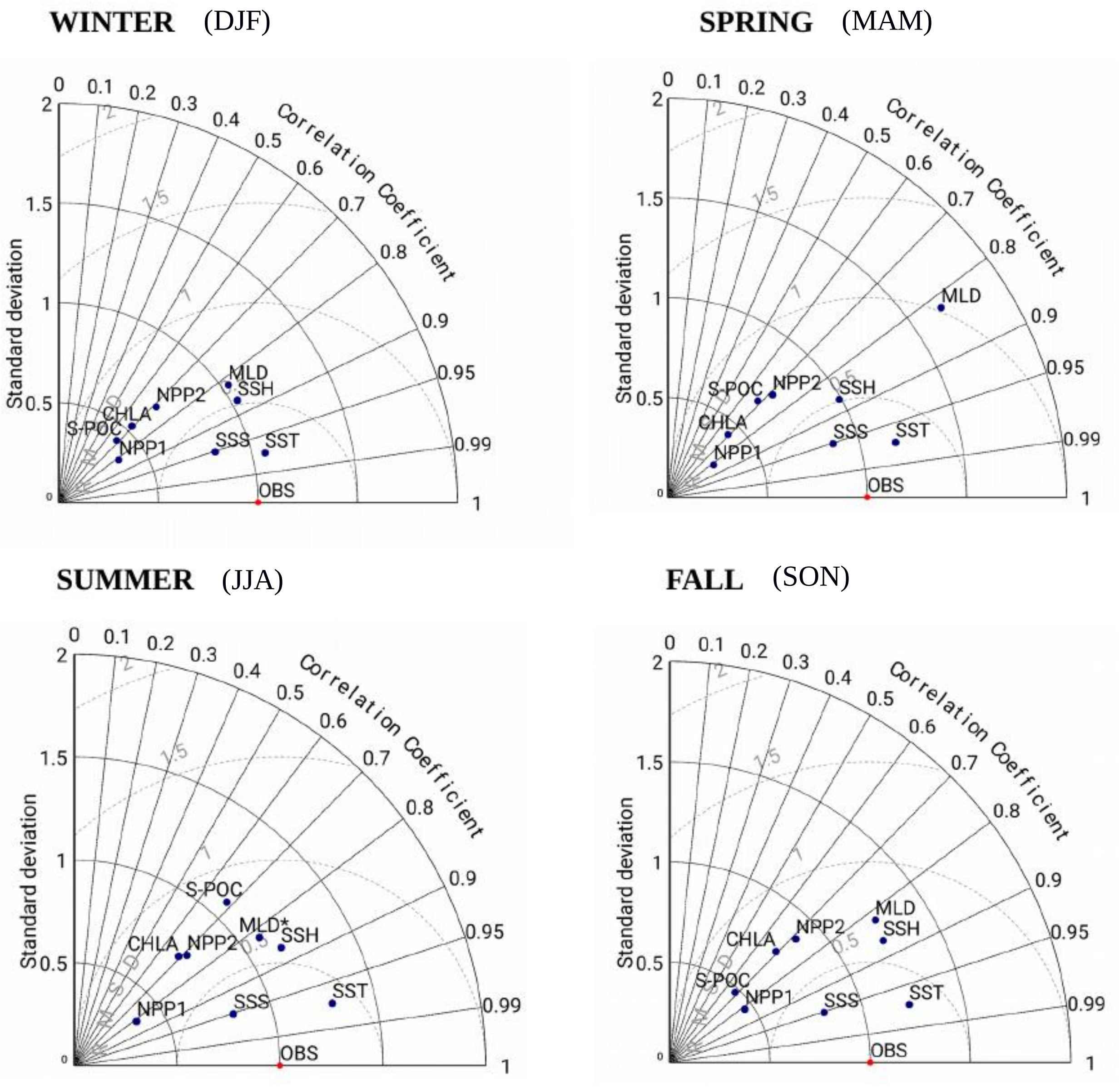

Figure MC1-4: Seasonal Taylor diagrams, including surface particulate organic carbon (S-POC) through a comparison with SeaWiFS satellite product estimate.

In the Summer diagram MLD was rescaled to MLD*=MLD/2. The summer MLD_{STD} is therefore 2 times as big as the one represented in the plot, while the correlation remains unchanged.

