

Comment on bg-2021-160

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Referee comment on "Causes of the extensive hypoxia in the Gulf of Riga in 2018" by
Stella-Theresa Stoicescu et al., Biogeosciences Discuss.,
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Review of the manuscript by Stoicescu, S.-T., J. Laanemets, T. Liblik, M. Skudra, O.
Samlas, I. Lips and U. Lips: "Causes of the extensive hypoxia in the Gulf of Riga in 2018".

General comments

1. 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.

2. 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.

3. 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.

4. 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.

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?

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?

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.

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.

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)).”

p. 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?

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)."

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

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.

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".

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

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

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).

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.

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.

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).

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.

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.

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

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.

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

p.3, line 2 – "...period OF about..."

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.

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

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

... to be continued after the Major Revision