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

Associate Editor Decision: Publish subject to minor revisions (review by editor) (21 Sep 2020) by Minhan Dai

Comments to the Author:
Dear Authors,

I have now had your revised MS further evaluated by two reviewers. Both reviewers appreciated your efforts during the revisions. However, both reviewers also pointed out that your paper has room to improve in its presentation. In addition, and as per the comment from reviewer #2 regarding Lagrangian vs Eulerian approaches, please explicitly state in your MS the limitations of the Lagrangian approach.

Sincerely,

Minhan Dai
Editor

We deeply thank the editor for handling our manuscript and for his comment.

Following the reviewers suggestions we have improved the presentation of the paper by shortening and better focusing the introduction as suggested by reviewer #1. We have also added some additional clarifications to the description of the Lagrangian experiment as requested by reviewer #1.

As for the Lagrangian approach limitations associated with the lack of an explicit representation of subgrid mixing, we would like to highlight the fact that we fully acknowledge this caveat in the discussion section (please see line 34, page 25 to line 21, page 26).

However, contrary to what Reviewer #2 comment may seem to suggest, we believe the bias associated with this to be small. In fact, given the high spatial and temporal resolution of our velocity field, there is effective vertical mixing occurring as a result of vertical advection and resolved mesoscale and submesoscale dynamics (e.g., secondary ageostrophic circulation around fronts) that is captured by the Lagrangian experiment. We also cite Frischknecht et al. (2018), who have found highly consistent budgets between Eulerian and Lagrangian frameworks they applied in their analysis of the California Current System. We now further stress this in the discussion (please see lines 11-15, page 26). We also have included the following additional statement in the introduction to further highlight these potential limitations of the Lagrangian approach (lines 30-33, page 3):

"However, a Lagrangian approach also comes with a number of disadvantages. Perhaps the most important one is the difficulty to fully take into account subgrid mixing that is associated with the model treatment of unresolved physics."

Report #1

Submitted on 02 Sep 2020
Anonymous Referee #1

The manuscript by Hailegeorgis et al. substantially improved from it's first version. The authors did a good validation of the ocean-biogeochemical model and wrote a nice discussion (section 8), which helps to contextualize better the work contribution.

We are grateful to the first referee's efforts in reviewing the manuscript. Below, we highlight our point-by-point responses to the reviewer comments and explain the revisions we've made to the paper accordingly.

However, the paper still need some extra work. Specifically, the description of the model experiment and results still need clarification. This is highly relevant to capture the paper's take home messages. Besides, an additional effort should be done to improve the Introduction section, reducing wordiness and improving paragraph's flow. I would recommend five well written paragraphs instead of seven. Specific comments are indicated below:

Done. We have significantly shortened and better focused the Introduction, reducing wordiness and dropping unnecessary or redundant information. In particular, we have reduced the Introduction to five paragraphs instead of seven initially (please see page 2, lines 7-11; page 2, line 33 to page 3, line 8; page 3, lines 31-33).

Page 5: The authors provided a scheme to explain better the experiments, but this scheme seems to me a little bit confusing. I am wondering why they defined those 3 modeling steps. I also wonder why the second step was called "qualitative", although this step quantifies position, velocity, and tracer concentration. It may be helpful to state clearly the goal of each step in page 5. Besides, it could be helpful to explicitly link the steps with the results presented in sections 3-5. I suggest moving an improved experiment diagram to the paper main body.

The quantitative and qualitative experiments referred to in the initial revision of the manuscript are names used in ARIANE to describe two types of Lagrangian experiments, that either only save the initial and final positions and volumes associated with particles crossing some predefined entry sections (Quantitative) or the

entire trajectories associated with each particle (Qualitative). In our case, we use the former to identify the location, time and volumes of particles at their release/seeding and the latter to track them along their trajectories. We agree with the referee that this naming may be a source of confusion for people not familiar with the ARIANE jargon. Therefore, we dropped these names and focused on the goal of each step, as also suggested by the referee. We have modified the labels of the three stages of the experiment (page 5, lines 20-21) and edited the names of the stages in the subsequent description accordingly.

As we have clarified in the text the three steps involved in the Lagrangian modeling experiments, we have dropped the schematic in the supplementary information as it mostly brings redundant information. We have, instead, added a brief clarification of the steps in Figure 1c.

