

## ***Interactive comment on “Dynamics of the Deep Chlorophyll Maximum in the Black Sea as depicted by BGC-Argo floats” by Florian Ricour et al.***

### **Anonymous Referee #2**

Received and published: 25 September 2020

**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.

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Introduction: well written and exhaustive.

Material and Methods: 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. Section 2.4. Clarify how many HPLC samples were analyzed.

Results: Lines 222-230. Authors should show HPLC profiles. It is very difficult to know how many profiles have been analyzed from HPLC data. 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. 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 sub-surface layers (Mikaelyan et al., 2018, 2020).

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

Figures and tables: Figures and tables are appropriate. Anyway, minor suggestions are given below: - Figure 2. Include the second x-axis for CDOM. - Table 1. Include the number of data used to perform this statistical analysis. - Figure 6. Include in the figure caption the meaning of the solid black line.

References: prior work are fully cited

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