

## ***Interactive comment on “Annual cycle of sound-scattering mesoplankton in the oxycline and hypoxic zone in the northeastern Black Sea” by Alexander G. Ostrovskii et al.***

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Received and published: 10 March 2021

Comments to MS os-2020-106. "Annual cycle of sound-scattering mesoplankton in the oxycline and hypoxic zone in the northeastern Black Sea" by Ostrovskii et al. This manuscript presents an interesting dataset taken in the hypoxic zone of the Black Sea. The authors have undoubtedly done a good amount of data collection, analyses, and present their results and conclusions in a reasonable way. However, in its current form, the manuscript still requires further revision according to comments from the another reviewer (RC1) with which I fully agree.

We accomplished a major revision of the manuscript. The most important modifications

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are as follows:

- 1) From the reviewers' comments we learned that many questions can be clarified if the zooplankton sampling data and their analysis are introduced into the manuscript. Hence we invited Dr. Elena Arashkevich who is an expert in the Black Sea mesozooplankton to join as a coauthor. In particular, the net sampling data were added into the section Results to validate the sound-scattering layers observed. Other parts of the ms were revised accordingly.
- 2) Although the concept of mean and median profiles of the sound-scattering data is rather simple we switched to more straightforward approach. We binned all of the acoustic data profiles into the daytime and nighttime groups and computed the daytime and nighttime averaged R-profiles for each observational month to infer the seasonal variability.
- 3) We additionally analyzed most recent observational data obtained after May 2020.
- 4) We rewrote both Abstract and Introduction, extended Methods, rearranged and extended Results with 4 new figures, and rewrote both Discussion and Conclusions, References were revised and updated. Certain figures were changed or redrawn. Some material was brought from main body of the manuscript into the Appendix to streamline the research story and to focus on the main points of the ms. Also, mistakes were corrected.

Below, we give our point-by-point answers.

Some aspects that I consider authors should improve, clarify and justify in a better way are: Comment #1: The title is on the annual cycle, but nowhere the annual cycle was actually shown. So, what do authors understand by annual cycle? Authors have taken data from 2013 to 2020 which sounds great, but they have not estimated the annual cycle. I advise authors to perform these calculations in order to affirm the title of their manuscript or simply delete and rewrite it again.

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Response: The title of the manuscript was rewritten as follows: Seasonal variation of the sound-scattering zooplankton vertical distribution in the oxygen-deficient waters of the NE Black Sea

Comment #2: The abstract must be completely rewritten since it is not consistent with the classic and logical way of establishing the purpose and motivations of the study, then some background indicating the main results and conclusions.

Response: Thank you for pointing this out. The Abstract is rewritten as follows: At the northeastern Black Sea research site, observations of 2010-2020 allowed for study of dynamics and evolution of the mesozooplankton vertical distribution in the oxygen-deficient conditions via analysis of sound-scattering layers associated with dominant zooplankton aggregations. The data were obtained with profiler mooring and zooplankton net sampling. The profiler was equipped with the acoustic Doppler current meter, the conductivity-temperature-depth probe, and fast sensors for the dissolved oxygen [O<sub>2</sub>]. The acoustic instrument conducted ultrasound (2 MHz) backscatter measurements at 3 angles while being carried by the profiler through the oxic zone. For the lower part of the oxycline and the hypoxic zone, the normalized data of 3 acoustic beams (directional acoustic backscatter ratios, R) indicated the sound-scattering mesozooplankton aggregations, which were described by zooplankton taxonomic and quantitative characteristics based on stratified net sampling at the mooring site. The time series of ~14,000 R-profiles as a function of [O<sub>2</sub>] at depths where [O<sub>2</sub>] < 200 μM were analyzed to determine month-to-month variations of the sound-scattering layers. From spring to early autumn, there were two sound-scattering maxima corresponding to (1) daytime aggregations mainly formed by diel-vertical-migrating copepods *Calanus euxinus* and *Pseudocalanus elongatus* and chaetognaths *Parasagitta setosa* usually at [O<sub>2</sub>] = 20-90 μM and (2) persistent monospecific layer of diapausing CV *C. euxinus* in the suboxic zone at 3 μM < [O<sub>2</sub>] < 10 μM. From late autumn to early winter, no persistent deep sound-scattering layer was observed while the maximum of the daytime mesozooplankton aggregation shifted to the oxygen bounds of 10-30 μM. At the end