Major comment nr.2:
(2) Latitudinal partition of the domain
A) *In several places of the Introduction and Discussion the authors recognize that the Cape Verde frontal zone is a natural boundary between the subtropical and tropical domains. Nevertheless, for most of their analysis on latitudinal variability they use a partition in three areas or subregions, as shown in Figure 3b, which is not properly justified. I imagine this is done as an attempt to grasp the character of the meridionally convergent region near Cape Blanc but, as it is clear from the velocity fields in Figures 3 and 5, this is not correct. In my opinion only the southern subregion would comprise an area with approximately coherent dynamics. My suggestion here is to use four subregions of different size: the northern one (25- 32◦ N) would correspond to an area with substantial mesoscalar activity, with eddies and filaments generated both south of the Canary Archipelago and at the upwelling front; the second area would represent the permanent and intense central upwelling region (21-25◦ N); the third are would concentrate on the convergent region immediately south of Cape Blanc, which is the root of the Cape Blanc giant filament (about 17-21◦ N, though these limits change with longitude); the southern area (9.5-17◦ N) would correspond to the tropical region. Right now most of the discussion is either on the results for the latitudinal-average picture or (to a lesser degree) for the three proposed regularly-spaced subregions. With this alternative partition, the paper would certainly become much more informative.*
B) *I value very much the authors' efforts to provide bulk figures for the entire region but I think that plotting these results may be very misleading. For example, the data in Figure 8 suggests that the zonal flux of organic carbon is more intense than meridional one. I doubt this very much: in my opinion this is only an artefact that the latitudinal average tends to cancel the contributions of the southward Canary Upwelling Current and northward Mauritania Current and Poleward Undercurrent (please see references below regarding the main currents in the CanUS). My suggestion is to produce fewer plots on the results for the entire region (Figure 9 is fine but some other plots may be replaced by tables) and instead show what is happening in each area: the CanUS is so large that it surely deserves a closer view for each subregion.*

Answer to MC2:

A) We agree with Dr. Pelegrí that other choices for the subregional Budget Analysis domain were also possible. Our partition serves to quantify both the alongshore convergence of particulate organic carbon from both north and south of Cape Blanc and the subsequent intense offshore flow that takes place along the Cape Verde front. The use of wide domains allows us to have a more robust measure of the fluxes in a region of high mesoscale variability. This partition also avoids us to place boundaries in critical regions such as around the Cape Verde convergence; placing boundaries in such flux-intense regions would make the results of our budget analysis very sensitive to the exact latitude of the boundary.

However, we have considered the latitudinal partition proposed by Dr. Pelegrí, and repeated our analysis on his proposed domains, as shown in Figure MC2-1. The changes basically consists in a sub-division of the central domain into two smaller zonal bands. Our northern and southern zonal bands already satisfied Dr. Pelegrí's definitions, corresponding to a northern subregion rich in mesoscale activity (now only displaced by half degree) and a southern tropical subregion. The results of the new budget analysis are displayed in Figure MC2-2. As expected, northern and southern subregions are characterized by the same pattern of fluxes as those presented in the paper, since moving the southern boundary of the northern subregion by half a degree north does not affect the budget. The central subregion is split in a "central north" and "central south" zonal bands (green and orange lines). The impact of the offshore flux in these two zonal bands is very similar (Figure MC2-2, panel b). The flow of the Cape Verde front crosses the boundary between the "central north" and "central south" zonal band at about 1000km offshore, adding to the offshore flux in the "central south" subregion at this distance from the coast. However, this effect is an artifact generated by the split of the front in two segments. It thus does not add much to our understanding of the magnitude or the impact of the long-range offshore flux at these latitudes. As regard to the alongshore fluxes, we find that dividing the central subregion in two zonal bands does not clarify the source of the organic carbon that is exported offshore along the Cape Verde front. In fact, while before we could clearly identify the central subregion as a region of alongshore convergence of the organic carbon, now the budget for the "central north" and "central south" subregions depends strongly on the exact location of the intermediate boundary and the exact pathway of the Canary and Mauritanian currents near Cape Blanc. For example, in the 0km-100km offshore range, the "central north" subregion still exports southward more carbon than what it receives from its northern boundary, resulting in a net alongshore export to the "central south" subregion. In contrast, in the 100km-500km offshore range of the "central north" subregion a local recirculation of the carbon from the "central south" zonal band is visible in the meridional fluxes plot of Figure 11 of the paper. This recirculation induces a large net influx of organic carbon in the "central north" subregion. All these effects strongly depend on the exact location of the intermediate boundary in this region of intense flux convergence. As a consequence, we believe that the use of just one large central subregion for the budget analysis is more appropriate for our main purpose of understanding the magnitude and possibly the sources of the lateral offshore flux of organic carbon in the CanUS. To clarify the reasons that lay behind our choice of the domains used for the budget analysis, we will therefore include in the paper a clear and detailed explanation.

