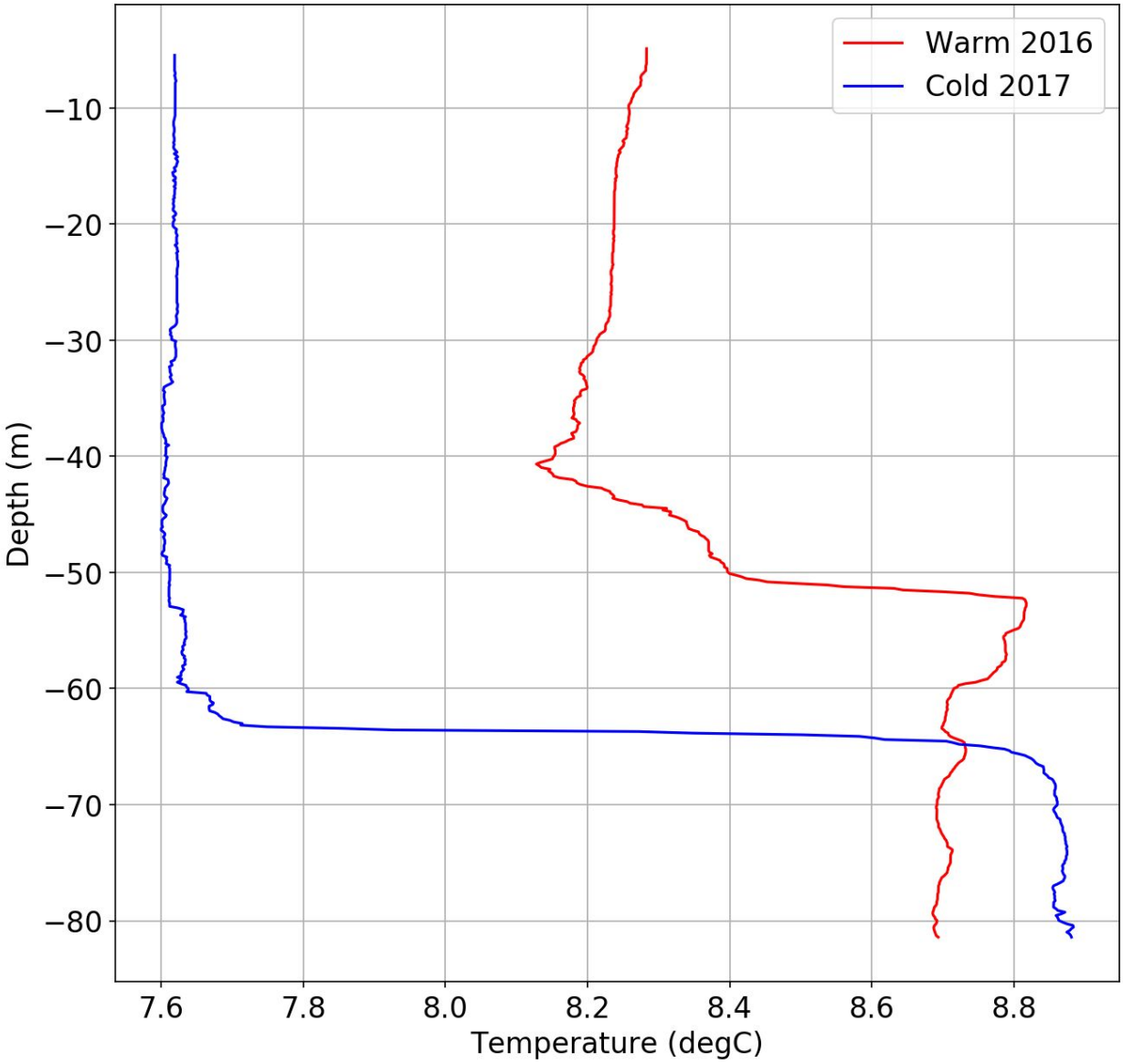
## General Comments

The authors investigate the drivers of differences in the vertical distribution of chlorophyll-a between 2016 and 2017 in the Black Sea using BGC-ARGO data. A key feature of interest in the vertical distribution is the so-called deep chlorophyll maximum (DCM), which the authors show is deeper and less intense in 2016 than in 2017. They account for this difference by arguing that cold atmospheric conditions in the winter of 2017 led to convective mixing and nutrient entrainment, thus increasing winter production. It is then argued that this increased production led to enhanced self shading in 2017, which accounts for why the DCM is shallower compared to 2016.

