The authors are grateful for the thoughtful comments given by the two anonymous referees on our paper. Below, we respond to each point raised by the reviewers and explain the changes we've made to the manuscript accordingly. The reviewers' comments are shown below in italics writing while our response is indicated in a red font.

Anonymous Referee #2

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We would like to thank the second reviewer for the comments that have, we hope, significantly improved our manuscript. Below, we highlight our responses, point by point, to the reviewer's general and specific comments and indicate the revisions we will make to the paper accordingly.

General comments

This study aims to determine the contribution of nitrogen upwelled within the coastal region of the Canary Upwelling System to the nitrogen budget of the open ocean through a Lagrangian study relying on model outputs generated by a coupled physical-biogeochemical experiment. Authors also aim at describing the timescales, the reach and the structure of this offshore transport to quantify the role played by upwelling on nitrogen enrichment of the NATR and NASE provinces as defined by Longhurst. I'm not sure the study makes a significant contribution to the issue of nitrogen irrigation of the NATR and NASE provinces. There are several reasons for this.

1) *First, the authors justify the originality of their study by the use of a Lagrangian approach as opposed to Eulerian approach which has been used in Auger et al. (2016) or Lovecchio et al. (2017, 2018) which have been mentioned in the present study. The authors in particular advance the capacity of their Lagrangian approach to define more faithfully the contribution of the coastal upwelling region in terms of nitrogen supply to the subtropical gyre (l5-10, page 4). This statement seems relevant with regard to the volume transported from the coastal region to the open sea, but much less obvious with regard to the transport of nitrogen. Indeed, the amount of nitrogen carried by each particle to a given location is quantified as the product of the particle associated volume and the concentration of the tracer associated with the particle when it reaches that location (l4-6 page 11). To my understanding of the methodology, the nitrogen concentration at a particular location does not necessarily come from the coastal upwelling but can be supplied locally, can change its chemical form or have a different origin.*

We do agree with the referee that in contrast to water volume, tracing the transport of nitrogen is somewhat more difficult given the chemical transformations between inorganic and organic nitrogen. In addition, subgrid mixing is not represented in our Lagrangian particle tracking but can affect nitrogen concentrations in ways that are not accounted for in our transport estimates.

Yet, as has been shown also by Frischknecht et al. (2018), the Lagrangian method permits a lot of new insight into the offshore transport of nitrogen, since total nitrogen, i.e., the sum of inorganic and organic nitrogen is conserved except for the part that is sinking, and that part that is being supplied through mixing. Indeed, along the way, nitrogen can be incorporated into organic matter and then being recycled again, but if it is tracked by our algorithm, this nitrogen is still coming from the coastal upwelling.

The component we lose through sinking does not affect our conclusions, since this component is lost to the ocean interior, from where it will not find its way back into the waters that are transported offshore. More importantly is our lack of consideration of the vertical mixing. We have good evidence that this component is relatively small. First, the total amount of nitrogen is decreasing with offshore distance, and not increasing. In fact, the decrease is driven entirely by the sinking component, and the spatial distribution of this loss fits well the spatial distribution of the export of organic nitrogen (Figure 7). In particular, we see a decline in total nitrogen as a function of distance to the coast that is sharper than for water volume (Fig 7). And this decline is larger for water particles originating from the southern subregion that are transported at the shallowest depths (Fig 9). If the supply of nitrogen from surrounding waters to upwelling waters due to mixing were large enough to cancel the loss due to organic matter sinking, there would be no such a sharp decline in the offshore transport of nitrogen as a function of distance to coast. This suggests that although the potential changes in nitrogen due to subgrid mixing can locally be important, they are unlikely to affect the large-scale transport estimates in a significant way.

Yet, we acknowledge that the lack of a representation of mixing in Ariane is an important caveat that not only can affect particles' depth, but also potentially their nitrogen content and hence locally our offshore transport estimate. This will be stated explicitly in the discussion of the method caveats.

