# BG-2020-295

## Link to current manuscript

- https://bg.copernicus.org/preprints/bg-2020-295/bg-2020-295.pdf

## Answers to Reviews

We thank the editors and reviewer team for their insightful comments and precious advice. Since several comments coincide between reviewers we decided to address their comments in this common document, which is addressed to all reviewers. The reviewer's comments are thus reproduced here in their integrality (in blue), with specific answers including when relevant references to comments from other reviewers.
We'd like to express here our general gratitude to the reviewers and appreciation for the pertinence of their remarks.

### **Reviewer #1**

*This paper provides a detailed description of the distribution of chlorophyll as function of time, depth and isopycnal in the Black Sea primarily based on Argo Float. The paper is original and seems to add to the current understanding of the phenomenology of the deep chlorophyll max by using a significantly larger and better spaced data set in time.This paper could benefit from proofreading by a native English speaker. I am returning an annotated PDF but being a non-native speaker myself I am sure significant improvement can be found that will make the paper clearer to read for others. While I do recommend this paper for publication (Minor revision) I have some comments that I feel if addressed could significantly improve this manuscript.*

***1.1*** *This paper adopts the view that the DCM is somehow a self-organizing feature determined by the depth of last year's mixing. This seems to me impossible. Phytoplankton (and their pigment) distribution in the upper ocean IS affected by many growth and loss processes in addition to ecological processes such as symbiosis, allelopathy etc'. Because floats are limited in their sensor payloads, researchers are forced to 'explain' their observation with the data at hand. However, they often forget that the observation at hand CANNOT be explained just with such data. For example, without loss processes phytoplankton would keep accumulating year round in the upper ocean. This obviously is not observed, rather their accumulation rates, even during 'blooms' are significantly slower than their division rate. Hence, loss processes are as important as growth promoting processes (e.g. light, nutrients) in determining the observed biomass. This needs to be acknowledged.*

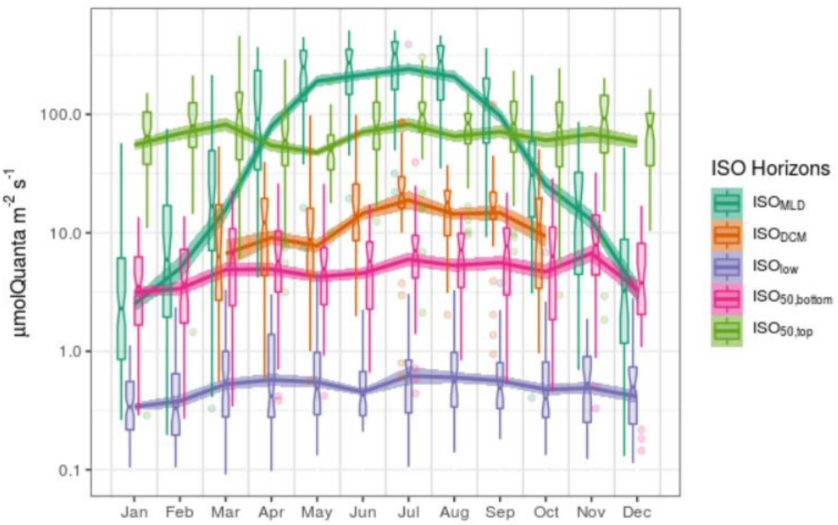
- The reviewer raises several important points here. We here provide our views on these remarks. Upon revision, these views will be integrated in an important reformulation of the discussion section.