In general, I agree with other reviewers that the hypothesis presented is interesting and could represent a significant contribution to the question of what factors control the DCM. However, I also agree that currently the authors do not present sufficient evidence to support their hypothesis. Furthermore, the methodology requires some important revisions which I explain below. I therefore recommend that the following revisions be undertaken prior to publication:

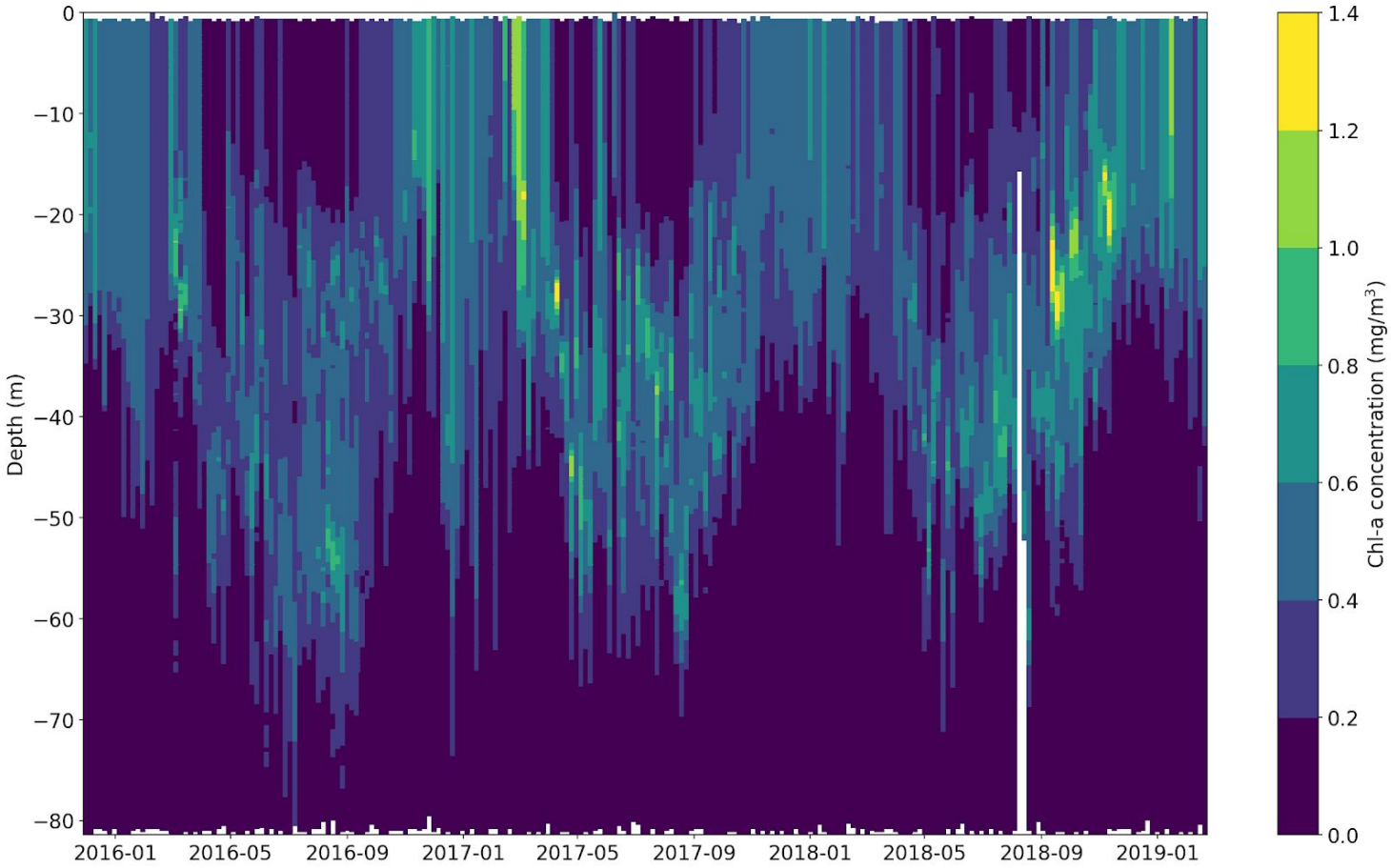
1. All monthly averaging should be removed or only added to supplement the higher frequency data. This is actually why there is little difference seen in the MLD between the 2 years - the differences have been averaged out. Below I show an example of temperature profiles for early February comparing the 2 years. Here it is clear that the MLD is deeper in 2017 by ~20 m, although if you average over the whole month you won't see much difference.