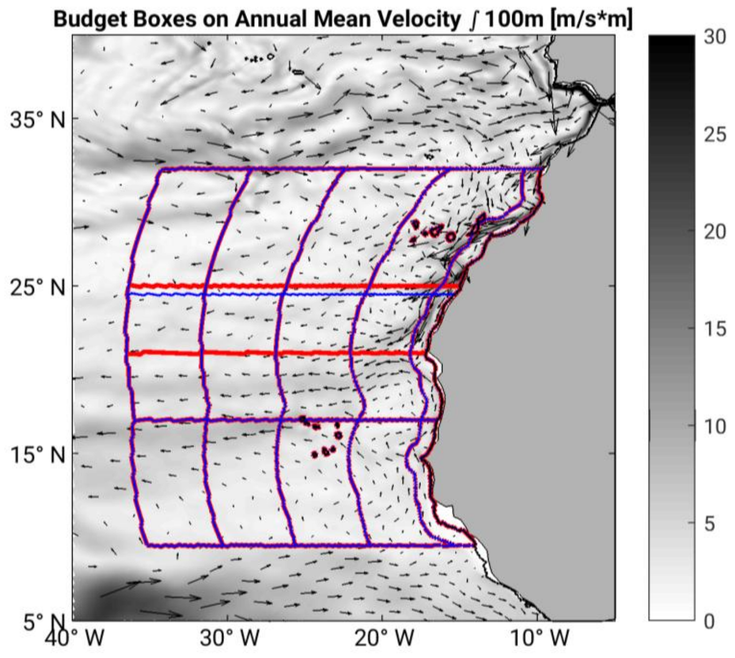

Figure MC1-1: Comparison between the latitudinal domains proposed by Dr. Pelegrí (red lines) and the domains used in the paper (blue lines).

