Authors's reply to Anonymous Referee #1 comments on bg-2021-38 Tzortzis et al.

Dear referee,

Thank you very much for your constructive comments and suggestions, as well as your English corrections. Biogeoscience has given its green light to revise our paper. So we have reworked the manuscript, taking into account your suggestions and those of the other reviewer. In order to follow our reply, your comments have been copied here in blue, after the '==>' symbol.

General comments

==> The manuscript of Tzortzis et al. constitutes an interesting analysis on phytoplankton community dynamics in response to a frontal region in the Western Mediterranean Sea. Extensive in situ datasets are used to characterise the hydrodynamics of the region, and provide insights on the response of phytoplankton community structure to fine-scale ocean dynamics associated with the frontal region. Generally, the results are fairly well presented and are interpreted appropriately in the discussion and conclusions. One of my major issues is that the manuscript contains superfluous information/analysis at times. There are many figures and different types of analysis presented, but the authors do not summarise all of these findings in a succinct and logical manner in the discussion. In some cases, the text/analysis can be condensed and moved as supplementary material, removed, or expanded upon. Finally, although the grammar is generally OK, I would recommend it for a check by an English editing service if possible. I recommend the manuscript for major revisions prior to publication in Biogeosciences.

You have highlighted that the major problem in our manuscript was some superfluous information, in particular in the Results and in the Discussion. We thank Reviewer 1 for this concern and have modified these parts taking into account your suggestions and those of the other reviewer as well in order to improve the manuscript. Indeed, in the revised version of our manuscript, we have separated the Discussion and the Conclusion, and we also have divided the Discussion into several subsections to help the reader.

In the first subsection of the Discussion (part 4.1 “Physical properties of the front”), we have developed our ideas about the estimation of vertical velocities in our case, and then we have dug deeper into our interpretation about the two AW observed and their role in the structuring of the front.

The second subsection (part 4.2 “Biogeoschemistry”) resumes some points that we had mentioned in the Results section of the initially submitted manuscript. In this revised version, we have more detailed the implication of these results for our study.

In the third subsection (part 4.3 “Physical-biological coupling in the frontal area”), we have clarified the role played by the front physical forcing on the distribution of phytoplankton abundances.

Finally, in our “Conclusion and Perspectives” (part 5), we have added some sentences (lines 438-446) in order to clarify and highlight the novelty and the implications of our study.

Furthermore, we have combined figures, following your suggestions and those of the other referee (see figure 5 in the new manuscript). Some figures have also been moved in Appendices (see figures A1, A2, A3, A4 and A5).

Specific comments

Abstract

==> Line 7: Another word to replace “towed fish”? I presume you mean the SeaSoar?

==> Line 7: I think you can rephrase the sentence to: “Multi-parametric in situ sensors mounted on the vessel, a towed fish/SeaSoar instrument and an ocean glider”

Yes, you are right, we deployed a SeaSoar; we prefer to avoid the use of the commercial name in the abstract (it is specified in the Method section) and we modified the sentence by using “towed vehicle”, see line 7 (page 1) in the revised manuscript.

==> Line 8: Remove “A” before “particular attention”

We have corrected that, see line 8 (page 1) in the revised manuscript.

==> Line 14: “Phytoplankton community structure”?

We have modified the end of the abstract taking into account the comments of the other reviewer, in order to highlight the originality of our study. See lines 13-18 (page 1) in the revised manuscript.

Introduction

==> Line 17: rephrase to “oceanic ecosystems” and remove “the”.

==> Line 18: The word “compartment”. Perhaps it is possible to find another alternative here?

==> Line 19: Global climate change?

We have modified the first sentence of the introduction, see lines 20-23 (page 2) in the revised manuscript.

==> Line 19: Since several years? Satellites have been acquiring observations of ocean colour/phytoplankton biomass for at least 2/3 decades. I would rephrase here. Also, I think you can remove “of phytoplankton”

See line 24 (page 2) in the revised manuscript.

==> Line 22: I think the line following “the term fine scales” needs the addition of commas e.g., The term "fine scales" refers here to ocean dynamical processes that occur on horizontal scales of the order of 1–100 km, are characterized by a small Rossby number, and have a relatively short lifetime from days to weeks”.

Done, see lines 27-28 (page 2) in the revised manuscript.

==> Line 25. “Fine scale” should be “fine scale features”?

Done, see line 29 (page 2) in the revised manuscript.

==> Line 27. See previous comment

Done, see line 31 (page 2) in the revised manuscript.

==> Line 31. “In respect to bulk production”. I think this can be removed, as you begin a new sentence talking about diversity as opposed to primary production…

Done

==> Line 32. I would replace “Indeed”, with “however”.

Done

==> Line 32. “effect of the fine scales” – not grammatically correct. You mean “effect of fine-scale oceanic features”? Please check and fix this throughout the manuscript.

Done

==> Line 34. I don’t think “in situ samplings” is grammatically correct. Please check and modify if necessary.

We have put it in the singular form (see line 38 (page 2) in the revised manuscript).

==> Line 37. “associated to” should be “associated with”

Done, see line 41 (page 2) in the revised manuscript.

==> Line 38. Comma needed after “surface ocean”

Done, see line 42 (page 2) in the revised manuscript.

We have also added a sentence (lines 57-60 (page 3) in the revised manuscript) to highlight the originality of our study, following the comments of the other reviewer.

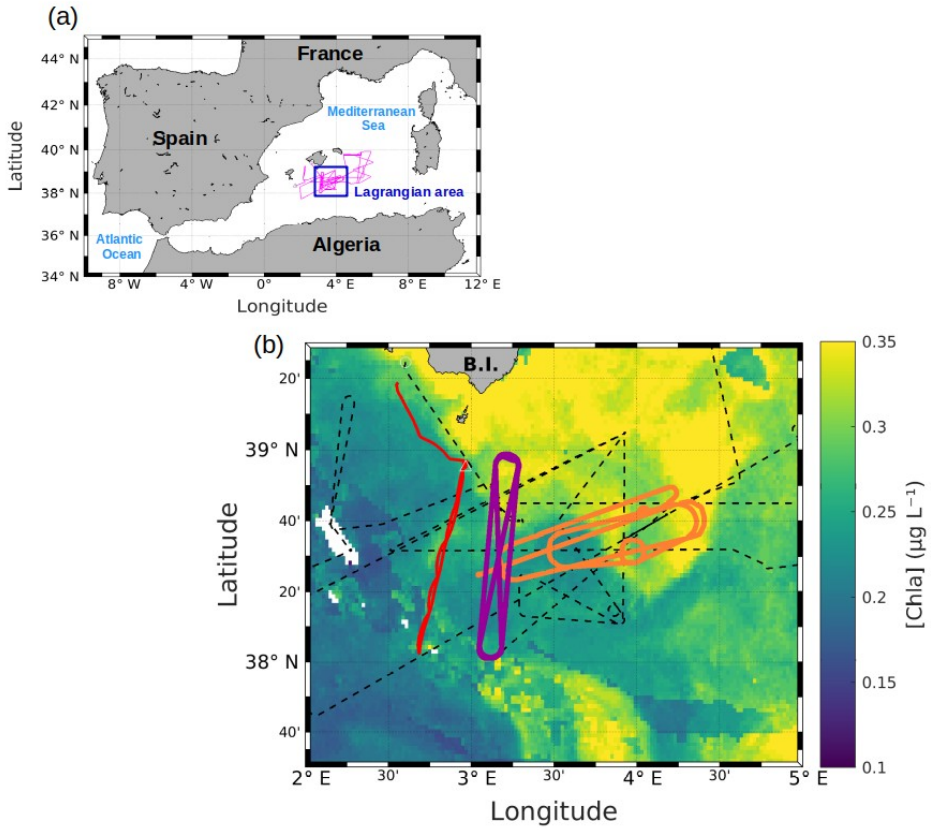
We have also replaced “low” by “moderately” (lines 56, 66 in page 3 and also in the title), following the comments of the other reviewer.