This highlights that the phenomenon being investigated occurs at much higher frequency than monthly, which needs to be taken into account in more detail than is currently done.

2. Similarly to point 1 above, the data should be presented with as little interpolation as possible. It is clear from figures 2, 4, 5 and 6 that some kind of spatiotemporal interpolation has been done to produce such highly “smoothed” plots. Below I show an example of how the chl-a data look for float 6901866 with a minimal amount of interpolation (here I only use a linear interpolation in the “depth”

dimension for the missing data, and gaps of greater than 5 m are not interpolated) I suggest to change the figures to something more like this, which portrays the data more accurately:




Here it is clear that the high chl-a values seen in winter of 2017 are actually composed of 2 short periods (10-15 days) of elevated growth, one in December and another stronger one in March. Figure 2 in the current manuscript makes it seem like one long period of sustained growth. Figure 6 does actually show these 2 pulses, but since 2016 and 2017 are split into separate panels one cannot easily see the 2 distinct growth periods. The plot above also shows that the DCM is most intense (highest chl-a) in the autumn of 2018 - it might be interesting to look into why this is the case.

3. I follow the argument that the upliftment of isopycnals is associated with a rise in the nutricline and therefore nutrient entrainment into the MLD. However, I would argue that simply referring to other literature where this relationship has been established is not sufficient to say that it has occurred in the present case. Since

this entrainment of nutrients is key to the argument being made, it follows that it should be explicitly shown with data. Here I recognise that the nitrate data may be biased in these particular floats as the authors have suggested. However, the important point is that nitrate concentrations should be higher in the cold 2017 year, so biases in the concentration may not preclude the use of this data (since we look for relative differences, not absolute values). So long as the bias is properly taken into account I would argue that the data should be used to support the argument.
If the data are really not appropriate, perhaps other proxies for entrainment of deep water could be used (e.g. dissolved oxygen)?

4. If convective mixing is indeed present in winter of 2017, then one should be able to see strong cooling events preceding the mixing events. For this one could perhaps use a reanalysis product or something similar. The heat flux could even be estimated for these cooling events, although it may be enough to just correlate temperature anomalies with the mixing events. If there are indeed strong cooling events preceding the mixing, then this would certainly strengthen the argument.

5. I recommend that the authors provide a quantitative estimate of the DCM depth, so that its temporal variability be assessed objectively. I can think of various ways this could be achieved, perhaps by obtaining the mean depth of the 90th or 95th percentile of chl-a concentration for each profile. A time series of the DCM depth could then be produced for both floats and the cold/warm years compared quantitatively.

6. The level of English in some parts of the manuscript detracts from the value of the science being presented. I provide some suggestions for specific passages below, however, I would strongly suggest that the authors further edit the manuscript to improve clarity and the communication of the findings.

## Specific Comments

All figures: The captions lack detail and in many cases are unclear. I suggest carefully reviewing them, adding additional details and rewording to avoid confusion. I give some examples below, but I suggest to revise all captions.

