Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2020-210-RC2, 2020

Biogeosciences
Discussions

Interactive comment on "Warmer winter causes deepening and intensification of summer subsurface bloom in the Black Sea: the role of convection and self-shading mechanism" *by* Elena A. Kubryakova and Arseny A. Kubryakov

Anonymous Referee #2

Received and published: 18 July 2020

This study investigates on the role of winter mixing and self-shading as drivers of the DCM position and magnitude in the Black Sea. Results are based on the analysis of a two-year time series of chlorophyll-a and photosynthetically available radiation (PAR) vertical profiles acquired by Biogeochemical-Argo floats. The analysis focuses on differences between a warm winter in 2016 and a cold winter in 2017. Self-shading by phytoplankton bloom has a main role in positioning the DCM. The paper cannot be published in the present form. Main criticisms are reported hereafter:

1) The role of nitrates is widely discussed though data have not been presented. The

role of NO3 is discussed using literature and MLD differences between 2016 and 2017. However, from Figure 4, MLDs look very similar. To support conclusions, NO3 profiles must be analysed. These profiles exist and have been acquired by the float 6901866. Other NO3 profiles have also been acquired in the same area by the float 6903240.

2) The difference between winters seems more related to mesoscale circulation than to a severe winter. In Figure 1, red squares clearly show that in winter 2017 the floats were entrapped within eddies, which would help explain also density features in Figure 2d. To support conclusion the impact of mesoscale should be addressed and, if any, excluded.

3) Self-shading is interpreted only as a change in chlorophyll concentration (derived from fluorescence measurements) which is used as a proxy of algal biomass or at least to indicate productive layers. Using only chlorophyll makes hard to establish if changes in the magnitude of DCM are due to actual changes in productive biomass because the same modifications could be due to photoacclimation. Optical measurements such as the optical backscattering coefficient could help decipher what changes in chlorophyll are due to actual modifications in algal biomass. Backscattering measurements are available for all the floats in the Black Sea.

4) Argo data analysis lacks of details, applied protocols and procedures and this makes difficult to evaluate its appropriateness. For example, for chlorophyll, it is unclear if NPQ at the surface and CDOM influence at depth have been corrected. How chl has been calibrated? Radiometry has been quality controlled before Kd computation?

Other comments below:

Line 15: 0.2-0.6 mg m3 is a large range of variability

Lines 17-18: to rephrase as it looks contracditory

Line 42: remove "in" before references

Line 59: why only these floats? More floats are available in the Black Sea

Line 60: the floats sampled a longer period. It would be interesting to see what's happened during winter in other years.

Line 69: which equation in Xing 2011 has been used? Have you calibrated Chl with Xing procedure? In this case, I suppose you have calibrated Chl using Ed(490). Ed(490) and derived Kd are quite correlated with PAR (see Morel et al., 2007 Remote Sensing of Environment) thus making analysis of the relationships between Chl and PAR as presented in this study not fully independent.

Line 69: how have you taken into account spikes and other source of errors in Chl profiles?

Line 70: please give the reference for 5 umol photons.

Line 73: have PAR profiles been quality controlled? Have you taken into account also clouds when computing Kd? Could it be possible to see Kd profiles? I imagine that they should be noisy as a 1 m window is very narrow.

Line 77: Claustre et al 2010 is not the right reference for the public database. Argo data are available at http://doi.org/10.17882/42182 or can be downloaded from the DAC (such as Coriolis).

Line 77: Temperature and salinity have been quality-controlled?

Figures 2, 4, 6: profiles have been monthly averaged? Details are missing to understand how single profiles have been managed before drawing the figures.

Line 81: replace "large" with "high"

Figures 2 and 3: add MLD

Line 105: to prove with data

Line 109: why average instead of integration over depth?

Figures 4c and d: add MLD and euphotic depth

Line 124: remove "yin yang sign"

Line 133: add reference for isolume 3 umol photons, why not using the 1% of irradiance just below the surface?

Line 134: shallower instead of higher?

Figures 5 and 6: I suggest to show PAR and Kd time series on the same figure

Caption figure 6: explain the choice of 25 umol photons

Figure 6: show MLD, DCM, euohotic depth

Figure 7 does not sum up your case study as in the figure MLDs are different. In this study the MLDs in winter look very similar.

Paragraph at Line 208: the influence of CDOM is less detectable from PAR as PAR integrates irradiance between 400 and 700 nm while the highest CDOM influence is in the UV range. As irradiance profiles at 380 nm are measured by BGC floats, Kd(380) should be computed to corroborate or not your statements.

Line 213: please give some explanations

Line 214: Are lipids colored?

Line 237: with no nutrient data this statement cannot be proved by this study

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2020-210, 2020.