Materiels and Methods

==> If I am not mistaken, you selected the two sampling trajectories based on two regions of Chl-a concentration using the satellite-based SPASSO tool. Based on Figure 1, I can see a region of high Chl-a corresponding to the WE transect, but have trouble distinguishing the second region of unique surface Chl-a that justifies the position of the NS transect. Perhaps it is the colour scale/colorbar limits. Are the regions also selected based on SST and currents? In any case, I would rephrase or try and be more specific of why these two sampling transects were selected, and which areas you are referring to. I believe the colour scale can be improved to highlight this.

We have modified this figure, see figure below (i.e., figure 1 (page 24) in the revised

manuscript).



(a) Route of the RV Beautemps-Beaupré during PROTEVSMED-SWOT (pink line). The blue box corresponds to the area sampled with Lagrangian strategy. (b) Map of satellite-derived [Chla] provided by CLS for 3 May 2018, selected in the Lagrangian area and superimposed on the route of the ship (black dotted line). The orange and purple lines delimit the two areas called "hippodromes": West-East (orange) and North-South (purple). The red line represents the route of the SeaExplorer glider.

==> I am not really familiar with FSLEs or Langrangian techniques. Thus, out of curiosity, is the FSLE a commonly used index for detecting fronts/fine scale features? Can you provide citations supporting this?

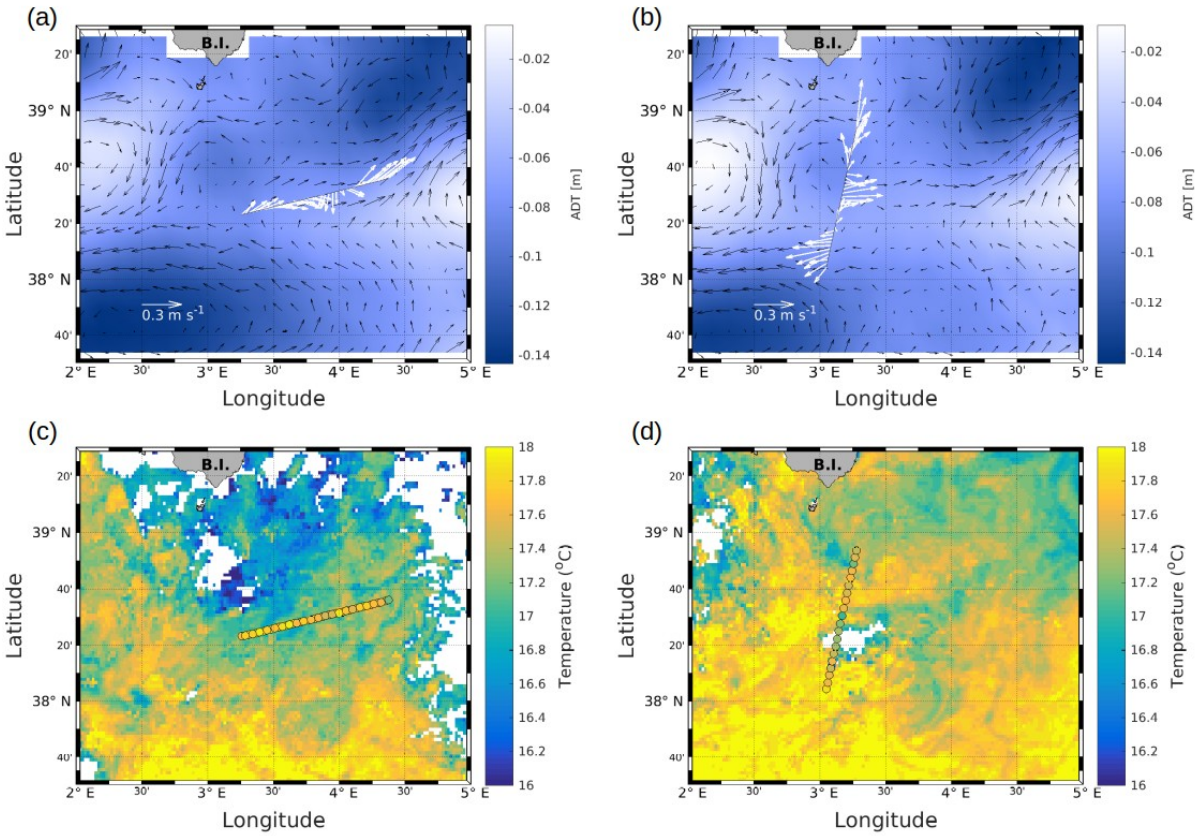
The first study that showed the interest of using FSLE-derived fronts for biogeochemical studies was probably Lehahn et al., 2007 (see in particular their Fig. 8 and 9). Before, Abraham and Bowen (2002) have been the first to apply the Lyapunov exponent technique (although finite-time, not finite-size) to the ocean, in turn borrowing some ideas from dynamical system theory (see in particular Boffetta et al., 2001). For campaign studies, the FSLE analysis permits to identify biogeochemical regions of potential interest. This strategy has already been tested, either in post-cruise or real-time analysis, during many campaigns such as LOHAFEX (Smetacek et al., 2012), Latex10 2010 (Petrenko 2010), KEOPS2 (d'Ovidio et al., 2015), STRASSE 2012, OUTPACE 2015 (Rousselet et al., 2018 ; de Verneil et al., 2019), OSCAHR 2015 (Marrec et al., 2018 ; Rousselet et al., 2019), PEACETIME 2017, SARGASSES 2017, FUMSECK 2019 (Barrillon 2019 ; Comby et al., 2021), TONGA 2019 (Benavides et al., 2021) and SWINGS 2021 to identify structures of interest. A review on the FSLE and other satellite-based Lagrangian techniques can be found in Lehahn et al., 2018).