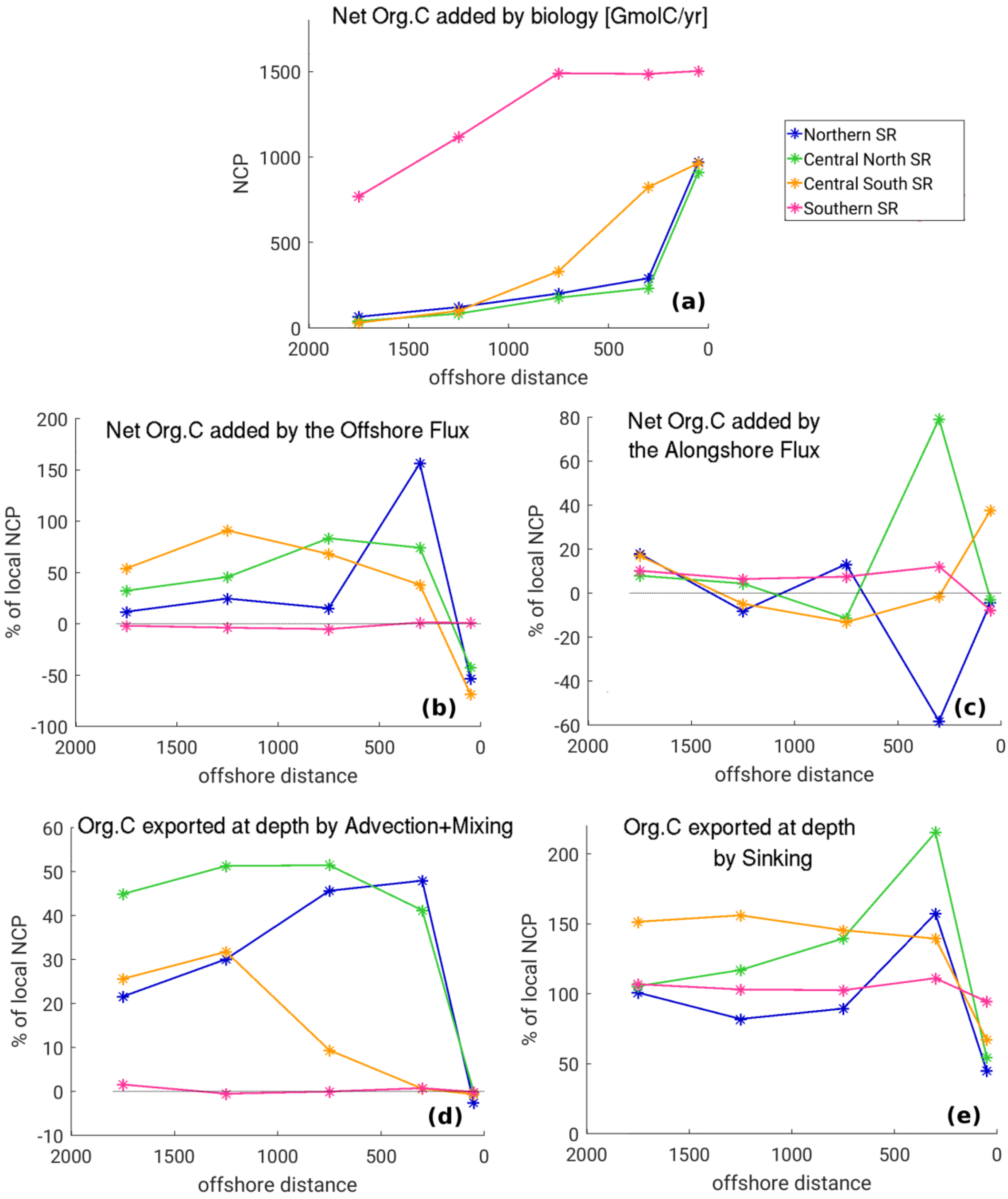

Figure MC1-2: Budget analysis results based on the subregions proposed by Dr. Pelegrí. Trends of NCP and impact of the organic carbon fluxes (divergence of the flux / NCP) by subregion.

B) We thank Dr. Pelegrí for this comment, and we agree with him on the fact that alongshore fluxes are locally more intense than the offshore fluxes in the nearshore. We also agree that, since Figure 8b shows the meridional fluxes averaged over the whole CanUS, the contribution of the northward and southward alongshore currents mostly cancel each other. In this specific case, will reconsider the picture and its description and we will discuss more explicitly the relative contribution of the zonal and meridional components of the fluxes. In general, we still believe that an in depth discussion of the bulk fluxes is very relevant for our purpose of quantifying the overall magnitude of the lateral fluxes from the North African coast on the North Atlantic gyre. For this reason, we plan to keep the original figures.

Major comment nr.3:

(3) Upwelling of coastal and offshore inorganic nutrients

The coastal upwelling region is a source of inorganic nutrients to the surface layers in the coastal transition zone that are later exported offshore (e.g. Pelegrí et al., 2006; Pastor et al., 2008, 2013). Such a flux of inorganic nutrients is a prime element in the offshore net primary production and the sign of the NCP north of Cape Blanc. However, this issue is not mentioned in the manuscript until the Discussion. The subject is important enough to deserve careful attention when examining the sources and sinks for NPP, it is the difference between new production using the subsurface load of inorganic nutrients or production after remineralization.

The offshore waters in the southern subregion are also largely affected by the presence of upward Ekman pumping, i.e. offshore upwelling resulting from positive wind-stress curl. Again this is an important aspect in the dynamics and NPP balance of this subregion, which is again acknowledged very late in the manuscript and only partly discussed.

The model could be used to assess these different contributions. Perhaps this was not the objective of the authors, which is fine, but then the potential relevance of the upwelling and transport of inorganic nutrients on the NPP and NCP within the entire region should be properly discussed since early in the manuscript.

