

Biogeosciences Discuss., author comment AC2
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Reply on RC2

Stella-Theresa Stoicescu et al.

Author comment on "Causes of the extensive hypoxia in the Gulf of Riga in 2018" by
Stella-Theresa Stoicescu et al., Biogeosciences Discuss.,
<https://doi.org/10.5194/bg-2021-160-AC2>, 2021

Please find the the answers to Reviewer2 comments below. For clarity we added our
responses directly under each comment.

General comments

- The manuscript presents a case of hypoxia developed in the Gulf of Riga in 2018 and analyses its causes determined mainly by reduced ventilation of the deep layers due to hydrophysics. As such, the mechanism of hypoxia emergence as a result of imbalance between oxygen biological demand and its supply in some water and sediments domains is known for decades. Neither the seasonal occurrence of hypoxia is exceptional in the Gulf of Riga. Therefore, such regional case study of a particular year might be interesting for the wide global audience only if it would present something not only geographically but also methodologically new and generalized. Besides, the manuscript in its current state appears as a technical report to some monitoring agency rather than precise and focused scientific paper. In that I concur with many comments and suggestions already made by Reviewer #1.

Response: Thank you very much for the comments. We will revise the manuscript taking into account this general comment as well as referred comments by Reviewer1. The parts not so relevant to the main questions of the study will be shortened, keeping only those sections/paragraphs that support discussion on the main results. As also replied to Revier1, we have two main questions: 1) What was the reason for the observed extensive near-bottom hypoxia in the Gulf of Riga in 2018? and 2) Was it an exceptional event, or is it a feature that could occur in the Gulf of Riga and similar basins regularly and/or even more often in the future? We hope the results focussed on these questions are of interest to the wider audience.

- Selecting for analysis only 2017 and 2018 without clear explanation of the choice, you pretty much reduce the interest in the manuscript for global audience and even make questionable its suitability for Biogeosciences vs. some other journals, explicitly dealing with either the Baltic Sea problems or regional issues. In that respect, the manuscript

can be saved by a comparative analysis involving a set of "hypoxia years" (e.g. 1996, 2003, 2008, 2012, 2014, 2015, 2018 (some of them you list at p.12, lines 1-5) and a set of "hypoxia-free years" of your choice. If you you'll find the similarities or even regularities between the cases, such causative, mainly geophysical (?) relationships, even if semi-quantitative, could then be used together with available and evolving climate projections of relevant parameters. If there are none found, such "negative" result, being clearly shown and explained, would still increase the scientific knowledge about the Gulf of Riga.

Response: We agree this is an important point and will include relevant text in the Discussion section. However, we cannot compare the long-term data sets since we have profile data available from 2012. Before that, the rare near-bottom oxygen measurements were done, which are analysed and the relevant analysis is included (as trend estimates since 2005). Thus, we are not able to include any statistical analysis regarding stratification, inflow-outflow estimates, etc. Instead, we will improve qualitative discussion on this matter.

- The occurrence of hypoxia (O₂ less than 2 mL/L) in the Gulf of Riga is not new and was often observed already in the 1970s and 1980s (see, for instance, <http://nest.su.se/nest/> and then go to Baltic Sea=>Marine distributed databases), when also deep-layer salinity was higher until its drop in about 1990 and have been fluctuated without a long-term trend since then. Since it remains unclear why your data analysis was limited to 2005-2018 (p. 6, line 30), a short text expanding a time perspective would help to set a scene for the global audience.

Response: The earlier conclusions based on similar monitoring data are included. It is mentioned in the Introduction section, why we analyse these data sets, but possibly hidden a bit. When revising the manuscript, we will keep only the text relevant to the main focus in the Introduction. We hope it will make it clearer.

- Both the text style (the very manner of describing and presenting, e.g. describing in detail what is seen in Figures, especially features that would not be used further) and its volume (could, perhaps, be halved) look inappropriate to me for the scientific paper. In addition to and supporting suggestions by Reviewer #1, pieces of text that could and should be condensed are indicated in Specific comments, even with a suggested example of the editing.

Response: We will improve the manuscript taking into account this comment and suggestions (as well the similar suggestion by Reviewer1) regarding the presentation style and focus of the manuscript.

