EGUsphere, referee comment RC1
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Comment on egusphere-2022-1090

Anonymous Referee #1

Referee comment on "Temporal and spatial evolution of bottom-water hypoxia in the Estuary and Gulf of St. Lawrence" by Mathilde Jutras et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-1090-RC1, 2022

This manuscript evaluates the temporal changes in hypoxia in the St. Lawrence River Estuary and Gulf from the earliest measurements in the 1930s to present. This manuscript is apparently an update of a previous paper by the main author (Jutras et al. 2020, JGR) with the main conclusion that hypoxia expanded drastically in recent years reaching almost 10,000 km2 in 2021. This conclusion may not be wrong but the authors have not managed to provide compelling evidence to support it!

First, the method used for assessing the extent of hypoxia is very coarse. The authors determine the area deeper than 275 m from the mouth of the St. Lawrence Channel to the farthest station with hypoxia. This approach does not consider that oxygen profiles are irregularly distributed along the gradient, which in combination with the gradual broadening of the channel can give quite variable results. The authors do acknowledge this limitation, but instead they should try to improve the spatial integration of the profiles by developing a more sound data processing approach, e.g. by fitting the oxycline as function of depth along the channel gradient. Moreover, hypoxic conditions can be observed at shallower depths than 275 m (cf. Fig. 3) and it can also be deeper, so why make this simplistic approach of considering the area deeper than 275 m. How do the authors define hypoxic conditions when oxygen concentrations are changing with depth? Importantly, more precise estimates can be obtained by minor improvements in the data processing through formulating an appropriate model. Apparently, most of the profiles are from spring and summer, but there appears to be no filter on which profiles are actually used or that seasonal changes in oxygen concentrations are taken into account. Were all data within a year (mainly spring and summer) pooled disregarding seasonal differences? The seasonal variability adds further to the uncertainty and potentially adds bias to the estimates. Finally, there seems to be cross-sectional variation in oxygen concentrations across the channel (cf. Fig. 2), so how was this taken into account when interpolating spatially along the axis of the channel? Was there always a clear spatial oxygen gradient or could there be cases, where hypoxia was observed beyond (further out) stations without hypoxia, i.e. did a more irregular oxygen pattern ever occur? Overall, there is a general lack of clarity in the description of the data processing.

Second, I do not think the authors provide a compelling case when arguing for changes in the inflowing Atlantic water masses affecting with different temperature and oxygen properties. Since this is a focal point for the manuscript, I strongly suggest (actually, a requirement) that more information and support for this is provided in the manuscript (and not just referencing some of their own previous work). What is causing the increased inflow of NACW (changes in AMO or another climate index)? What are the specific temperature and oxygen properties for the different water masses that normally ventilate the bottom layer? Are these changes visible at the outermost stations in the St. Lawrence channel? Can the changing water masses at the mouth be traced towards the head of the LSLE (it should be possible as the residence time is stated to be 4-7 years)? With such a long residence time for the gulf and estuary, that would lead to some mixing of the different water masses, how come there is such an abrupt change bw 2015 and 2016? The authors need to substantiate this conclusion much better. I am left with more questions than convictions from reading the manuscript.

Detailed comments:
L. 30-32: The assertion that hypoxia and anoxia occur naturally in the mentioned systems is not entirely correct. To my knowledge, Chesapeake Bay did not experience hypoxia before the arrival of Europeans and the following deforestation. The Baltic Sea has had periods with hypoxia/anoxia in the geological past, but the spatial extent was never at the magnitude of the current spread. This sentence should be modified to avoid potential misinterpretations that the current spread of hypoxia is natural, which it is not!
L. 92: 'distinct' should be 'discrete'.
L. 128-130, Figure 4: Were these observations (for linear regression) made at the same depth? If not, then the regression and the results from it do not make sense. It is commonly seen in such monitoring that sampling depths are getting closer to the bottom where oxygen gradients can be strong in more recent years with the development of more advanced CTD's. The authors need to analyse if depths are the same throughout the time series, and if the seasonal sampling time is more or less the same. It is also important to assess whether the samples represent the same salinity to ensure that the waters have the same properties. Why have the authors decided to show only data from the head of the LSLE? If the hypothesis of lower oxygen in the inflowing Atlantic water is correct then the same pattern should be largely paralleled throughout the channel at specific locations. This would increase the support for the hypothesis.
L. 154-158: The argumentation here is essential for understanding the changes in oxygen in the LSLE, but instead of showing any evidence the authors refer to their previous study and one from 2005, neither of those contain the more recent data that motivated the study according to the introduction. It is necessary for the authors to present updated datasets for these patterns of mixing on the shelf. Without such data this argumentation remains unconvincing.
Figure 6: What are the observations showing, i.e. are they observed measurements or means from several cruises or ….?
Figure 7: The authors cannot present important results without providing more explicit information on how these were computed. It is insufficient to reference Jutras et al. 2020b, assuming that the approach in that study is well known.
L. 175: Should be 'NACW'.
L. 187: 'temperature'