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

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Reviewer: Emmanuel Boss, UMaine

This paper provide 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 proof reading 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. This paper adopt 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 force 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 process are as important as growth promoting processes (e.g. light, nutrients) in determining the observed biomass. This needs to be acknowledged. 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 function of the isolume value with the same different horizons. 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. 4. Period 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 $\sim$28 days.

I have additional more minor comments on the attached PDF.

Dear authors, I am often wrong. If you feel my comments are 'off the mark' please feel free to contact me and I will be happy to change them if convinced.

Please also note the supplement to this comment:
https://bg.copernicus.org/preprints/bg-2020-295/bg-2020-295-RC1-supplement.pdf