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Interactive comment on "Climate change induced a new intermittent regime of convective ventilation that threatens the Black Sea oxygenation status" *by* Arthur Capet et al.

Anonymous Referee #2

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The title sounds interesting and attractive. However, for me, the result sounds as follows: There were two cold winters in the period of 2012-2019, and in each of them was not only the amount of cold waters larger, but the concentrations of oxygen were also higher. There is nothing new, except that this has been recorded by BioArgo. Neither, the scientific content of the manuscript supports this title.

We read in the Abstract: "Oxygen records from the last decade indicate a clear relationship between cold water formation events and oxygenation status at different isopycnal levels, suggesting a leading role of convective ventilation in the oxygen budget of the upper intermediate layers." This just repeats what has been known for many years. This

finding is just a confirmation of previous knowledge (see several papers of Konovalov et al). Possible shifts of temperature-oxygen relationships in different periods have also been broadly addressed in these studies. Authors say nothing about that. This brings me to the major criticism. Authors have to make clear what the new knowledge is, which can be gained from their study compared to older ones.

One big problem is that there is a lack of balance in the manuscript. Much attention has been given to the long-term variability. On this subject, I cannot find anything new. The intimate link between analyses of 65-year and 7-year time series is not clear.

The good part of the research is the analysis of data after 2012. However, its relation to the previous periods is completely unclear, and neither is it well articulated in the manuscript. In this part, authors should clearly describe which floats they use, and how these floats capture the temporal and spatial variability. Important to know is whether what is observed is a clear signal or just noise. The statistics of data, and how representative for the Black Sea state they are, need deeper consideration. Fig. 1 shows perhaps that data used in the analyses are not homogeneous. If the data is not homogenous, the subsequent interpretations of long-term changes are not credible.

The statement "... suggests that the CIL renewal, that was taking place systematically each year in precedent regimes, has now become occasional." contradicts what is known from earlier studies. They have to at least refer to Lee et al. (2002. Anthropogenic chlorofluorocarbons ...) who claim that the residence time is $\sim$5yr at 80-120m. I would recommend that they explain what the problem in this earlier estimate is, if any.

The issue of regime shift is not convincing. The question is: can oxygen data over 7 years only identify regime shift? What was the previous oxygen regime? What I see in the oxygen data are just two ventilation events, not a shift. Authors ignore referring to important works about regime shift (e.g., the review article of Oguz in Front. Mar. Sci., 25 April 2017). They have to study the references in this review.

Based on my comments above, I am very sorry that I cannot recommend publishing the manuscript in this form.

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