- We obviously agree with the fact that the observed phytoplankton biomass results from an equilibrium between growth, loss and transport processes, which usually vary in time and depth in response to biotic and abiotic factors. The DCM, as suggested below, is an operational construct. First, it arises from the combination of a profile in biomass and a profile in the chl:C ratio, and it is thus only considered for the reason that it is easily observable (*see revision of Fig. 9 proposed below).* Second, the planktonic population that composes it is frequently renewed. To ease the discussion below we use "DCM" to refer to this subsurface agglomeration of phytoplankton biomass, acknowledging these two precisions.
- When characterizing DCM as a "self-sustaining" structure, we do not mean that this agglomeration of phytoplankton biomass has the means to avoid loss terms. Instead, we mean that by acting on its surrounding vertical environment, the DCM maintains environmental gradients that constrain its own development. As time scale analysis implies, population renewal within the DCM is frequent : the vertical distribution of phytoplankton therefore evolves according to the balance between growth, losses and transport terms. But the environment to which these terms respond is bended by the presence of the DBM. For instance, accounting for light attenuation by phytoplankton and nutrient recycling upon cellular decay provides mechanistic explanations for such "bending forces" (eg. Klausmeier and Litchman 2001; Beckmann and Hense 2007). The fact that such mechanisms would induce an hysteresis in the pycnal position of the bloom, and that this is the most likely explanation for the high concordance between density DCM position and density reached by winter MLD is the main message of (Navarro and Ruiz 2013).
- In our case, such a concordance is very clear for the first part of the DCM season (March -> May). Then, in a second part of the DCM season (July-> September, June seems to be a transition period during these two states), we observe :
  - A decoupling between particle backscattering (our best proxy for biomass) and chlorophyll profiles, and a maximum in the chl/bbp ratio below the DCM, suggesting efficient photoacclimation (Fennel and Boss 2003).
  - Upward pycnal and depth displacement of the DCM (Fig. 5 & 6).
  - Upward displacement of the DCM in the isolume coordinates (see ***1.2***).
- Our discussion will be rewritten to express that :
  - The appearance of a DCM at the basis of the MLD when this latter is shoaling at the end of winter, is in agreement with the general Sverdrup theory, considering that the Black Sea MLD extends beyond the euphotic depth in winter.
  - The "Navarro hysteresis" description holds for the first part of the DCM season, and induces a stable density position of the DCM.
  - We observe a shift in the DCM structure occurring in June, towards another structuration that holds for July-> September.
  - The fact this shift occurs at the time of the year when surface irradiation is maximal, and opposes the expected responses to high surface irradiance (Beckmann and Hense 2007), suggest an important role of biotic factors (eg. change in phytoplanktonic population (Mikaelyan et al. 2018), and/or

in grazing pressure). However those are hypotheses that we do not have the means to confirm or infirm on the basis of our dataset.

*1.2 The treatment of light in % light levels and only at the DCM level is insufficient at best. Rather, just as was done with density and depth, chlorophyll should be plotted as a function of the isolume value with the same different horizons.*

- This is a very helpful suggestion. Initially we were reluctant to use PAR values, because only a part (~70%) of the profiles included PAR data. However, we now propose to include the following figure, based on this subset, as suggested by the reviewer. Regarding other figures related to this issue, we propose to :
    - maintain the relative 1% isolume (yellow) in Figs. 5 & 6, if only for the fact that it is a commonly used reference.
    - Suppress panel 10b.




- Several observations arise from this figure, which will be accounted for upon revision of the discussion section :
    - The winter MLD extends over the entire photic zone (this can also be observed on Fig. 5, if we consider the 1% of surface PAR as the definition for the euphotic zone).
    - The upper and lower boundaries of the ‘Chla bulk’ are rather constant in this framework.
    - There is a shift in the skewness of the Chla distribution, with the DCM being observed
        - Close to the lower boundary in the first part of the DCM season (March->May), hence at low absolute PAR values.
        - More centered between lower and upper boundaries during the second part of the DCM season (July-> September), hence at higher absolute PAR values.
    - There is a shift in the spread of the DCM in this context, with high spread in the first part of the season, and low spread in the second part.
- We indicated in 1.1, how those aspects will be employed in the revised discussion.

*1.3 The claim that sigma_low is constant is not consistent with Fig. 6. Seems to have a range of variability similar to the 1% PAR, sigma_50,bottom, and sigma_DCM.*

- Agreed. It will be re-discussed in the frame of 1.2 instead.

***1.4.** Periods of free internal waves are NOT days-week. It is gated by the buoyancy and inertial oscillations. There could be internal tides whose dynamic exhibit neap-spring like oscillations which have a time scale of~28 days*

- We will propose, upon manuscript revision, a review for time scales of processes leading to vertical displacement of isopycnals.