We agree with Dr. Pelegrí on the importance of including a discussion of sources and sinks of nutrients in the region. For this reason we have decided to include in the paper a figure showing the lateral and vertical fluxes of inorganic nutrients (Figure MC3-1). This will highlight the importance of these fluxes and improve the discussion of the pattern and of the subregional variability of NCP, underlining in particular the importance of the coastal upwelling of nutrients in the northern and of the Ekman pumping in the southern Canary Upwelling System.

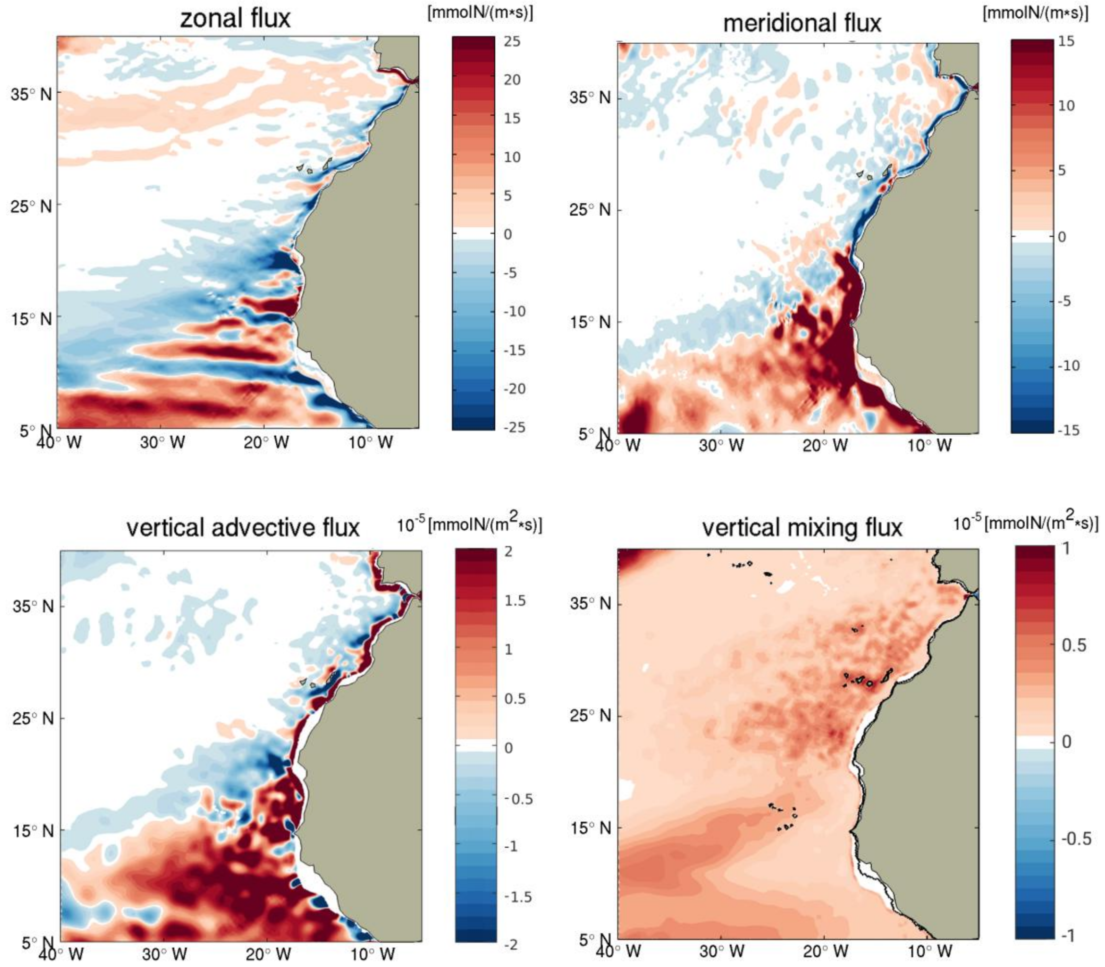

Figure MC3-1: Inorganic Nitrogen fluxes in the first 100m. Horizontal fluxes were integrated in the first 100m, while vertical fluxes were sliced at 100m depth.

Answers to Minor comments

(4) p 1, l 10: *divergence or convergence?*
The divergence is negative, which means flow is convergent.

(5) p 2, l 11: *replace "and can create" by "can create."*
Thank you, it was a typo. We will correct it.

(6) p 2, l 15: *"Arístegui."*
Thank you, it was a typo. We will correct it.