2) *In addition, authors indicate some limitations of the biogeochemical model (absence of colimitation, absence of nitrogen fixation; l30-34, p31) but omit the potential role of different communities of phytoplankton. Indeed, the model used only represents a single phytoplankton community, the representation of diatom organisms (comprising a siliceous skeleton and likely to contribute significantly to the export of organic matter) could influence the export in the model. In terms of export, it has also been shown that the alternation of phase of intensification and relaxation of the upwelling favorable winds is important for the dynamics of the upwelling systems (significant efflorescence generation and sedimentation). The use of climatological wind in this study is likely to play a role in the results because it does not represent these alternations. These aspects should be mentioned in the limitations of the study.*

We agree that similar to other state-of-the-art models, our model (especially the biogeochemical module) has other limitations beyond what we have already acknowledged in the original version of the manuscript. Yet, the fact that the simulated distributions of nitrate and its seasonality agree relatively well with the observations (as shown in the new validation figures; see also our response to comment 4 by reviewer 1) suggests that the impact of these limitations on the study conclusions is likely limited.

Nevertheless, following the referee's suggestions we have added two additional potential model limitations in the caveat section: 1) the fact that the model does not represent multiple phytoplankton groups and 2) the use of climatological winds lacking high-frequency variability that may lead to a misrepresentation of some aspects of the complex upwelling dynamics.

3) *Then, the conclusions of the study highlight the importance of the Capes in the generation of filaments which represent privileged export sites but the influence of topographic accident on the generation of filaments has already been studied theoretically (eg Meunier et al., 2010), through hydrodynamic simulations and observations for certain filaments of the Canary upwelling system. The quantification of the overall contribution of filaments and the extension of the source waters supplying the main filaments of the system nevertheless provides interesting information, even if the three-dimensional dimension of upwelling is becoming more and more essential in the literature targeting these upwelling regions.*

We agree with the referee in that previous studies like Meunier et al. (2010) and Troupin et al. (2012) have demonstrated the importance of coastal topography and capes in particular in the formation of coastal filaments. Therefore, we will add references to these previous works in the revised manuscript.

In particular:

- on page 22, Ln 14, we will change the statement :"Coastal filaments along the West African coast can occur everywhere, but it is well established that the majority of the filaments are persistently associated with the major capes along the coast" to "Coastal filaments can occur everywhere anywhere on the coast in the CanCS, but previous studies have shown that capes can facilitate their formation (Meunier et al., 2010; Troupin et al., 2012)".
- on page 24, Ln 10, we will cite Meunier et al. (2010) and Troupin et al. (2012) to highlight that the alongshore advection can interact with capes to result in the formation of a coastal filament that then exports upwelled water to the open ocean.

4) *The role of mesoscale activity on residence times and the kinetics of transport from the coastal zone to the open sea is also part of the presented results. Mesoscale activity in the transition zone has been widely studied in all eastern boundary upwelling systems and fairly exhaustively in the northern part of the Canary system, in particular from the ROMS model (Mason et al., 2011 & 2012 ; Troupin et al., 2012).*

We agree that previous studies mentioned by the referee have studied the mesoscale variability in the northern Canary system. We will add references to these papers in the revised manuscript.
In particular, we will cite:

- Mason et al. (2011) in section 5 among the papers we cite showing mesoscale variability and overall transport complexity in the Canary.

- Mason et al. (2012) will be cited in our literature review in the introduction section and section 5.2 on recirculation.

- Troupin et al. (2012) will be cited in our description of the Cape Ghir filament.

- Barton and Aristegui (2004) is an additional reference that we will add on mesoscale activity in the Canary (see subsection 5.2).

5) *In the southern part of the area studied, the underestimation of EKE (Figure 1), an activity also highlighted by the occurrence of eddies in this region (Schutte et al., 2016), is not mentioned and is likely to impact the results in this region. The literature on the region is also to be completed, in particular to take into account recent studies by German, Senegalese and French teams. This update particularly concerns the southern part of the system which would allow the authors to describe their results more precisely. Hydrological conditions off Mauritania are described in Klenz et al. (2018), the vortex activity is studied in Schütte et al. (2016), the understanding of the dynamics of the Mauritanian current was revisited by Kounta et al. (2018), and the functioning of the Senegalese upwelling by Ndoye et al. (2014, 2015, 2017) or Capet et al. (2017). These studies point in particular to the importance of the Mauritanian current (to which I prefer the name West Africa Boundary Current; Kounta et al., 2018) on the dynamics of upwelling.*

   We agree with the referee that the model underestimates the EKE in the coastal area of the southern subregion, and particularly so south of Cape Verde. Following the referee's comment, we will explicitly mention the underestimation of the EKE in that region and its potential implications among the study's caveats.