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of winter, the acoustic backscatter was basically uniform in the lower part of the oxycline and the hypoxic zone. The assessment of the seasonal variability of the sound-scattering mesozooplankton layers is important for understanding of biogeochemical processes in the oxygen deficient waters.

Comment #3: A weak point in the work is the lack of simultaneous sampling of zooplankton. If this was done, authors should show their time series with these samples to validate the acoustic records. If these samples are already in previous papers then authors should emphasize this point throughout the text. This important aspect is not very clear.

Response: The zooplankton data of several net sampling surveys carried out in different seasons and at different time of the day are used to verify the acoustic records as well as to specify the mesozooplankton species and their biomass in the aggregations in the sea oxygen-deficient zone.

Comment #4: The Introduction section should be rewritten. The backgrounds, motivation, objectives and hypothesis that were tested are missing. For example, paragraph from line 41 should be earlier in the text.

Response: The Introduction is rewritten as you suggested.

Comment #5: Line 70. Correct to "species".

Response : Corrected

Comment #6: Line 92. Please indicate authors or doi (if applicable) rather than insert manual. The same for lines 98 and 99.

Response: The authors are not mentioned in that manual.

Comment #7: Line 120. The authors say ": : transducer is most sensitive to particles with a diameter of 0.23 mm: : ." then, it is necessary to make a simple comparison between this particle diameter with the respective diameter of the copepod species.

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This would help to clarify ideas and further support its acoustic validation.

Response: In the revised ms, the copepod species sizes are defined at the end of the subsection 3.1.

Comment #8: Line 133. Please avoid unconventional symbols or nomenclature. Simply indicate 1 m instead of “10<sup>0</sup>”.

Response: Modified throughout the manuscript. Comment #9: Line 143. Do you have observational evidence that the same zooplankton aggregations were sampled? If so, authors should show it as results or if it is an assumption to mention it as such throughout the MS.

Response: Available observational evidence that the same zooplankton aggregations are sampled is included in the revised manuscript.

Comment #10: Line 155. Please indicate accuracy and detection limit for oxygen sensors. This point is important since in several figures there are very low O<sub>2</sub> values, so these values are questionable if the detection limit of the instrument is not known. If this is the case, the values below the detection limit must be deleted.

Response: Done. The measurements of the dissolved oxygen using the SBE 43F and Aanderaa 4330F sensors at the moored profiler were carefully studied in the paper by Ostrovskii and Zatsepin: Intense ventilation of the Black Sea pycnocline due to vertical turbulent exchange in the Rim Current area, Deep-Sea Research I, 116, 1–13, doi:10.1016/j.dsr.2016.07.011 (2016) as cited in the revised manuscript. According to the SBE 43F specification the accuracy should be no worse than  $\pm 2\%$  of saturation as compared with 5% for Aanderaa 4330F that delivers the resolution of  $< 1$  mM or 0.4 %. In practice in the Black Sea, the SBE 43F showed very robust results in detecting the lower boundary of the sea oxic zone consistent with the observations of the sigma-density structure and definition of the oxic zone boundary for the northeastern region of the Sea (Yakushev et al., 2005 among others). In terms of the sensor inertia the SBE

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43F outperforms Aanderaa 4330F which is suitable for the purposes of this our study taking into account the profiler vertical speed of nearly 0.2 m/s. It should be noted that every July since 2014, the chemical oceanographers of SIO RAS took samples for determining the oxygen content at the station located to west of our profiler mooring (Dubinin et al., personal communication). We have compared our measurements with the sample data and found that in the suboxic zone two data sets differ by 2-4 mM only with the sampling data showing just slightly higher oxygen while the vertical gradients are essentially the same. We hope to describe the intercomparison data and to present the seasonal change of the oxygen vertical distribution in a separate paper in the future.