## Technical corrections (see annotated PDF):

Link to PDF:
https://bg.copernicus.org/preprints/bg-2020-295/bg-2020-295-RC1-supplement.pdf

- L11: replace 'pycnal level' with 'density level'. If you want your paper to be read and remembered by many avoid using jargon
  - => OK
- L17: remove 'this time' and replace 'nutrients lateral' by 'lateral nutrients loads'
  - => OK
- L34 : Why is this a result of less degradation? Can't it be a result of the large fresh water influx with a lot of DOC? CDOM is also produced from degradation of particulate organic material. Many coastal seas receiving large influx from land (e.g. the Baltic) have a large amount of CDOM.
  - It is true that the Black Sea has a concentration of dissolved organic carbon (DOC) roughly 2.5 times higher than the open ocean according to Ducklow et al. (2007). It is also true that the Black Sea receives DOC inputs from large riverine inputs, especially from the Danube on the northwest shelf with high concentration of DOC (Margolin et al., 2017). It has been shown in Margolin et al. (2016) that more than 50% of DOC in the Black Sea is of terrigenous origin, hence mostly delivered by rivers. The rest of the DOC originates from the Sea of Marmara (marine origin) and from autochthonous production. Therefore the accumulation of large quantities of CDOM in the Black Sea compared to other regions (e.g. the Mediterranean Sea) in general is surely partly due to the degradation of particulate organic material. Although, as explained by Coble et al. (1991), the anoxic characteristic of the basin below ~ 100m could also increase the concentration of dissolved organic matter fluorescence via solubilization of fluorescent material, diffusion of fluorescent compounds out of the sediments, production of fluorescent compounds related to nutrient regeneration, the absence of degradation of fluorescent compounds or via the presence of organisms producing high quantities of fluorescent material. In Para et al., (2010), it was shown that the source of fluorescent CDOM in the Bay of Marseilles was not only due to the inputs of DOC from the Rhône but

also from in situ production, for e.g. from phytoplankton production, zooplankton grazing and bacterial activity. Based on the above, we will reformulate this paragraph as follows: "*Moreover, large quantities of Coloured Dissolved Organic Matter (CDOM) are observed, much larger than in the Mediterranea Sea (Organelli et al., 2014) and in the global ocean (Nelson and Siegel, 2013). This fact results firstly from the allochthonous influx of terrestrial dissolved organic carbon (DOC) (Ducklow et al. (2007), Margolin et al., (2016), Margolin et al. (2017)). Second, anoxia is likely responsible for the accumulation of CDOM through autochthonous production of CDOM through solubilization of fluorescent material, diffusion of fluorescent compounds out of the sediments, production of fluorescent compounds within the detrital loop and the absence of degradation of fluorescent compounds (Coble et al., 2010, Para et al., 2010)*."

  - Para (2010): 10.5194/bg-7-4083-2010
  - Ducklow (2007): 10.1016/j.marchem.2007.01.015
  - Coble (1991): 10.1016/0304-4203(95)00062-3
  - Margolin (2016): 10.1016/j.marchem.2016.05.003
  - Margolin (2017): http://dx.doi.org/10.1002/lno.10791

- L38: sentence with weird tense structure. Get it reviewed by a native English speaker.
  - => OK
- L61: The problem with all these approaches is that they do not take into consideration dynamic loss processes.
  - Refer to our general answer in 1.1
- L90: Note that all these theories ignore any variability in loss terms. Without loss phytoplankton will have accumulated to unobserved concentration. Why do you think it makes sense to assume there is no variability in loss terms (as function of time and/or depth)?
  - Refer to our general answer in 1.1. In particular we now think that variability in biotic factor (eg. grazing) may be a reason for the shift observed.
- L95: remove 'on one hand' => OK
- L98: Replace 'On the other hand' by 'In addition' => OK
- L103: replace 'delivered from' by 'measured with' => OK
- L116: replace 'involves' by 'includes' => OK
- L118: replace 'provided by' by 'measured with' => OK
- L121: replace 'was' by 'were' => OK
- L125: 'Finally, Chla profiles were firstly [...]' : finally and firstly introduces confusion => rephrase it => OK
- L132: clarify 'extracted' => to replace by "whereas no quality filtering of CDOM values was possible due to unavailability of quality flags."
- L133: Does it mean you only used 3 floats with FLBBCD?
  - => No, we used Chla and BBP data from all 5 floats and CDOM when it was available hence for only 3 floats with FLBBCD. To avoid confusion for the reader, we will add "CDOM, when available" in that sentence.