   Following the referee's suggestion, we also have expanded our review of the literature in the region. In particular, we will cite the following works in the revised manuscript:

   - Schutte et al. (2016) and Kounta et al. (2018): will be cited in subsection 5.2 (as well as in the Introduction section) to highlight the potential importance of the Mauritanian current and eddies in the region in fueling offshore export of water.
   - We also cite Glessmer et al. (2009) and Peña-Izquierdo et al. (2015) to emphasize the importance of the Mauritanian current in the Introduction section.
   - Klenz et al. (2018): will be cited in section 5.1 to highlight the importance of both the poleward (Mauritanian) current as well as the equatorward Northern Atlantic (Canary) current, particularly during winter, as a source of upwelling in the southern subregion. This helps explain the patterns in Figure 13 for the source waters of exports at capes in the southern subregion.
   - Ndoye et al. (2017) and Capet et al. (2017): will be cited in our description of the Cape Verde filament and the importance of the local mesoscale activity in section 5.2.

6) *Finally, questions remain as to how to assess the contribution of nitrogen from coastal waters to new production. Indeed, I did not understand the use of VGPM models to quantify primary*

production knowing the large differences that exist in satellite-based models of primary production in the region (Gomes-Letona et al., 2017).

We used satellite-based (VGPM) NPP estimates because of their synoptic-scale coverage, which individual in-situ estimates lack. However, we are aware of the important uncertainties associated with this (and other satellite-based) product(s). Therefore, we will add productivity estimates based on in-situ measurements that are available for some Longhurst provinces (Tilstone et al., 2009) as well as estimates from the CbPM model (Westberry et al., 2008).

Indeed, in-situ estimate of primary production for the NATR based on Carbon-14 uptake from Tilstone et al. (2009) will be added. This estimate (1377mmol N m-2 yr-1) is less than that derived from satellite data (1753 mmol N m-2 yr-1). This suggests that the contribution of the Canary upwelling nitrogen supply can be locally even more important in relative terms, than what our initial estimates have implied. Using a carbon-based productivity model (CbPM), NPP estimates for the NATR and NASE both amount to around 2000 mmol N m-2 yr-1. This also suggests that the CanCS's contribution to the NASE is locally more important than what the province's VGPM value (2139 mmol N m-2 yr-1) suggested.

We will update Table 4 and the discussion of the potential contribution of the nitrogen transport to the total NP estimates in the NATR and NASE provinces accordingly.

Finally, Gomez-Letona (2017) compares three alternative estimates of PP and finds divergences between them. However, their estimates and comparisons are focused on the coastal area while we were primarily interested in finding estimates for the larger adjacent Longhurst provinces so that we could compare offshore transport of nutrients in our model with the provinces' total budgets.

7) *The manuscript is however well written, well illustrated with clean and condensed figures, the methodology is well described, and the main messages are clearly presented.*

   We thank the referee for his/her positive and encouraging comment.

8) *As a summary, the manuscript is of good quality but I hardly consider the results as really moving our understanding forward. The methodology that uses Ariane as a Lagrangian tool is supposed to make a difference in the description, quantification of nitrogen irrigation of NATR and NASE provinces but I'm not convinced that it solves the issues faced by an Eulerian approach.*

   Again we thank the referee for praising the quality of the manuscript. We believe that while it comes with important limitations, our study does improve the quantification of the coastal upwelling supply of nitrogen to the open ocean, relative to the Eulerian approach. We detail our reasoning in our response to the previous comment #1by the same reviewer.

Specific comments

9) *L21-23, page 2: Reformulate the sentence "Especially low-latitude ..." which is hardly understandable.*

   We will make this change.

10) *L12, page 3: The current of Mauritania must be considered in the light of the work of Kounta et al. (2018).*

    Done. See response to comment 25above.

11) *L30, page 4: Did you use ROMS or CROCO oceanic modeling system?*

    We used ROMS-AGRIF version 3.1.1 (which shares the same code with the current version of CROCO).

12) *In this section 2.1.1, indicate the shallowest depth used at the coast (hmin parameter).*

    We'll indicate the 50m lowest bathymetry in section 2.1.1 accordingly.