## Introduction

1. Line 27 (and subsequent use): I'm not sure what is meant by " nitroclyne." Please define this.

2. Lines 58-59. Is this really true that: "  The amount of Chl and related water clarity largely control the depth of the euphotic zone (Shigesada & Okubo, 1981; Morel, 1991) ." What about solar angle, time of year? Non-organic particles? Time of year is mentioned earlier in the text, but here it seems like Chl is essentially the only factor. I would reword to " The amount of Chl and related water clarity *strongly impact* the depth of the euphotic zone … "

3. Line 47. What is meant by the term " dynamic upwelling"? Please clarify in the text or reword, since this is not standard terminology.

4. Line 62 -63. What is the degree of shoaling of the euphotic zone reported in Letelier et al. (2004)? How is phytoplankton impacted and what is specifically meant by "deep layers" (i.e. how deep)?

5. Lines  80-82. *"Due to the strong haline stratification, the position of chemical layers in the Black Sea is tightly coupled to certain isopycnals and the variations of their concentration in density coordinates are significantly less than in z-coordinates.* " Do you mean that vertical variations in the concentration of certain chemicals is significantly less in density coordinates than in z-coordinates? If so, please state this more clearly since the wording is potentially ambiguous. I would also suggest briefly stating why this is important/ significant.

6. Lines 173 - 179: Do you mean here that large-scale circulation is intensified in cold years? If so, a revision of the wording is needed to make this clear. In addition, you would need to describe this phenomenon in more detail (i.e. what is the mechanism?).

## Results

7. Lines 223 - 229: This passage is currently very unclear. What negative anomalies are the authors referring to? Do they mean the negative values shown in Figure 6e and f? In that case, they should not be referred to as anomalies (which suggest a difference with respect to a long term mean) but as differences

(higher or lower chl-a in 2017/2016) or perhaps just “negative values.” I would suggest revising these lines, making clear what features the authors refer to and in which figure panels.
The authors also suddenly start talking about the geographical location of the 2 floats, without any preamble or reference to Figure 1. I suggest to remind the reader of the location and trajectory of the 2 floats before discussing chl features detected by each.

8. Line 244: What is meant by “compensational irradiance”? I suggest to clarify in the text.

Discussion

9. Figure 8: I don’t think it's that useful to have the NO3 depicted in both panels of the figure if the profile is exactly the same.

## Technical Comments

Introduction

1. Line 35: “The biomodelling study by Kubryakova et al. (2018)” → I would not use the word “biomodelling,” this is definitely not a standard term that is recognised by the community. Biogeochemical or ecosystem model would be more appropriate (or just “modelling”).

2. Line 45: “nutrients” should be nutrient.

3. Line 54: change “, documented for the Black Sea in … “ to “ which has been documented in the Black Sea (*references)* “

4. Throughout the manuscript please change “buoys” to floats. The use of buoys may lead to confusion since BGC-ARGO are floats.

Methods and Data

5. Figure 1: I suggest to only show the isobaths that are labelled (2000, 1600, 1000, 200 m), since as the figure is now there are so many that it becomes meaningless.

6. Line 125: What is the depth of the reference density used for the MLD calculation?

## Results

7. Figure 4: Which float is the data taken from? If it is an interpolation of both then the method of interpolation must be provided. Add details to the caption.

8. Figure 5: State in the caption how the difference is computed, is it 2016 - 2017 or the other way around? Following this, it would also be helpful to say what positive and negative values mean, e.g. “positive values indicate the chl values are higher in 2017”

9. Figure 7: It is unclear what is being compared here. Are the red lines 2016 and blue 2017? Or do they represent different floats? Please clarify in the caption, and also add legends to the figures.

10. Line 154: conventional should be *convectional.*

11. Line 213: “Ten-daily diagram… “ Change to “Fig. 6a-d shows the same features at a higher frequency of 10 days...”

12. Line 233: “Jule-September”

## Discussion

13. Lines 291 - 292: “ *Entrained in winter period nutrients and the rise of the irradiance causes the following spring growth of phytoplankton.* “ Reword as: “Winter entrainment of nutrients, followed by increased irradiance in spring, is known to lead to enhanced phytoplankton growth.”