- L150: english correction: “is linearly increasing with depth below 100 m up to 1000m” => suggestion of replacement: “linearly increases with depth below 100 m down to 1000m” => OK
- L159: Not clear if and how you correct for CDOM above 100m.
    - => The so-called ‘FDOM-based method’ described in Xing et al. (2017) does indeed correct Chla for CDOM data above 100 m profiles. To clarify, the FDOM-based method applies a linear regression between FChla and FDOM data below the FChla minimum (usually around 80 to 100 m) where we assume that FChla concentration is zero below, see Equation A3. Once the two regression parameters are determined (i.e. SLOPE_FDOM and ALPHA - see Appendix A), we propagate the correction to the **entire** Chla profile (Eq. A1). Nevertheless, CDOM fluorescence is pretty low in the surface layer (CDOM concentrations are low above 100 m), thus the correction is not significant above this depth (see amplitude of the correction on Fig. 2) Upon revision, we will clarify this issue by making it clear that the correction parameters are determined below the Chla minimum, but that the correction is then applied to the entire profile.
- L169: Don't they indicate that your dark value is probably too low? Given the magnitude of these negative values, you can ignore them.
    - The sentence will be replaced -> “Imperfect linearity between the raw chla and cdom profiles may eventually result in small negative corrected Chla values. Such occurrences were of insignificant amplitude and all located below 80m, and have been set to zero.”
- L181: Are you validating the relationship from Roesler et al? If yes, say so.
    - For clarification the beginning of the sentence will be replaced by “To validate the retrieval of Chla concentration from fluorometers … “
- L188: how do you define a 'bloom'?
    - This sentence will be removed as it is irrelevant.
- L211: an => a (english correction) => OK
- L237: Keep only significant units. I don't think you can determine [chl] to +/-0.001 with the technology you use. => Agreed. Only two decimals will be kept for fluorescence data through the entire manuscript.
- L243: How many HPLC profiles did you test your approach with? Did they come from different seasons? Different locations in the Sea?
    - Only **one** HPLC profile was taken, at the deployment of the new float. It was not possible to take additional collocated HPLC profiles after the float was deployed. Therefore, we have to assume that the absence of Chla at depth, as shown by our unique HPLC profile, is valid at basin scale and at all times. This assumption is supported by the relative spatial uniformity of CDOM profiles. Since a similar question was asked by the other reviewer, we will add upon revision a few lines specifically acknowledging this assumption.
- L266: Fig. 3 does not suggest profiles are homogeneous with season. Rather, in many season one types dominate.
    - By homogeneous with season, we mean a spatial homogeneity of the DCM vs. non-DCM seasonal shift, where profiles are mostly only DCMs or not DCMs. The mix of profiles only occurs at the transition of the DCM season (March and October). We were not talking about homogeneity of profiles within a meteorological season, e.g. in summer, there is indeed an

heterogeneity of profiles (Others, Gaussian-Sigmoid, Gaussian-Exponential, Gaussian) BUT they are all categorized as DCM in the DCM season, and that's what we referring to.
    - This sentence will be rephrased -> " *No meaningful spatial pattern of the DCM period can be evidenced at first glance (Fig. 4) and the beginning and end of the DCM season is consistent across the bassin*".
- L266: In Fig. 4 it seems that points hide other points so I am not sure I actually see all the profiles present. => See proposition of a new Fig. 4 at the end of this document.
- L280: What do you mean by Chla 'area'?
    - Our mistake. We should use just 'the bulk chla', which has been defined earlier in the manuscript.
- L283: why the word 'content' here?
    - It's a mistake. This section will be reformulated, following our answer to comment **1.2**
- L327: Replace 'Besides' by 'In addition' => OK
- L327: Are these the types of floats you analyze here?
    - We will clarify the confusion by replacing "free-floating floats" -> "BGC-argo floats". There should be no distinction.
- L346: UVP will not provide information on phytoplankton in general, only a few species of large chain formers, maybe.
    - That is absolutely true. The UVP6 will provide information on the ZOOplankton and particles such as their area, their grey level and their classification (i.e. detritus, Copepods, Annelids, etc.). The sentence will be removed.
- L347: It is not clear if this is observed in all the profiles from March.
    - See comment with suggested revision of Fig 9 below. The figure has been clarified by lowering the number of vertical bins, and the discussion extended by including a new column with the ration Chla/bbp. Spatial and interannual variability force us to use a normalised referential for this figure. It cannot be said that the situation (bbp peak at DCM) occurs for all profiles in March. What we can affirm is that when all March profiles are considered together, the distribution of bbp values at the DCM depth are significantly higher (eg. in terms of median) than the distributions of bbp values at other depths.
- L373: Internal waves have time scale faster than inertial oscillations and slower than buoyancy oscillations, hence NOT days to weeks.
    - See **1.4**
- L378: You have not shown that light, in absolute unit (e.g, Isolume) does not control the depth structure of the DCM.
    - This aspect will be developed in agreement with our answer to **1.2**
- L389: I don't see it. It seems to vary in the figure in a seasonal way.
    - Instead of "all along the season", we will distinguish the first part (March-May) and the second part (Jun-September) to acknowledge a slight density displacement of the bulk lower boundary.
- L394: You did not show that. To prove it show me depth of isolumes compared to depths of horizons associated with the DCM.
    - This section will be revised and clarified. Essentially, we consider that Navarro's arguments hold to explain an hysteresis in the density location of