13) *L26-28, page 5: EKE in the southern part of the domain is underestimated, please tell it and justify it.*

    We indicate in the revised manuscript that the model underestimates EKE in the southernmost part of the CanCS region as well.

14) *L30-31: A warm bias seems to occur in the south, maybe a map of SST differences would make biases straightforward for the reader.*

    We will include a map of SST difference that indeed shows a positive bias of less than 1C in the southern part of the domain.

15) *Figure 1: Arrows on a) and b) are almost invisible.*

    This will be corrected.

16) *Figure 2: Validation on annual field does not inform on the ability of the model to correctly simulate the upwelling occurring in the southern part of the domain at the winter-spring time of the year. I believe it would strengthen confidence on the simulation to add this component.*

    Seasonal evaluation figures will be added for sea-surface temperature, sea surface cholorophyll mixed layer depth as well as vertical sections of temperature, salinity and nitrate (see also our response to comments 4 and 25 by referee #1).

17) *L6-7, page 11: Why not telling here why you chose 70 m depth as upwelling criteria rather than explaining the reason much later.*

We will move our justification for using the 70m depth here (page 11).

18) *L14-15, page 13: the description of the upwelling does not fit with the dynamic of the upwelling in the southern part of the domain (Ndoye et al., 2014, 2015, 2017; Capet et al., 2017)*

We will correct this statement to" "Similarly, the Ekman-driven upwelling in the southern subregion is restricted to the winter and spring (Pelegri and Benazzouz, 2015; *Capet et al., 2017*)"

19) *L4, page 18: rather 300 km than 200 ?*

This will be updated to 300km.

20) *Section 6: NPP and regenerated production are calculated by the coupled model. Why authors use satellite-based models here?*

Data-based NPP estimates are used because the model domain covers only partially the NASE and NATR provinces. We will added new in-situ based NPP estimates from Tilstone et al. (2009) and a new satellite-based NPP product (Westberry et al., 2008) (please also see our response to previous comment (6)

21) *L31-34, page 31: Authors indicate some limitations of the biogeochemical model (absence of colimitation, absence of nitrogen fixation; l30-34, p31) but omit the potential role of different communities of phytoplankton. Indeed, the model used only represents a single community of phytoplankton, the representation of diatom type organisms (comprising a siliceous skeleton and likely to contribute significantly to the export of organic matter) could influence the export in the model. In terms of export, it has also been shown that the alternation of phases of intensification and relaxation of the upwelling favorable winds is important for the dynamics of the upwelling systems (blooms and sedimentation). The use of a climatological wind in this study is likely to play a role in the results because they don't represent these alternations.*

We acknowledge two additional model limitations in the caveat section: 1) the fact that the model does not represent multiple phytoplankton groups and 2) the use of climatological winds lacking high-frequency variability that may lead to a misrepresentation of some aspects of the complex upwelling dynamics. Yet, the fact the model is able to represent the observed large-scale distribution of nitrate and its seasonality (in the original and new validation figures above) suggests that the impact of these limitations on the study conclusion is limited. It is also important to note that when it comes to the issue of offshore transport, the specific nature of the phytoplankton community is of secondary importance. What matters much more is the role of dissolved organic matter, whose production could be related to phytoplankton community structure, but likely only weakly so. Please see our response to previous comment 2.

22) *Affirmation lines 33-34 is true on an annual basis but could be false during the monsoon season when subtropical warm depleted open ocean waters invade the shelf in the southern part of the Canary Upwelling System.*

As our focus is on the annual-mean nitrogen transport we keep this statement.

23) *L16-20, page 32: I agree that the western section is much more extended than the northern and southern exits but the role played by the West Africa Boundary Current (Mauritania Current here) plays a quite important role in cross-shore exchanges, it should be taken into account.*

We answered a similar comment made by Reviewer #1 (please see response to comment 25 by the first referee).

24) *L16-19, page 33: The final conclusion states that this study emphasizes the need for improving the resolution of eastern boundary currents in global coarse resolution models. I think it would be fair to cite at least Large and Danabasoglu (2006) who stressed this point 15 years ago.*

We will cite Large and Danabasoglu (2006) as a previous study pointing towards a similar conclusion.