Specific comments

p.1, line 10 – is it really exceptional, in what sense and by which characteristics – minimum of absolute or % saturation oxygen concentration, extent of hypoxic zone?

Response: The extent of hypoxia was the largest in 2018 compared with the other years since 2012. However, we agree that it is better to call it "extensive". It also fits better to

our suggestion that it is nothing exceptional but a development that will occur in the future if the load is not reduced and the meteorological conditions support longer stratified periods.

p.1, line 11 – “Forcing data...” appears as a kind of slang, should be explained, something like “meteorological” or “weather”, especially in Abstract. Forcing of what, how, etc... For instance, could temperature and salinity per se be considered as forcing for oxygen because of the oxygen saturation?

Response: We will replace “forcing” with “meteorological”, to be precise.

p.2, lines 3-7 – sloppy unnecessary description, remember about long nutrient residence times and the vicious circle; what about point sources with undertreated discharges from WWTPs? Can be easily removed altogether.

Response: We will shorten this section avoiding unnecessary sloppy or straightforward descriptions.

p.2, line 14 – among hypoxia suddenly about aeration, as, for instance, if there were no hypoxia events in the Gulf of Finland; add and mention Lehtoranta et al. doi: 10.1016/j.jmarsys.2017.02.001, refer to Stoicescu et al., 2019.

Response: We will add suggested references.

p.3, lines 4-5 – could be modified: “The Gulf of Riga water exchange with the Baltic Proper takes place via the Irbe Strait in the west and the Suur Strait in the north (Petrov, 1979; Astok et al., 1999) with dominating contribution of the Irbe Strait (Lips et al., 1995; Skudra and Lips, (2017).”

Response: Thank you, it is better.

3, lines 6-19 – Can then be compressed down to two ideas – general surface and deep-water patterns, and its seasonal alterations. Should it be placed in Introduction vs. Discussion?

Response: We will keep this in the Introduction, but in a shortened version.

p.3, lines 21-28 – the amount of text can be halved by deleting trivial things, some of which were already indicated above. Just as an example: “In winter, the whole water column is well mixed. In summer, stratification is mainly maintained by the seasonal thermocline, while the contribution of haline stratification is rather moderate (Stipa et al., 1999; Liblik et al., 2017). (opt. - Summer CTD profiles from 1993–2012 have shown that) In 1993-2012, the strongest stratification of water column occurred in the years with the highest upper layer temperature in summer and river runoff in spring (Skudra and Lips,

2017).”

Response: Thank you. Yes, we agree and follow your suggestion.

p.3, line 30 – Another example of necessary cut-out is a trivial description of the annual cycle of DO. It would be enough to have nice color picture(s) with isopleths further, in the Results or even Discussion

Response: We will shorten the text.

p.4, lines 14-23 – where and how information on loads would be used further; if retained during revision, reference to (Yurkovskis, 2004) about external input vs. internal processes could be extended with references to Savchuk (2002, 2005, 2018), where such things are discussed in detail. Although all this discussion about the nutrient buffer capacity as well as mentioning of DIP vs. Ox fluxes should be transferred to Discussion, if relevant. Reference to the HELCOM Periodic Load Compilations, both published and publicly available as time series at http://nest.su.se/helcom_plc could be appropriate here.

Response: Thank you. We will use load data from PLC (available until 2017; thus, relevant for our analysis) in the analysis. References and text will appear mostly in the Discussion, as suggested.

To resume, the entire Introduction must be thoroughly re-written and condensed, replacing the textbook-like general geographical and imprecise descriptions and numbers, which would not be used further either in Results or in Discussion, by indication of why you made this study, i.e. what the problem is and how you dealt with it. The questions and hypothesis should be clearly formulated to be then positively or negatively answered in Discussion. Evidently, all that have to be made for an analysis of extended set of “hypoxic years”.

Response: As explained above, we will revise the Introduction section, condense and focus on two main questions: 1) What was the reason for the observed extensive near-bottom hypoxia in the Gulf of Riga in 2018? and 2) Was it an exceptional event, or is it a feature that could occur in the Gulf of Riga and similar basins regularly and/or even more often in the future? Unnecessary paragraphs or sentences will be deleted or shortened.

Section 2. Do we really need such detailed description of equipment? Could the data sources be just moved to the Acknowledgments in the end?