the DCM for the first part of the DCM period. However, in the second part, the DCM structure indeed seems reshaped aside from this initial MLD density.

- L399: This seems to negate what you said above…
  - Indeed, there is some confusion in this description. This apparent contradiction will be alleviated upon reformulation of this section.
- L404: winter mixing is limited in depth by a very strong permanent halocline which depends more on sill processes than winter mixing if I am not mistaken.
  - We propose to replace “by the intensity of winter mixing.” -> ”by the vertical extent of the winter MLD”
- L412: I did not see any absolute light related figures in this manuscript.
  - We aim to address this issue by including consideration of absolute light conditions. See our answer to comment **1.2**.
- L413: Phytoplankton do not care about relative light levels, but rather absolute ones. They are also impacted by their own absorbing pigment (the PUR concept of Morel).
  - See **1.2** for absolute light values. A change in the composition of the phytoplanktonic community (and spectral distribution of their absorption coefficient) will be suggested as one potential factor (among many others) to explain the shift between the first, and second part of the DCM season. However, as pointed out in **1.1,** we do not have the means to confirm or infirm any of these hypotheses.
- L420: This needs to be translated to isolume as it is meaningless in the current context and should be computed to to just the DCM location but its vertical spread.
  - Fig 10b will be suppressed. Discussion referring to light conditions will be supported by a graphic showing the different chlorophyll horizons in terms of absolute irradiance, as requested in **1.2**.
- L427: Yes, but your observation miss loss processes. If those were stratified, they could stratify phytoplankton biomass and pigments. We cannot ignore loss processes. W/o them phytoplankton biomass will keep increasing much more rapidly than observed and throughout the year.
  - See discussion in 1.1
- L459: Are these corresponding values for depths from 12 -> 50 or the values at the maxima? I think that to elucidate the light question you need to compute the isolume at the DCM, 50_up and 50_down, just as you did for other parameters (e.g. density).
  - What we meant to say is : “During the DCM season, 50-70 % of the total Chla is found in a 10 m layer located around the DCM depth. Observed DCM depth varies in a range between 12 and 50 m below the surface”. The conclusion will be rewritten upon revision, according to our answer to **1.1, 1.2, 1.3**.
- L461: it is not alleviated, it is compressed.=> ok
- L463: I don't see it in your figure.
  - See fig. 10. Please consider revision of the discussion, mostly with Navarro’s hypothesis holding for the first part of the period only, and then a displacement of the DCM according to other, unknown, probably biotic, factors.
- L465: What is sustaining it? The DCM is an operational construct. The population of algae dominating it likely changes throughout the season.
  - See 1.1
- FIG 6: What dose the thick black line represent?
  - It is a mistake, it will be removed.