Page 6: Suggestion: "Characteristic of upwelling" => "Upwelling patterns"

Done.

Page 9: Suggestion: "But the offshore distribution of the upwelling" => "But the upwelling zonal distribution"

Done.

Fig S22: Do these patterns represent the distribution of (1) upwelled particles at a given time, or (2) the locations where particles were upwelled above 70 m depth? Please clarify.

The patterns represent the location where upwelling occurs. We have changed the caption to "Zonal distribution of upwelling volume (a-c) and nitrogen content (d-f)…"

Page 10: Suggestion: "the median time for particles to traverse a given offshore distance interval" => "the median particle's travel time for a given offshore interval"

Done.

Page 10: I am wondering what was the impact of islands in the offshore transport of water and nitrogen. This is an aspect that was not discussed in the paper.

The role of islands is not explicitly studied in this work. However, in our discussion of the role of recirculation we refer to the role of eddies entrained by the Canary Archipelago (Canary Eddy Corridor) as a potential contributor to the prominence of the indirect transport of upwelled waters in the central and northern subregions. Please see (page 20, line 16 to page 21, line 4):
"The importance of the indirect transport of upwelled waters in the central and northern subregions can be linked to the prominent role played by mesoscale eddies there. Indeed, eddies with length scale of 100 km to 300 km are known to be important at these latitudes (Mittelstaedt, 1991). These include a recurrent cyclonic

eddy south of Cape Juby and the cyclonic and anticyclonic eddies entrained by the Canary Archipelago, forming the so called Canary Eddy Corridor (CEC), which is located at 22º –29º (Arístegui et al., 1994; Piedeleu et al., 2009; Sangrà et al. 2009). This region of long-lived westward-propagating eddies is known to contribute strongly to the offshore transport of organic matter and carbon (Sangrà et al., 2009)."

Page 11, line 6: I'm lost, why is that coastal upwelling area the most sampled?

Done.

Page 14: Suggestion: "4.4 Structure of offshore transport" => "4.4 Vertical and horizontal structure"

Done.

Report #2

Submitted on 07 Sep 2020
Anonymous Referee #2

We are grateful to referee #2 for his/her efforts in reviewing the revised manuscript. Below, we highlight our point-by-point responses to the reviewer comments and explain the revisions we've made to the paper accordingly.

I am still not completely convinced of the level of added value brought by the Lagrangian approach compared to the Eulerian approach. In their response, authors acknowledge the "lack of consideration of the vertical mixing". But they continue saying that "We have good evidence that this component is relatively small ...". However, in the region centered on the Cape Blanc, which is the region that exports an important amount of nitrogen from the coast to the open ocean, Auger et al. (2016; NSB and SSB regions) show that vertical diffusion is the second most important nitrogen source and sink terms. This term represents about 20% of the inputs in the 0-100 m layer, with sedimentation representing about 50%. The Lagrangian approach

thus also contains flaws and uncertainties (see also the list of caveats) and I am not sure that this quantification of the upwelling region's contribution to new production in the NATR and NASE regions adds much more than what the circulation patterns and processes affecting the different biogeochemical provinces tell us.

We agree with the reviewer that the lack of subgrid vertical mixing is an important caveat that we fully acknowledge in the discussion section (please see line 34, page 25 to line 21, page 26). However, we believe the bias associated with this limitation to likely be small. In fact, given the high spatial and temporal resolution of our velocity field, there is effective vertical mixing occurring as a result of vertical advection and resolved mesoscale and submesoscale dynamics (e.g., secondary ageostrophic circulation around fronts) that is captured by the Lagrangian experiment. Additionally, we have good evidence that this component is likely small. We cite Frischknecht et al. (2018), whose experiment on the California Current System found highly consistent budgets between Eulerian and Lagrangian frameworks. We have added a description on this on page 26, lines 10-15. Nevertheless, we have included an additional statement in the introduction to further stress the disadvantages of the Lagrangian approach (please see lines 30-33, page 3).

Nevertheless, the authors have shed light on all the questions asked, have taken into account most of the suggestions and have improved the first version of the paper.

We thank the reviewer for his/her positive comment.

Some expressions and typos will probably have to be checked during the editing phase. Among them:

L13, page 5: "50is" should be "50 m is"

Done.

L14, page 7: Fig. 1c and not 1b

Done.

L9, page 9: Fig. 1c and not 1b

Done.