Response: We think the details are necessary. However, we will condense the text.

Section 3.1.1 must be drastically condensed and replaced instead with O₂ vs. Salinity graphs and regressions for the time interval expanded backwards.

Response: We would prefer to keep this part as it is. Almost no monitoring data is

available before 2005, except in the mid-1990s. Thus, we cannot extend the trend analysis.

Section 3.1.2 must be condensed by removing boring description of nice Fig. 3, which, however, have to be extended back in time. Numerical values should be collected in Table, but only if you would use them further in Results or Discussion. For instance, a too verbose description of the hypoxia extent in 2018 (p. 12, lines 6-12) could be quite condensed just indicating that the values from the survey justify estimates based on the central stations, which are given in the previous paragraph (and compiled in Table).

Response: We agree that the text can be shortened (will do so). However, Fig. 3 cannot be extended since we do not have dissolved oxygen profile data before 2012.

Section 3.2. The time interval should be unified and graph's description must be condensed by editing the boring description of what is seen on graphs, reformulating and stressing those features and peculiarities important for further Discussion.

Response: The time interval is the same for all graphs (1979-2018) except the river runoff. For the latter, we have data since 1993. The idea is to show the year 2018 in comparison with the long-term averages and variability. We would prefer to keep the graphs as they are. However, we will shorten the text by focusing on those aspects necessary/important for explaining and discussing the results.

Section 3.3. have to be entirely and much more laconically re-written (avoiding detailed description of pictures and stressing only the features used in further analysis and discussion) according to the analysis of expanded set of hypoxic and non-hypoxic years, suggested above.

Response: We agree and will shorten the text part accordingly.

Section 3.4 could be retaining as an example obtained with a specific equipment, but must be very much condensed by some generalizing instead of describing in detail what has happened from day to day. Besides, explain and justify, why the uncertainty estimated for the synoptic scale could give an estimate for two-week period and would be higher at a seasonal one-two months scale (p. 21, lines 12-15).

Response: We will shorten the text focusing only on the near-bottom layer and the sudden deepening of the upper mixed layer relevant to discussing the results. Regarding the uncertainty, we suggest that if a single measured oxygen value is taken as a characteristic value for a period, then the uncertainty is higher for a longer period.

Section 3.5 – It should present estimates obtained from a “hypoxic years” vs. “non-hypoxic years” sets. Numbers for calculations should be moved in Appendix, if necessary at all.

Response: We cannot provide estimates for the years without dissolved oxygen profiles

(before 2012). Also, the estimates could be biased when the inflow through the Irbe Strait is not evident in the deeper layer at station 114. We will restructure the table keeping only necessary rows, and provide some estimates for the other years (where possible).

Evidently, the entire Discussion must be re-written according to results from "hypoxic years" analysis. It could also be enriched by considering also results and conclusions for the geographical locations other than the Gulf of Riga and the Baltic Sea.

Response: We will revise the Discussion section taking into account similar comments from you and Reviewer1. We focus the discussion on the mentioned two main questions and try to enlarge the geographical relevance by referring to scientific publications from elsewhere.

Technical suggestions and corrections (because of expected re-writing, below I suggest a few corrections to the pieces that could likely be retained)

p.2, line 10 – Conley et al., 2009 should be added here as well

Response: We agree.

p.2, line 12 – be consistent, use the Baltic Proper everywhere, or, if necessary, use names of narrower localities – the Gotland Deep, the Bornholm Deep, the Eastern Gotland basin, etc.

Response: We will re-check it, although, in this particular case, we think the name is correct and understandable also for readers from other regions.

p.3, line 1 – "...being in annual balance " would be easier reading

Response: We agree.

p.3, line 2 – "...period OF about..."

Response: We agree.

p.3, line 5 – refer also to Astok, V., Otsmann, M., Suursaar, U., 1999. Water exchange as the main physical process in semi-enclosed marine systems: the Gulf of Riga case. *Hydrobiologia* 393, 11 –18.

Response: Thank you. Will do so.

p.3, line 6 – "...Suur Strait IS OF 5 m2..."

Response: OK, this part is rephrased.

p.4, line 14 – If it AT LEVELS then it should be "... about 90.5 and 2.5 thousand tons a year, respectively"

Response: OK, this part is rephrased.

... to be continued after the Major Revision