## Reviewer #2

*Summary: This manuscript contributes to improve the studies about the relationships between the DCM phenology and the drivers in the Black Sea using BGC-Argo floats.This paper is of scientific relevance, well written and logically organized.* ***Unfortunately, however the results presented are not yet fully convincing in its present form, and some further work is needed to improve the manuscript.***

*General comments:*

- *Language and grammar: generally, the manuscript is well written.*
- *Title: The title reflects most of the authors guidelines in the manuscript.*
- *Abstract: The abstract presents a good summary of the manuscript. The context of the study is clearly defined.* ***A suggestion could be to highlight the obtained results better.***
    - The abstract will be modified upon revision of the manuscript to include further insights arising from Reviewer 1 comments : ie. inclusion of absolute light horizons, and enhanced mechanistic consideration.
- *Introduction: well written and exhaustive.*

***2.1*** *Lines 120-125* ***:*** *The authors should explain why they remove the descending profiles. I think that for statistical studies it is better to have the greatest number of profiles.*

- Usually, the floats are taking measurements while going upwards in the water column. Therefore, for 4 out of 5 floats, there are no descending profiles except sometimes during deployment for testing purposes. Actually, only the float 6900807 measured upwards and downwards during its lifetime. The reason why we have removed those descending profiles is briefly explained in the manuscript (L121-123): The time interval between the two profiles (up and down) is small (~hours). Using both ascending and descending profiles would be useful for short timescale applications, which is not the case here, hence it would be redundant information. We propose to adapt this section (L120-125) as the following : *“First, descending profiles were removed (398 profiles coming mostly from the float 6900807, empty descending profiles were systematic in the float 6903240) for one main reason. The time interval interval between the up and down profiles is small (~ hours) therefore using both types of profiles would bring redundant information as opposed to short timescale applications. We would not observe significant differences in the Chla distribution between these two profiles.*

***2.2*** *Section 2.4. Clarify how many HPLC samples were analyzed.*

- As this information was also not clear to the other reviewer, we will clarify the fact that we could only have one HPLC profile (at the deployment of the new float) with samples taken at 12 different depths.

***2.3*** *Lines 222-230: Authors should show HPLC profiles. It is very difficult to know how many profiles have been analyzed from HPLC data*

- See previous comment.

***2.4** Lines 257-260 : Authors indicate that the non-DCM season is largely dominated by Gaussian-sigmoid forms. Please, include in figure 2 this profile for non-DCM category.*

- *That's indeed a good suggestion. Instead of changing Fig. 2 which addresses the different correction steps for Chla data, we will add every type of profile in the non-DCM season in Fig. 1, in relation with Fig 3.*

***2.5** Authors indicate that they have HPLC data to validation exercise. I suggest the authors analyze the planktonic community composition using pigment marker (HPLC data) due the clear differentiation of planktonic community composition in surface and subsurface layers (Mikaelyan etal., 2018, 2020).*

- Based on the IOCCG report on phytoplankton functional types (https://ioccg.org/wp-content/uploads/2018/09/ioccg_report_15_2014.pdf) and on the HPLC data that we have (12 points, one profile), we have found that diatoms (indicated by Fucoxanthin) are present in the 0-50 m range (concentrations ranged between 0.13 to 0.16 mg/m$^3$). Low concentrations of Peridinin, 19'-Hexanoyloxyfucoxanthin, 19'-Hexanoyloxyfucoxanthin, Alloxanthin and Zeaxanthin - the major marker pigments used for phytoplankton classification with HPLC according to this reference - (all concentrations ~ 0.01 to 0.05 mg/m$^3$) indicate a low to very low abundance of dinoflagellates, prymnesiophytes, pelagophytes, pryptophytes and cyanobacteria in the 0-30 m range. Below 50 m, all concentrations are ~ 0.001 mg/m$^3$ except the Fucoxanthin (0.03 and 0.01 mg/m$^3$ at, respectively, 70 and 90 m. Below that, we can assume that there is no more [alive] phytoplankton (Chla ~0.01 mg/m$^3$). This remaining Chla could also be a result of phaeopigments. Moreover, based on the article of Mikaelyan et al. (2018), it can be said that we are in the beginning of the 'Cold Winter' loop where we observe numerous diatoms in Spring, which follows the usual scheme for temperate regions (diatoms -> dinoflagellates -> coccolithophores -> dinoflagellates -> diatoms).
  The suggestion from the reviewer probably arises from the misunderstanding on the number of HPLC profiles available to us. We will complete the description by mentioning explicitly the observed composition (ie. dominance of diatoms) and refer to the mentioned works as concern planktonic composition.