In the revised manuscript we have integrated some ideas detailed above, see lines 102-112

(page 4).

==> Do maps of altimetry/SST also show the existence of the front between the two water masses?

In our work altimetry is the input data for the FLSE (see above). At 38° N 20', the altimetry-derived surface current directions change drastically along the NS transect, suggesting a front. However, this is not clearly the case for the WE transect. We don't think altimetric maps are essential for the paper because the front is clearly visible with the VM-ADCP current and the FSLE (see figures above, or eventually in the future supplementary material). Maps of SST can provide another view of fine-scale dynamics. However, the front is not clearly visible on the map of SST. Indeed, gradients of temperature are not enough contrasted in spring, in the Mediterranean Sea. It is easier to locate the front with the map of [Chla] (cf Fig. 1) than the SST, that is why we think these maps aren't necessary for the paper, and we have not added these figures in the revised manuscript.



Horizontal velocity measured by VMADCP at 25 m, along the WE (a) and the NS (b) transects, superimposed on altimetry-derived surface velocity provided by AVISO. Temperature measured by TSG along the WE (c) and the NS (d) transects, and superimposed on SST. The dates of AVISO and SST correspond to the dates of each transect, i.e., 9 and 11 May.

==> Line 142. "later" should be "latter"?

Done, see line 152 (page 5) in the revised manuscript.

==> Is Figure 2 absolutely necessary to include in your results? It relates mainly to your methodology and I suppose isn't overly important for the story you are trying to tell. I would

consider moving this to supplementary material.

Following your suggestion, we have moved this figure in Appendices. See figure A1 (page 37) in the revised manuscript.

==> I think it would be helpful to modify your figure 1 to show broader study region/familiar landmarks, so readers not familiar with the Mediterranean Sea can get more of an idea of the region you are working in.

Thank you for the suggestion, see new figure 1 (page 24) in the revised manuscript.

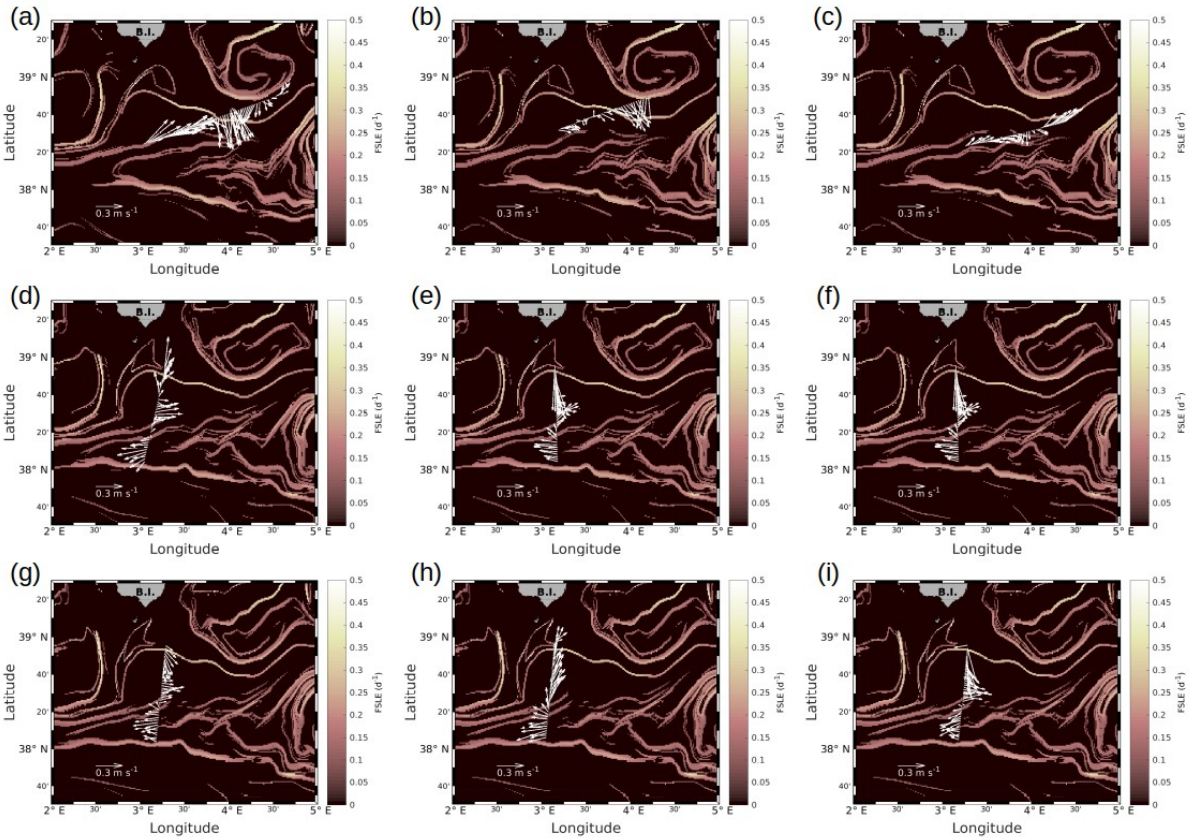
Results

==> It would help to try and highlight the specific zonal feature being discussed in Figure 3. I can see several features based on the FSLE map, corresponding to the latitude 38° N 20'.

On the NS transect, two FSLE features cut the transect at around 38°N 20' exactly where the horizontal current directions change drastically. The orientation of the WE transect makes it harder to distinguish a clear separation of the current direction, due to its alignment with the fine scale structure. However, a FSLE feature cuts this transect just above 38°N 20' and, at this point, the current begins to change and turns to the North-East. We have clarified this in the revised manuscript, see lines 209-213 (page 7).

==> Out of curiosity, do the other transects (not presented) show the same results?

We obtained similar results for the other transects as shown on the figure below. We have added this figure in Appendices (see figure A2, page 38) in the revised manuscript.



Horizontal velocities measured by VMADCP, along transects of the WE hippodrome (a), (b), (c) and the NS hippodrome (d), (e), (f), (g), (h), (i).

(a) 8 May 12:50 - 9 May 00:30

(b) 9 May 09:00 - 9 May 15:30

(c) 9 May 16:50 - 9 May 23:45

(d) 11 May 02:00 - 11 May 08:40

(e) 11 May 10:00 - 11 May 16:45

(f) 11 May 17:55 - 12 May 00:50

(g) 12 May 01:50 - 12 May 08:20

(h) 12 May 09:30 - 12 May 16:40

(i) 12 May 17:30 - 13 May 00:20

The lines in bold correspond to the WE and the NS transects presented in the paper. In our study, we have chosen to select the transect (c) for the WE hippodrome, because we deplore a lack of temperature and salinity data for the other transects of the WE hippodrome, due to technical problems with the Seasoar.

