

Interactive comment on “The temporal variability of oxygen inventory in the NE Black Sea slope water” by Alexander G. Ostrovskii et al.

Alexander G. Ostrovskii et al.

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Dear Reviewer:

Thank you very much for careful reading of our manuscript. We revised it in line with your comments. Below we list your comments together with our responses.

1. English grammar needs substantial improvement by native English speaking oceanographer. Our respond: The paper was rewritten and polished by the Wiley Language Service.
2. Explain more clearly concepts and literature review (see further some specific comments). Our respond: The definitions of the important features of the sea stratification were put into Introduction. The literature cited was expanded.

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3. I do not find a clear prove that “the new CIL emerged by horizontal advection above the pycnocline only at the end of the observational survey”. This is mentioned in the abstract and in p. 5, 6 where authors mention Fig. 9. They should explain how they deduce this from the figure. Our respond: At the end of the survey, the temperature profile showed two minimums. The lower one was located at the same depth range below the pycnocline core where the temperature was above 8.55C. The upper colder minimum emerged on March 3 above the pycnocline core.

4. When talking about time scales authors could make spectral analysis to quantify the relevant periods: “the hypoxia boundary depth fluctuated on two time scales: 17 h due to the inertial oscillations and approximately 5 days due to the current meanders and eddies. Our respond: The spectral analysis is carried out for the data ensemble of the hypoxia onset depth of January 2016 when there was no gap in the data. The spectrum is plotted and inserted in Results as a new figure.

5. Stanev et al., 1995 is cited but not added in the reference list. Our respond: The missing reference is added.

6. When you say “In general, factors that provide the resistance of the Black Sea ecosystem to anthropogenic and climatic effects are weaker than those in other marginal seas adjacent to the European continent” cite the origin of this statement or provide evidence. Our respond: The reference to the paper by A. E. Kideys (Fall and rise of the Black Sea ecosystem. Science, 297, 1482-1484, doi:10.1126/science.1073002, 2002) is added.

7. The statement “In the wintertime, intensive cooling and vertical convective mixing are known to occur in the Black Sea, which enables the top layer to achieve its maximum thickness and minimum temperature (refer to Piotukh et al., 2011 and corresponding references).” is too general. It is not Piotukh et al. (2011), who first came to this conclusion. Authors should cite relevant papers. Perhaps stress that Piotukh et al. (2011) came to this conclusion or areas subject to the present research. When

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describing the formation of CIL take in discussion Stanev, E. V., M. J. Bowman, E. L. Peneva, and J. V. Staneva (2003b) Control of Black Sea intermediate water mass formation by dynamics and topography: comparisons of numerical simulations, survey and satellite data. *J. Mar. Res.*, 61, 59-99. Our respond: Proper references are added

8. Rephrase “However, the ARGO buoys profiled the water column at five-day intervals. Therefore, their data could not be used to evaluate typical time scales of short-time fluctuations in the oxygen inventory.” Some Argo floats in the Black Sea are programmed to measure with finer sampling rate. Re-programming can be done during the operations. Our respond: The text was modified to take this into account.

9. The phrase “In the center of the western cyclonic gyre, the depth of the top mixed layer was limited by 40 m also in an anomalously cold winter (Gregg, Yakushev, 2005).” has to be specified in the context of present paper. Is what happens in the western gyre relevant to the eastern part, the latter being the subject of this study. If yes, prove it. Our respond: These lines were deleted.

10. p. 8, l. 5, In the paper of Gregg and Yakushev there is no a word “hypoxia”. They talk about SOL. To my knowledge suboxia is more used when describing shelf processes. Please check with hydrochemisists. Our respond: These lines were deleted.

11. Authors could consider studying the relevance of following papers Stanev, E. V., S. Grayek, H. Claustre, C. Schmechtig, and A. Poteau (2017) Water intrusions and particle signatures in the Black Sea: a Biogeochemical-Argo float investigation. *Ocean Dynamics*, 67, 1119–1136, doi: 10.1007/s10236-017-1077-9 Stanev, E. V., Poulain, P.-M., Grayek, S., Johnson, K. S., Claustre, H., & Murray, J. W. (2018). Understanding the dynamics of the oxic-anoxic interface in the Black Sea. *Geophysical Research Letters*, 45. <https://doi.org/10.1002/2017GL076206> to their study Our respond: The reference was added to the ms.

Sincerely yours, Alexander Ostrovskii

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Please also note the supplement to this comment:

<https://www.ocean-sci-discuss.net/os-2018-91/os-2018-91-AC3-supplement.pdf>

Interactive comment on Ocean Sci. Discuss., <https://doi.org/10.5194/os-2018-91>, 2018.

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