***2.6** Lines 371-375 : Please, clarify the physical processes that can modify isopycnals and their time scales. This paragraph is confusing (internal waves,mesoscale, submesoscale, . . .).*

- *See 1.4.*

***2.7** Section 4.3. Lines 388 – 394: The authors explain their results based on the hypothesis formulated by Navarro and Ruiz (2013). It would be interesting for the authors to verify that the density where the DCMs are located during the summer months is just the density of the MLD in the previous winter.*

- The suggested verification is explicitly presented in lines 401-408 and figure 10a. Indeed, we are explaining our results in conformity with the hypothesis of Navarro et al. (2013) only because we observed such results. Lines 401 -> 408 explain that we tested this hypothesis by looking at the ratio between the density where the DCMs are located and the density of the MLD of the previous winter (respectively for each float to account for the fact that they are not located in the same region each winter). Those ratios are displayed in Fig. 10a. In summer, it can be observed that the medians of those ratios for the summer months are rather close to one, hence the agreement with Navarro.

***2.8*** *Section 4.4. This section is very weak. Either it will improve or it should be withdrawn. The authors can analyze whether there are interannual differences in density in the MLD during winter months and analyze if the density of the water where the DCM is located changes during the stratification season in different years.*

- Since it is not central to the main discussion, but yet allows to illustrate the interannual variability of the described seasonal succession. We propose to transfer this section in an appendix.

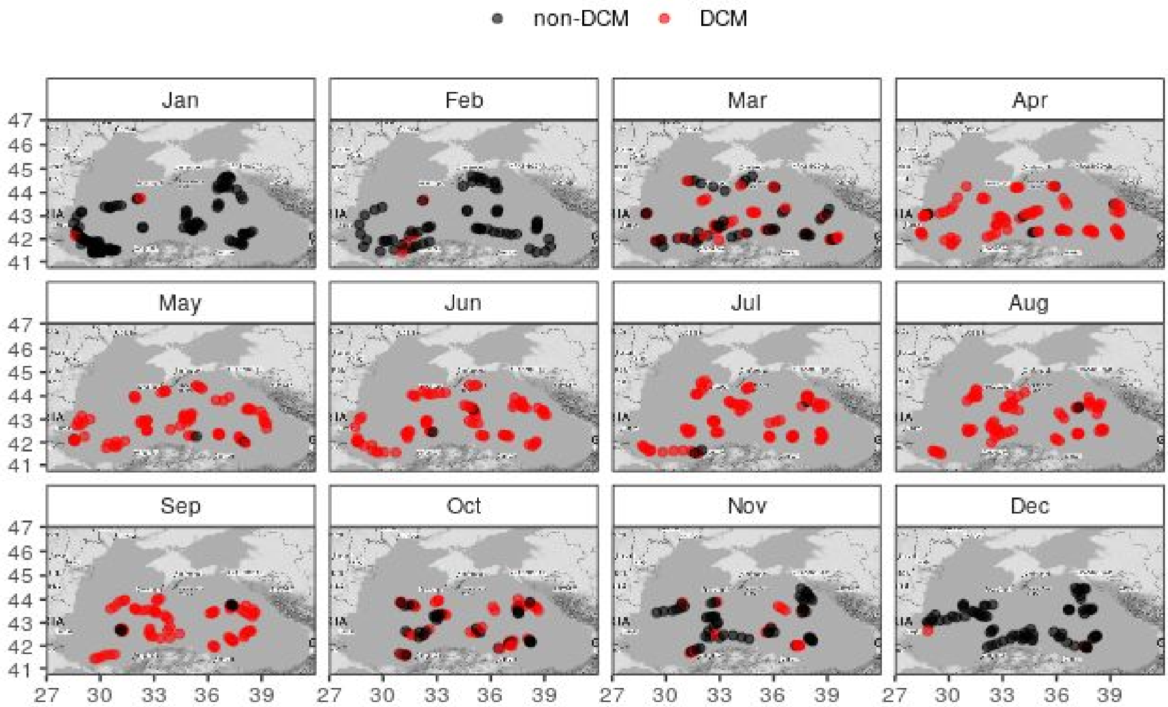
## Technical corrections:

*Figures and tables: Figures and tables are appropriate. Anyway, minor suggestions are given below:*