==> Line 218. I would help the reader and refer to your figures here (Figs. 5b and d). I suppose the triangles indicate the position of this?

Triangles in Fig. 5b and Fig. 5d indicate the geographical positions of the best separation between the two types of AW, as also described in Table 1. We have rephrased this part, see lines 230-232 (page 8) in the revised manuscript.

==> Line 225 onwards. This is your results section and thus I would avoid trying to discuss your observations using citations. Perhaps this information can be moved to the discussion.

We agree and we have moved this information in the Discussion section of the manuscript revised manuscript (see lines 356-361, page 12).

==> Lines 237-239. Again, this seems more like discussion material. Furthermore, although it is nice that you have shown similar results in temperature and salinity using an independent glider dataset, is the addition of a figure necessary here? You can probably briefly mention that the glider dataset showed similar results. I only mention this as the manuscript text is relatively short, and yet you have 16 figures. I would think about condensing your analysis slightly and think about where figures may be more appropriate as supplementary material.

We have chosen to keep this sentence in the revised version of the manuscript (see lines 251-252, page 9). However, following your suggestion, we have moved figure 6 in Appendices (see figure A3, page 39 in the revised manuscript).

==> Following my previous comment, Figures 7 and 8 are not described in much detail. For example, Lines 241 – 244 are fairly broad, considering you are talking about three separate transects, for each hippodrome in Figures 7 and 8. I would try and be more clear and descriptive with your results here. Indeed, the data show a clear, interesting separation between the two different water masses (although note that this is less apparent in your density plots...).

We have rephrased this part following your suggestions (see lines 254-260, page 9). We have also combined figures of the Seasoar sections together (see figure 5 in the revised manuscript), as well as figures of the glider section. Furthermore, we have moved figures of the glider section in Appendices (see figure A4, page 40).

==> Line 247. For your DO and Chl-a plots, please re-clarify what hippodrome you are referring to (the NS one).

The vertical sections of [Chla] and O_2 have been obtained with the SeaExplorer glider. The glider has performed an outward and a return route, parallel to the NS hippodrome (cf Fig. 1).

==> Lines 247 – 249. What does “richness of structures” mean? Please be more descriptive with your results, or otherwise, remove superfluous material.

==> Line 251. Chla is higher where exactly? Please expand and provide detail to your analysis.

==> Lines 251 – 254. What plots are you referring to here? Also avoid general explanations in your results, especially without providing evidence or context e.g. “probably associate with vertical dynamics of the front”. Provide more details (in the discussion) or remove. Please go through the whole manuscript and avoid such general statements.

We have moved these parts concerning the biogeochemistry observed with the SeaExplorer glider, in Discussion (see part 4.2, in the revised manuscript). Furthermore, we have also reworked and try to clarify these parts.

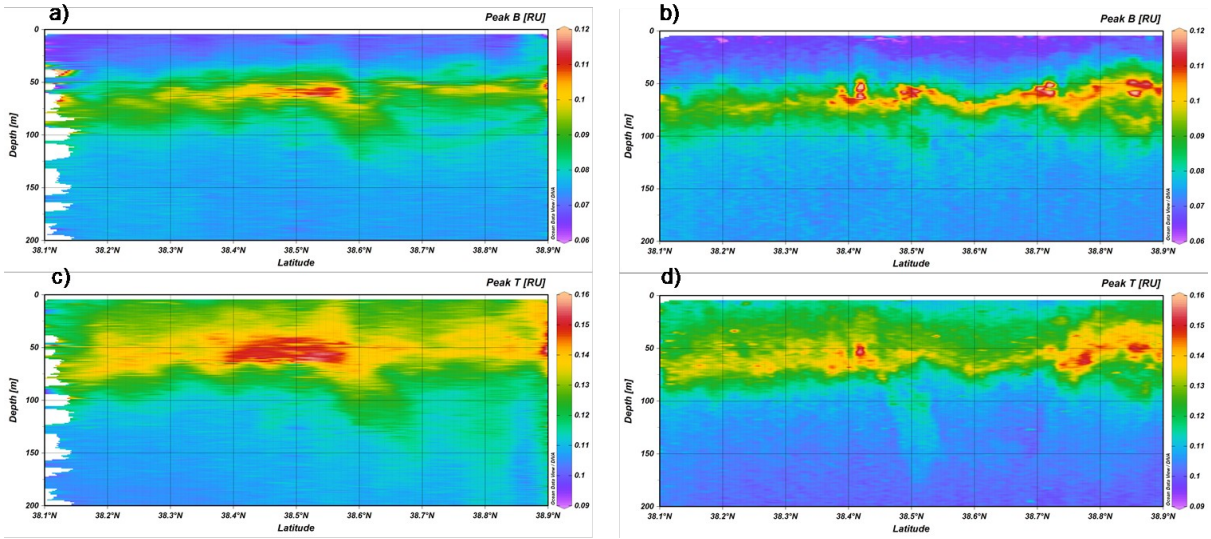
==> Line 255 onwards. I am struggling to understand the connection you are trying to make between Chl-a/DO and your “peak T/peak B”. In the methodology, it is not fully clear to me what the motivation was for measuring these parameters. Please clarify. Furthermore, you do not really discuss these parameters in your discussion. Please consider what information is directly relevant to your analysis and justify the inclusion of each of your figures with corresponding text.

Tryptophan- and tyrosine-like FDOM fluorophores (peaks T and B, respectively) are recognized to have an autochthonous origin in the marine environment, being produced through the activity of autotrophic and heterotrophic plankton organisms, in particular phytoplankton and heterotrophic bacteria (Stemond and Cory, 2014), and are known to be indicators of bioavailable/labile DOM (C and N) (Hudson et al., 2008; Fellman et al., 2009). Even though phytoplankton activity is considered a source of tryptophan- and tyrosine-like fluorophores (Determann et al., 1998; Stedmon and Markager, 2005; Romero-Castillo et al., 2010), bacterial degradation appears to be a source, but also a sink for these fluorophores, depending on the availability in nutrients (Cammack et al., 2004; Nieto-Cid et al., 2006; Biers et al., 2007).