Comment #11: Line 179. Please explain in more detail or reference about the hydrogen sulfide zone.

Response: The references are added.

Comment #12: Line 189. The “R ratio” should be explained in more detail. Perhaps one way to start is by inserting an equation and defining the parameters one by one. Furthermore, authors should emphasize the rationale for using this ratio when other methods are available in acoustic measurements.

Response: The equation is inserted and more explanation is added in section Methods.

Comment #13: Line 195. Correct “daythe”

Response: Corrected

Comment #14: Fig. 4. Why do the authors define the hypoxia zone based on density values and not on O<sub>2</sub> concentration values? Not bad, but for comparative and conventional purposes it is more useful to define this zone with O<sub>2</sub> values.

Response: In the revised section Introduction, the hypoxia zone is defined

Comment #15: Line 206. Delete “compare”

Response: Deleted

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Comment #16: Lines 219-227. This paragraph should be in the discussion section.

Response: This paragraph is moved into Discussion.

Comment #17: Figure 7 refers to October 2004... other figures to November 2019. Why this difference? Authors must give justified reasons to present their results in this way.

Response: We rearrange the order of presenting the results.

Comment #18: Figures 8, 9, 11-14. The O<sub>2</sub> concentration values are in power format. Please avoid this form and use conventional format. The transformed scale is justified by emphasizing on curves, but numbers not.

Response: The axis labels of these figures are modified.

Comment #19: Figure 9. Is it about the entire time series or some selected months? Please clarify.

Response: The figure caption is modified.

Comment #20: Figure 10. It refers to June-July 2014, why? All these discrepancies in presentation of results into figures without adequate justification and methodological clarification make difficult to understand the main message of the MS.

Response: Thank you for pointing at these discrepancies. We make necessary modifications to address this issue.

Comment #21: Figure 11. Refers to August 2019.

Response: The figure is replaced.

Comment #22: Line 288. Without detailed information on the detection limit of O<sub>2</sub> sensors, it is difficult to accept the values indicated here, i.e. 4-9  $\mu$ M.

Response: The information about the sensor accuracy is added. Please, also see response to the comment10 above.

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Comment #23: Lines 318-319. Fluorescence was not measured in this study, if it was, it should be indicated accordingly. If there are support from other sources, they should be indicated.

Response: The work is devoted to the distribution of zooplankton in the lower part of the oxygen zone, so we do not consider the data on chlorophyll.

Comment #24: Figure 14. This figure is fine as a corollary to the main message. I like it, although in 3D view it is somewhat difficult to visualize. Perhaps authors could rotate angles a bit more for better visualization or separate information into two 2D panels.

Response: The figure is modified; however we wonder if it should be included in the revised ms. Probably we would better use it in another paper where we hope to discuss the importance of other environmental factors including the temperature and Chl-a for the dynamics of the mesozooplankton,

Comment #25: Lines 443-444. I do not understand what authors are trying to say in this sentence. Please rewrite.

Response: The sentence is rewritten.

Comment #26: Line 449. Researchgate is a popular and excellent platform to disseminate scientific research; however, to deposit datasets I suggest to use platforms specifically designed for this purpose. Please consider it.

Response: So far we are limited in the options to post the data. However recently, we became involved into the European HORIZON-2020 BRIDGE-BS project and will contribute relevant data into the project data archive in the future.

Comment #27: Finally, consider to reduce the length of the conclusion paragraph.

Response: The section Conclusions is shortened.

Please also note the supplement to this comment:

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<https://os.copernicus.org/preprints/os-2020-106/os-2020-106-AC2-supplement.pdf>

Interactive comment on Ocean Sci. Discuss., <https://doi.org/10.5194/os-2020-106>, 2020.