(7) p 3, l 5: *other relevant references are Pelegrí et al. (2006) and Pastor et al. (2013).*
Thank you for your suggestion.

(8) p 3, l 24: *also Pastor et al. (2013).*
Thank you.

(9) section on Methods: *how does the model calculate the vertical velocity?*
The vertical velocity is computed through integration of the mass-conservation equation of an incompressible fluid ($\vec{\nabla} \cdot \vec{u} = 0$) from the ocean floor upwards. For detail about the equations solved by ROMS and its numerical solution technique we refer to Shchepetkin and McWilliams (2005) and the ROMS Wiki (https://www.myroms.org/wiki/Equations_of_Motion).

(10) caption of Figure 1: *Gran Canaria is cited in the caption but not located in the map.*
Thank you, we will add the name in the figure.

(11) p 10, l 10-13: *please clarify.*
We will revise this sentence.

(12) caption of Figure 4: *VGPM is first mentioned here but it is defined nowhere in the manuscript.*
The used SeaWiFS VGPM product is described in detail in Table A3 (Appendix) among the products used for the Model evaluation. We will add a reference also in the text.

(13) p 12, l 21-24: *asides the Canary Current and the Mauritanian Current you should also probably refer to the Canary Upwelling Current (associated to the coastal upwelling jet) and the Poleward Undercurrent (please see references below).*
Thank you, we will include it in the description.

(14) p 12, l 31-32: *". . . NPP is a better measure than chlorophyll for evaluating. . ."*
Thank you, we will correct the sentence.

(15) p 13, l 1: *Pastor et al. (2013) is probably a better reference.*
Thank you for your suggestion.

(16) caption Figure 6: *panel b also includes sediment remineralization?*
Yes, all panels include remineralization. We will adjust the figure caption accordingly.

(17) p 18, l 3-5: *are you using two different definitions for excess export?*
No, ΔE is always defined as ΔE= vertical export - NCP

(18) p 20, l 1-2: *here and elsewhere it is best to not refer to lines, they should be defined in the figure's caption or legend (otherwise you would have to define them everywhere).*
Thank you, we will revise the references.

(19) p 20, l 27 and 33: *"north of the Cape Verde front. . ."*
Thank you, we will correct it.

(20) p 22, l 2: *". . .south of Cape Blanc."*
Thank you.

(21) *please revise caption of Figure 12.*
Thank you, we will revise it.

(22) Figure 13: *I suggest that you also show the meridional fluxes.*
Thank you for your suggestion. We evaluated these figures, and we have decided to add a discussion of the vertical sections and depth trend of the meridional fluxes by subregion in the text. However, we prefer not to include this plot, since it does not add any substantial information that cannot be inferred from the 2D plot of the meridional fluxes, Figure 11b.

(23) caption Figure 13: *"vertical" rather than "vertcal."*
Thank you. We will correct the typo.

(24) p 24, l 14: *"(Figure 13)."*
Thank you. We will correct the typo.

(25) p 25, l 6-8: *this is likely an artefact of the SW-NE orientation of the coast.*
Thank you, we will revise the paragraph to take this effect into account.

(26) p 25, l 10 and 15: *please include references.*
Thank you, we will add references .

(27) p 27, l 8: *see also Pastor et al. (2013).*
Thank you for your suggestion.

(28) p 28, l 7: *"these."*
Thank you, we will correct it.

(29) p 28, l 10-18: *usage of so many conditionals raises doubts on the reader.*
Thank you, we will revise the writing.

(30) p 28, l 27: *remove "from Section 4.1."*
Thank you, we will remove it.

(31) p 28, l 28: *is this the right way to cite a figure within a reference?*
We will revise the reference.

(32) p 29, l 3: *here and elsewhere separate numbers from units, i.e. "2000 km" rather than "2000km."*
Thank you, we will correct it.

(33) p 29, l 16-17: *please revise writing.*
We will revise these sentences.

(34) additional references: *asides those mentioned above, there are other works that would help better describe the circulation patterns in the CanUS, such as Mason et al.(2011), Peña-Izquierdo et al. (2012, 2015), Pelegrí and Peña-Izquierdo (2015), Pelegrí and Benazzouz (2015).*
Thank you for the suggestions, we plan to add these references.