In the present work, higher contents in tryptophan- and tyrosine-like fluorophores were found in the northern part of the transect ("older" AW) relative to the southern part ("young" AW). The same distribution pattern was observed for total Chla and O2 concentrations, as well as microphytoplankton abundance. These results highlight the strong coupling between hydrology, phytoplankton activity and DOM concentration in this area. In addition, it has been recently shown that various groups of microphytoplankton might produce tryptophan- and tyrosine-like fluorophores (Romero-Castillo et al., 2010; Fukuzaki et al., 2014; Retelletti

Brogi et al., 2020), which is in agreement with our observations. The fact that tyrosine-like fluorophore was rather associated with Chla concentration and tryptophan-like with O2 concentration reveal that these two fluorophores were probably not issued from the same phytoplankton groups. Moreover, it seems that tryptophan would be more susceptible to be released by heterotrophic bacteria (in addition to be released by phytoplankton) than would be tyrosine-like material (Hudson et al., 2008; Tedetti et al., 2012; Stemond and Cory, 2014).

In the revised manuscript we have integrated some ideas detailed above, in the part 4.2 of the Discussion.



Vertical profiles (5-200 m depth) of fluorescence intensities of tyrosine-like fluorophore (peak B in RU) (a, b) and tryptophan-like fluorophore (peak T in RU) (c, d) measured by the SeaExplorer glider (Mini-Fluo sensors), along the outward route: 6 May 00:00 - 9 May 21:00 (a, c) and the return route: 10 May 00:00 - 13 May 21:00 (b, d). Slight spatial interpolation was made using Data-Interpolating Variational Analysis (DIVA) method from Ocean Data View (ODV) software version 4.6.5, Schlitzer, R., http://odv.awi.de, 2014.

We have modified part 3.3 of the Results: “Characterization and distribution of phytoplankton by flow cytometry”, following the comments of the other reviewer. In the revised version of the paper, we have described the nine groups of phytoplankton that we have identified thanks to flow cytometry (whereas in the old version we had grouped together the pico and nano phytoplankton groups). That’s why, we have also modified the figures of the transects with the abundances of phytoplankton groups (see figures 7 and 8, pages 30-31 in the revised manuscript).

Discussion and Conclusion

==> Line 361. I would avoid using informal text like “thanks to the flow cytometry measurements”.

Thank you for the suggestion.

==> Lines 349 -359. Why don’t you mention the consequences of these dynamics in terms of upwelling/downwelling here? This is what is driving your phytoplankton variability after all?

==> 379 -380. Please expand on this! How does all of your statistical analysis support your results? And why is there no reference to the figures highlighting this analysis?

We agree with your suggestions and we take into account it in the new discussion section, (see in particular, 4.3 subsection).

==> Lines 337 – 348. This is quite confusing as written.

We have rephrased this part. See lines 350-360 (pages 11-12) in the revised manuscript.

==> Lines 362 onwards. What about the other phytoplankton groups identified by flow cytometry? You simplify here that there is two main groups, and yet quite an in depth analysis is presented for the other groups in figures 12 -15.

In the new Discussion (see part 4.3 in the revised manuscript), we have extended our analysis on all the phytoplankton groups identified by flow cytometry. We have also provided a detailed analysis explaining the effect of the front on the distribution of the phytoplankton groups.

==> What contributes to the variability in phytoplankton community structure along the WE hippodrome? It is quite clear you have two distinct northern and southern water masses, but I wonder if there are other physical mechanisms that may be driving the variability you see longitudinally? What about the horizontal movement of water masses?

Again, we take into account your question in the new discussion section, (see in particular, 4.3 subsection).

==> Overall, the discussion needs to fully encapsulate the results that you present. As it stands currently, it appears at times to only take bits and pieces of your story and I feel much of your previous analysis is ignored.

In order to clarify and improve our Discussion we deeply rewritten this section, that is now divided in three subsections and separated by the Conclusion section...

Authors's reply to Anonymous Referee #2 comments on bg-2021-38 Tzortzis et al.

Dear referee,

Thank you for the attention that you have given to our work. Biogeoscience has given its green light to revise our paper. So we have reworked the manuscript, taking into account your suggestions and those of the other reviewer. In order to follow our reply, your comments have been copied here in blue, after the '==>' symbol.

General comments

==> The impact of fine-scale physical processes on plankton community is indeed very important. We have fully realized the significance of this problem, but limited by the observation means (especially biological parameters), the current understanding is very limited. Based on these backgrounds, I think this work is a very good attempt. The author's cruise design is very targeted, various equipment is very effective, the text description is very clear and detailed. However, from the perspective of research papers, I did not see the logical chain driven by scientific hypotheses. Instead, they used various devices to verify some predictable results. If the physical-biological processes and mechanisms in fine-scale are consistent with those in meso-/large- scales, why are they so important and are unique? When the scale becomes smaller, what is the most important scientific question in the process of physical-biological processes? Because this part is not highlighting enough, I have been looking forward to the new results (differences with large-scale and classical observation) and thinking about so what? What is the implication? As a research paper, I would like to see the author's point around a new result, or a logical inference.

First of all, we kindly disagree with you on the fact that showing consistency between some biophysical stirring processes in meso- and large- scales is a non-result or a result of poor interest. Such a consistency should not be automatically expected. An equally plausible scenario could have been that, in regions that are much less energetic than boundary currents, like the region we study here, the physico-chemical contrasts induced by the horizontal stirring are not sufficiently strong to spatially reflect into different phytoplanktonic communities. That said, we acknowledge that we did not detail our scientific questioning enough and we were not clear enough about the novelty and the implications of our results.

That's why, in the revised version of our manuscript, we have reworked the abstract in order to highlight the novelty of our study (lines 13-18, page 1). In the Introduction, we have also developed in more detail the implication of our work, insisting on the non-predictability of our results (see lines 58-60, page 3). We have also deeply rewritten our Discussion and Conclusion following your comments and those of the other reviewer. Indeed, in the revised manuscript we have separated the Discussion and the Conclusion, and we have also divided the Discussion into several subsections:

In the first subsection of the Discussion (part 4.1 "Physical properties of the front"), we have developed our ideas about the estimation of vertical velocities in our case, and then we have dug deeper into our interpretation about the two AW observed and their role in the structuring of the front.

The second subsection (part 4.2 “Biogeoschemistry”) took back some points that we have mentioned in the Results in the previous version of our manuscript, but in this revised version we have more detailed the implication of these results for our study.

