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Discussions

Interactive comment on "A Lagrangian study of the contribution of the Canary coastal upwelling to the open North Atlantic nitrogen budget" *by* Derara Hailegeorgis et al.

Anonymous Referee #2

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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. 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. 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. 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. 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). 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.

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).

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.

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.

Specific comments

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

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

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

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

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

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

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

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.

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

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)

L4, page 18: rather 300 km than 200 ?

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

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. 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.

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.

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.

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