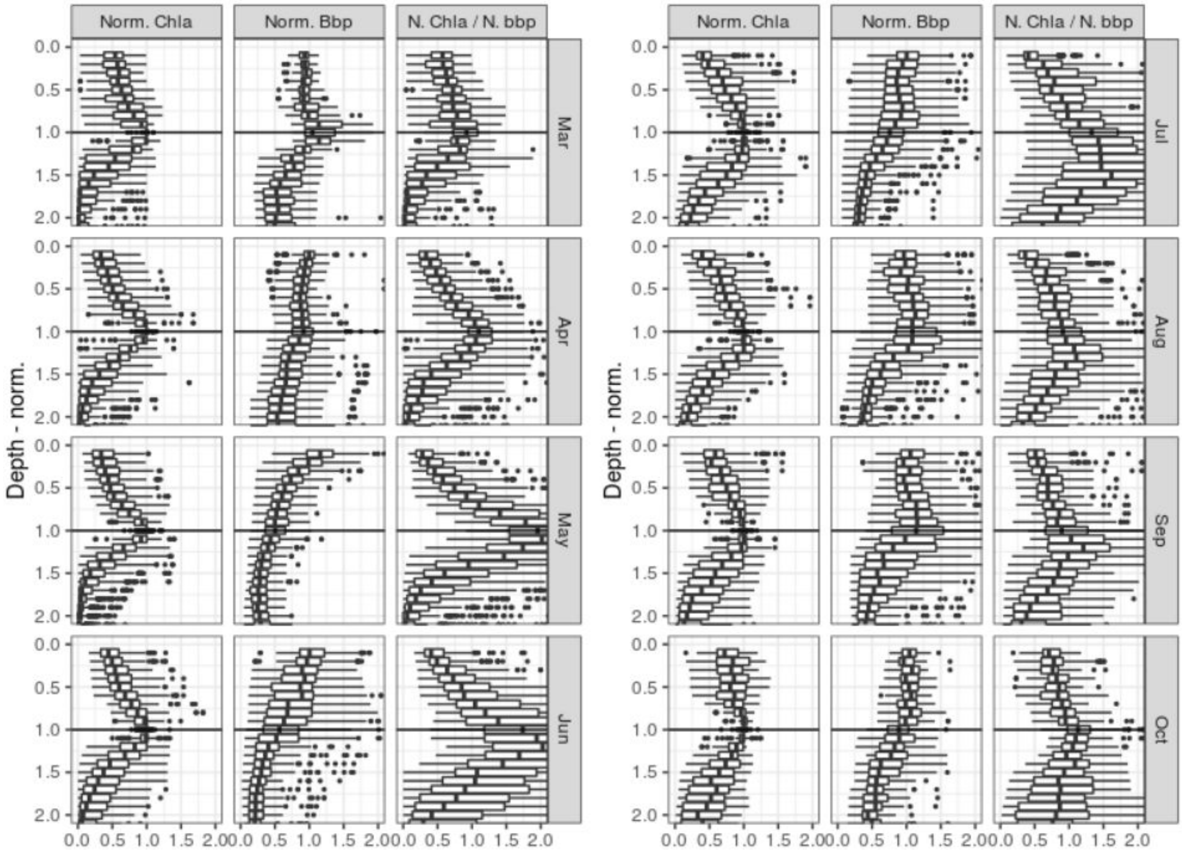
- *Figure 2. Include the second x-axis for CDOM.* ⇒ OK
- *Table 1. Include the number of data used to perform this statistical analysis. =>* The number of data points used to perform this analysis is specified at Line 185 in the Material & Method section. It will be added in the table caption.
- *Figure 6. Include in the figure caption the meaning of the solid black line.* ⇒ It is a mistake, it will be removed.

# **Additional figures revision.**

Proposition for a new Fig. 4.



Proposition for Fig. 9.




We propose to revise Fig. 9 with the above (addition of a chla/bbp ratio column, and reduction of the number of bins for normalized depth).

Spatial and interannual variability constrain us to use a normalised referential for this figure. Besides, only the optical proxy bbp is available, and no proper calibration is available to translate those in terms of organic carbon content.

Nonetheless, the addition of a chla/bbp ratio allows to evidence important aspects:

- Rather constant values for the upper part in March/October, expected for mixed conditions.
- A peak in the ratio located at the DCM for the first part of the DCM period.
- A peak in the ratio that is located below the DCM for the second part of the DCM season, during which there is a small maximum in bbp slightly above the DCM depth.
- This suggests a difference in the efficiency of photoacclimation between the two DCM periods.

Those aspects will be embedded within the revision of the discussion section.