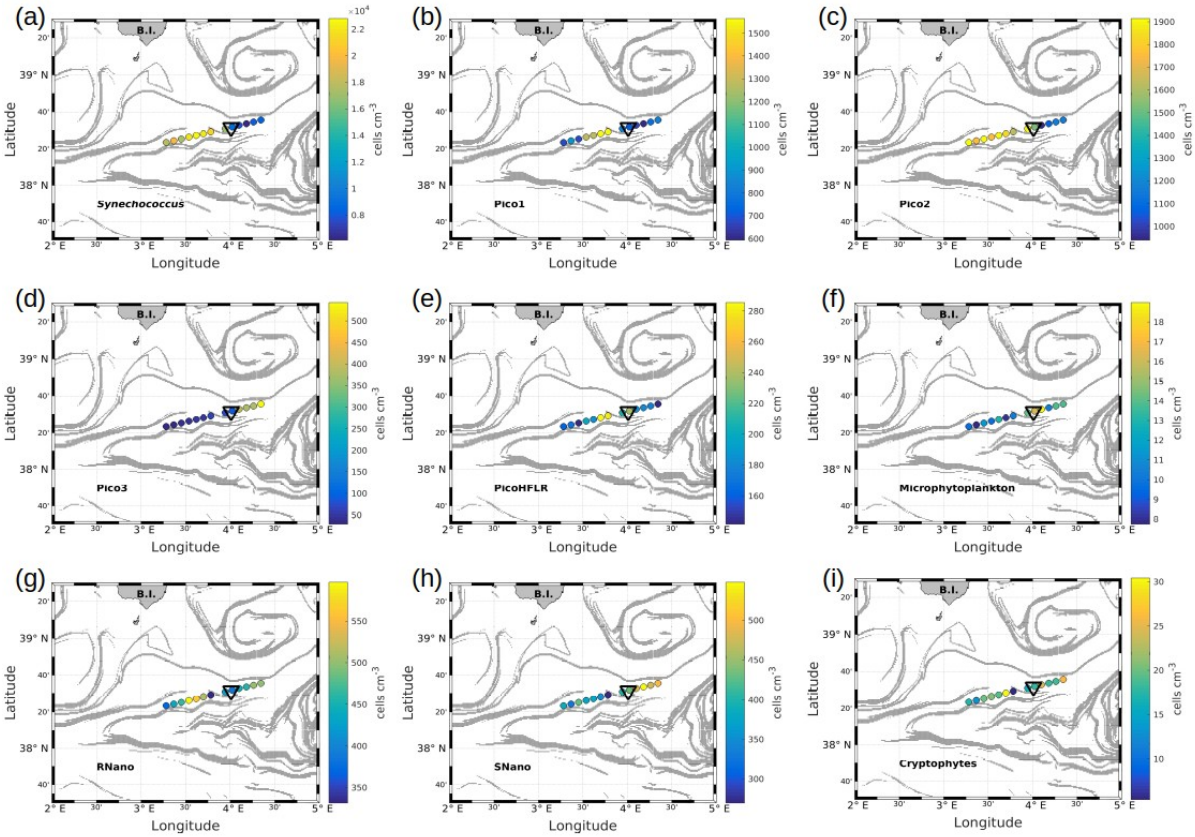
In the third subsection (part 4.3 “Physical-biological coupling in the frontal area”), we have clarified the role played by the physical forcing occuring in the front, on the distribution of phytoplankton abundances.

Finally, in our “Conclusion and Perspectives” (part 5), we have added some sentences (lines 438-446, page 14) in order to clarify and highlight the novelty and the implications of our study.

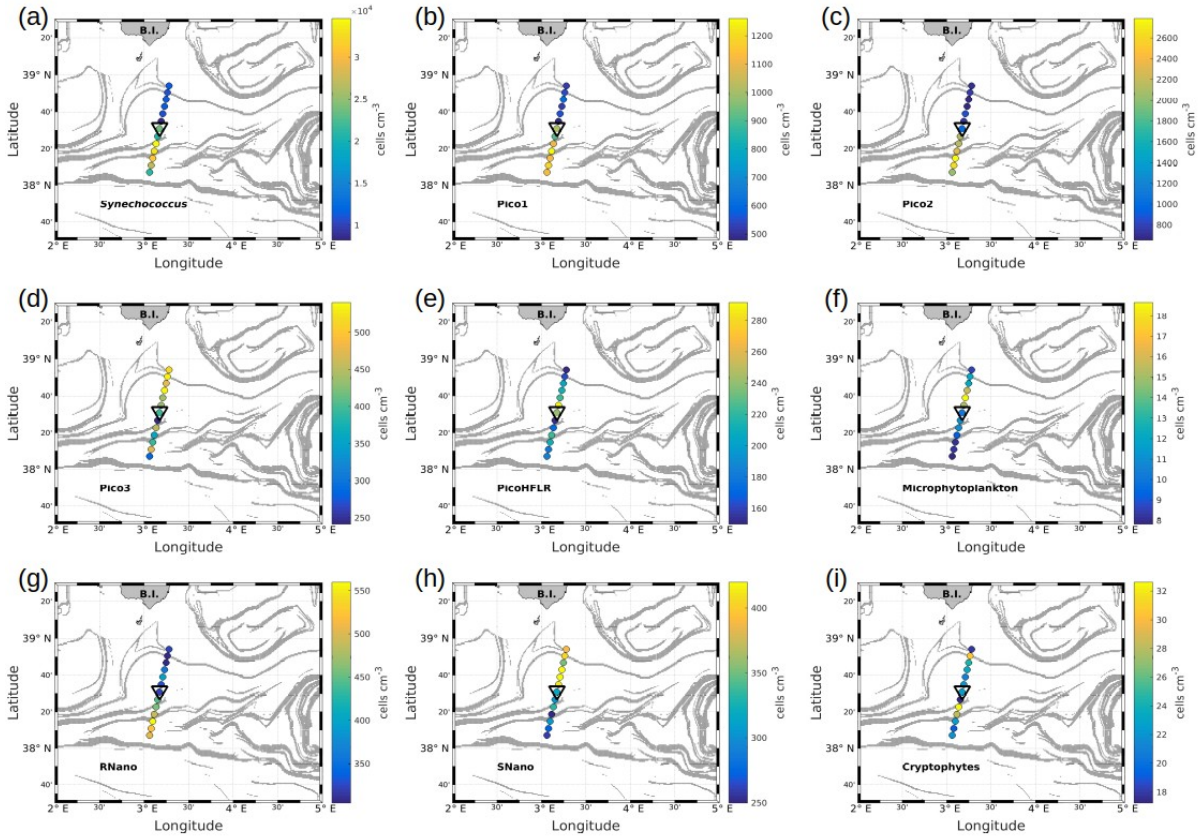
Furthermore, we have combined figures, following your suggestions and those of the other referee (see figure 5, page 28 in the new manuscript). Some figures have also been moved in Appendices (see figures A1, A2, A3, A4 and A5).

==> I noticed that there are two different water masses, one is old AW and the other is young AW. The later data analysis is almost organized according to this logic. Although the traditional physical ocean observation (such as water mass analysis) can also distinguish these two water masses generally, I found that the biological parameters do not seem to be completely consistent (at least some mathematical analysis is needed to clarify from the seemingly chaotic distribution). If the author can dig in depth according to this logic and see if the underlying mechanism is universal (extrapolation), they may be able to find a clue.

The clustering analysis of flow cytometry data allowed to detect several groups of phytoplankton, in particular, to identify various groups of eukaryotic Nanophytoplankton (RNano and SNano) and eukaryotic Picophytoplankton (Pico1, Pico2, Pico3, and PicoHFLR). Nevertheless, in the first version of the manuscript, we showed figures where the abundances of nanophytoplankton represented the sum of the abundances of RNano and SNano groups. Thanks to your remarks, we have redone figures (cf figures here below and figures 7 and 8, pages 30-31 in the revised manuscript) taking into account RNano and SNano abundances separately.



Abundances (in cells per cubic centimeter) of the phytoplankton groups along the WE transect, superimposed with the FSLE field. Triangles indicate the front area.



370 Abundances (in cells per cubic centimeter) of the phytoplankton groups along the NS transect, superimposed with the FSLE field. Triangles indicate the front area.

On these new figures, it is possible to distinguish a clear separation of RNano and SNano abundances by the front. Furthermore, the distribution of RNano abundance is opposite to the SNano abundance. This opposite distribution is very likely the reason for the unclear
375 distribution that you noticed when both groups were merged. For the sake of consistency, the pico-phytoplankton groups (Pico1, Pico2, Pico3 and PicoHFLR) are also separated in the new version of the manuscript, which wasn't the case in the first version. In the new figure above, Pico1, Pico2, and Pico3 abundances appear now clearly separated by the front. The distributions of PicoHFLR, as well as Cryptophytes, remain less clear. These groups do not
380 appear as well correlated with temperature and salinity (used for the characterization of water masses), unlike the other phytoplankton groups. One explanation could be that these cells could be less sensitive to the environmental conditions than the other groups. Another explanation could be their low abundances combined to the fact that these groups are more difficult to define than the others: the limits of the groups are less obvious and maybe some
385 of the events labelled as PicoHFLR or Cryptophytes may in fact be some background noise.
The principal component analysis and the K-medoid algorithm already constitute an advanced mathematical analysis. The PCA results (cf figure 9, page 32 in the revised manuscript) clearly indicate what is qualitatively observed in the new figures and described above: the opposite distribution of the RNano and SNano abundance, the front separation for
390 the picophytoplankton abundances.

In the revised manuscript, we have replaced our figures by the figures shown above (see figures 7 and 8 in the new manuscript). We have also clarified the part 3.3 “characterization and distribution of phytoplankton by flow cytometry” in the revised manuscript, with some ideas developed above.

==> Although the author defines fine scale (line 23), there are other related descriptions, which are easy to be confused. For example, low energetic front (lines 53, 60) and moderate energetic front (Title. Line 5, 328).

We have harmonized the text using only the term “moderately energetic front”. See lines 56 and 66 (page 3) in the revised manuscript.

==> The size of phytoplankton is also confused. In fact, Synechococcus belongs to picophytoplankton, and I guess the “picophytoplankton” in the article means eukaryotic picophytoplankton. In addition, most cryptophytes are considered to be in nano size. Anyway, there's some confusion.

As mentioned above, we have identified several groups of phytoplankton by flow cytometry and used the conventional names used by flow cytometrists. Phytoplankton groups were resolved on the basis of their light scatter (namely forward scatter FWS and sideward scatter SWS) and fluorescence (red FLR and orange FLO fluorescence ranges) properties (Thyssen et al., 2015 ; Marrec et al., 2018). The names were indeed confusing in the original manuscript as some groups were related to the taxonomy (*Synechococcus*, Cryptophytes) while others were more related to size ranges (picoeukaryotes, nanoeukaryotes). We have corrected that in the new version, to help the reader. Indeed, for instance, *Synechococcus* belongs to picophytoplankton, but it is a prokaryote. We have decided to keep it separated in the study because of that and because it was unambiguously put in evidence by flow cytometry thanks to its higher FLO intensity induced by the presence of phycoerythrin pigments. Idem for Cryptophytes which can be pico- or nanoeukaryotes but can also be discriminated from the red-only fluorescing pico- or nanoeukaryotes based on their orange fluorescence.

We have clarified all that in the revised manuscript, see lines 264-269, 275-276 and lines 279-287 (pages 9-10).

Minor comments and suggestions

==> Abstract: It is necessary to present some new results based on fine scale observations.

Following your suggestions, we have modified the abstract (see lines 13-18, page 1).

==> Line 259, n=≈?

We have corrected, line 387 (page 13).

==> Line 323-331, It is repeated in the introduction

We have corrected that.

==> Line 365-370: I suggest more analysis, based on your high-resolution results, to give more evidence or new explanations.

We have reworked the Discussion to take into account your useful suggestions. See part 4.2 "Biogeochemistry" in the revised manuscript.

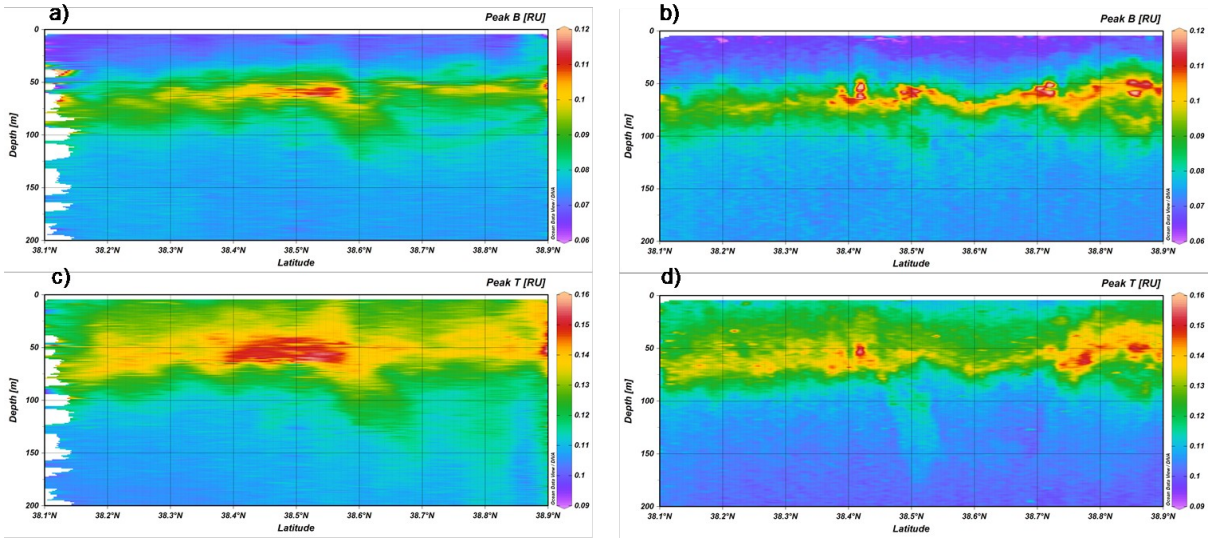
==> Line 373, The concentration and structure of dissolved organic matter may be controlled by physical processes (aggregation and dilution) on the one hand, and by biological effects (production rate and species composition) on the other. Whether these high-resolution matching data can be further mined.

Thank you for your suggestion concerning the interpretation of the structure of dissolved organic matter.

The part concerning the biogeochemistry obtained thanks to the SeaExplorer glider measurements have been moved from Results to Discussion (see part 4.2 in the revised manuscript) following some ideas mentioned here below.

Tryptophan- and tyrosine-like FDOM fluorophores (peaks T and B, respectively) are recognized to have an autochthonous origin in the marine environment, being produced through the activity of autotrophic and heterotrophic plankton organisms, in particular phytoplankton and heterotrophic bacteria (Stemond and Cory, 2014), and are known to be indicators of bioavailable/labile DOM (C and N) (Hudson et al., 2008; Fellman et al., 2009). Even though phytoplankton activity is considered a source of tryptophan- and tyrosine-like fluorophores (Determann et al., 1998; Stedmon and Markager, 2005; Romero-Castillo et al., 2010), bacterial degradation appears to be a source, but also a sink for these fluorophores, depending on the availability in nutrients (Cammack et al., 2004; Nieto-Cid et al., 2006; Biers et al., 2007).

In the present work, higher contents in tryptophan- and tyrosine-like fluorophores were found in the northern part of the transect ("older" AW) relative to the southern part ("young" AW). The same distribution pattern was observed for total Chla and O2 concentrations, as well as microphytoplankton abundance. These results highlight the strong coupling between hydrology, phytoplankton activity and DOM concentration in this area. In addition, it has been recently shown that various groups of microphytoplankton might produce tryptophan- and tyrosine-like fluorophores (Romero-Castillo et al., 2010; Fukuzaki et al., 2014; Retelletti Brogi et al., 2020), which is in agreement with our observations. The fact that tyrosine-like fluorophore was rather associated with Chla concentration and tryptophan-like with O2 concentration reveals that these two fluorophores were probably not issued from the same phytoplankton groups. Moreover, it seems that tryptophan would be more susceptible to be released by heterotrophic bacteria (in addition to be released by phytoplankton) than would be tyrosine-like material (Hudson et al., 2008; Tedetti et al., 2012; Stemond and Cory, 2014).



Vertical profiles (5-200 m depth) of fluorescence intensities of tyrosine-like fluorophore (peak B in RU) (a, b) and tryptophan-like fluorophore (peak T in RU) (c, d) measured by the SeaExplorer glider (Mini-Fluo sensors), along the outward route: 6 May 00:00 - 9 May 21:00 (a, c) and the return route: 10 May 00:00 - 13 May 21:00 (b, d). Slight spatial interpolation was made using Data-Interpolating Variational Analysis (DIVA) method from Ocean Data View (ODV) software version 4.6.5, Schlitzer, R., http://odv.awi.de, 2014.

==> Line 383, “provide an in-situ confirmation of the findings” may not enough. In fact, your data is so good that you don't need to prove other people's opinions at all.

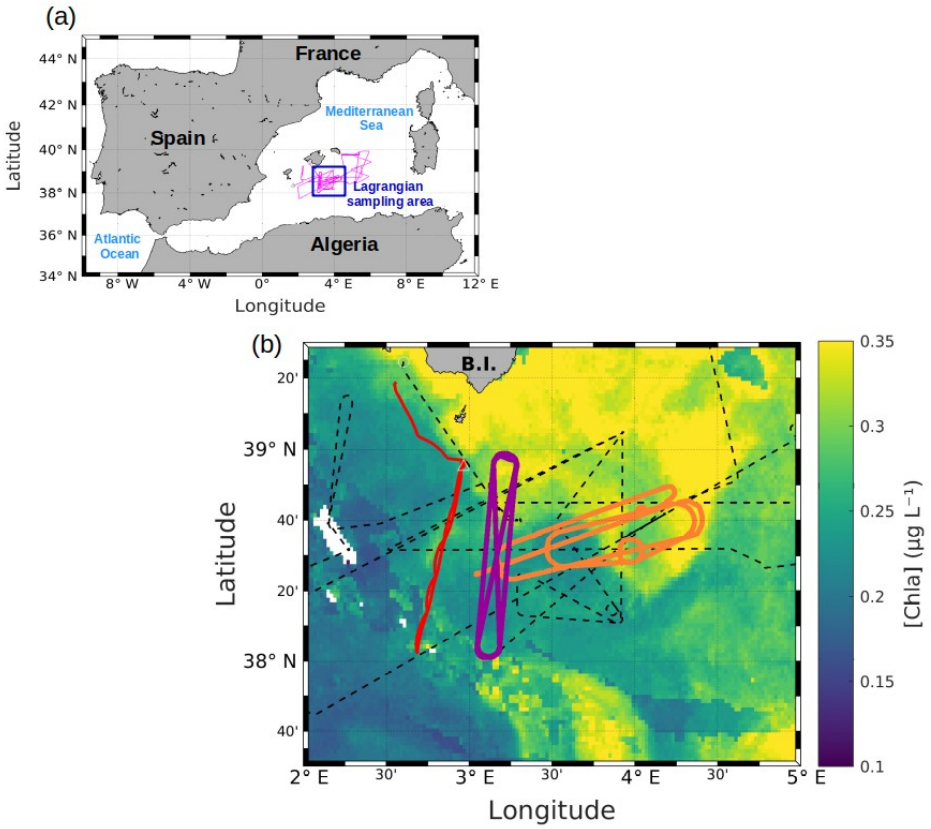
Thank you for your suggestion, we rephrased the sentence in the revised manuscript (lines 438-440, page 14). We maintained the mention of the previous modeling studies, that inspired our in situ experiment.

==> Line 387, change “Kurushio” to “Kuroshio”.

We have modified these parts in the Discussion of the revised manuscript.

==> Figure 1, large scale circulation and map may help readers understand better.

Concerning Figure 1, we have done a new figure shown below with a larger area (figure 1 in the revised manuscript).



(a) Route of the RV Beautemps-Beaupré during PROTEVSMED-SWOT (pink line). The blue box corresponds to the area sampled with Lagrangian strategy. (b) Map of satellite-derived [Chla] provided by CLS for 3 May 2018, selected in the Lagrangian area and superimposed on the route of the ship (black dotted line). The orange and purple lines delimit the two areas called "hippodromes": West-East (orange) and North-South (purple). The red line represents the route of the SeaExplorer glider.

==> Some figures can be combined and are more suitable for comparison, as Figures 2 and 4, Figures 3 and 5, and Figures 6-10.

We have combined figures, following the suggestion of the other referee (see figure 5 in the new manuscript). Some figures have also been moved in Appendices (see figures A1, A2, A3, A4 and A5).

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