Response to reviewer #1 on manuscript egusphere-2022-1090 titled "Temporal and spatial evolution of bottom-water hypoxia in the Estuary and Gulf of St. Lawrence" Comments posted Nov 10, 2022

RC1: 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!

We thank the reviewer for his(her) incisive and helpful comments.

RC1: 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.

We agree with the reviewer that the method used to estimate the areal extent of hypoxia is rather coarse. Below, we explain why using the 275 m isobath is currently the most precise way to estimate the area of the hypoxic zone. The real shape of the hypoxic zone does not follow exactly the 275 m isobath, especially close to the edges of the Laurentian Channel, where turbulent mixing and benthic activity might dome the oxybaths. This possible vertical shift in the oxybaths, where the waters intercept the seafloor, is ont of the uncertainties in our method. However, the spatial sampling is too sparse to assess these effects.

- Mixing is expected to dome the hypoxic zone upward, while benthic oxygen consumption is expected to dome the hypoxic zone downward. Consequently, the two effects partially compensate each other, and we do not expect the overestimation or underestimation to be very significant.
- At the western limit of the hypoxic zone, at the head of the Laurentian Channel, we expect mixing to bring oxygen deeper in the water column, leading to a slight overestimation of the hypoxic area when using the 275 m isobath. However, the head of the channel represents a small fraction of the hypoxic region.
- Another point is that, while the hypoxic layer usually reaches up to 250 m (see Figures 3 and 5), we use the 275 m isobath, which is a safer choice that might lead to an underestimation of the hypoxic zone. We could clarify this point by changing the sentence "The 275 m isobath is chosen because it is representative of the shallowest depth reached by the hypoxic waters (Figure 3)." to "The 275 m isobath is chosen because it is reached each year by the hypoxic waters (Figure 3). This isobath

represents a more conservative estimate than using 250 m, and leads to an underestimation of the hypoxic zone in some years.”.

- Finally, over the years, we have carried out several transects perpendicular to the main axis of the Laurentian Channel. We have looked at these as well as at additional transects from the BioChem dataset, and they consistently show that the layer of hypoxic waters extends, nearly at the same depth or on the same isopycnal, throughout the width of the Laurentian Channel, even where the estuary widens into the gulf, including at the most seaward station where hypoxic waters were detected in 2021. These cross-channel transects do not provide a very high resolution of the shape of the hypoxic zone close to the edges of the channel, as they only included 3 to 5 stations over the width of the channel. Yet, the data reveal that, as mentioned above, the shape of the hypoxic zone is not markedly domed.

We will to add a short discussion of these limitations in the Method section of the paper, that would read as follows:

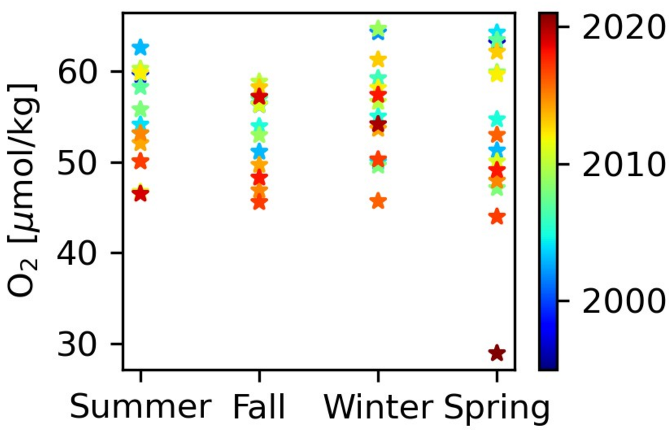
“The real shape of the oxycline is likely domed close to the edges the Laurentian Channel, where it intersects the seafloor, under the action of turbulent mixing and benthic respiration. This doming is, however, limited to the vicinity of the seafloor, as suggested by transects perpendicular to the Channel, and the resulting error is expected to be small.”

Notwithstanding, we thought about how we could improve the estimate.
First, as suggested by the reviewer, we could derive the depth of the oxycline along the channel and use that depth. We verified whether the depth of the top of the hypoxic layer varied systematically with distance along the Laurentian Channel (the hypoxic layer extending, to shallower depths closer to the head of the channel compared to within the gulf). We found no clear trend.
Second, since the sampling resolution is relatively coarse in the estuary and gulf, we could use a modelled, high-resolution climatology to look at the shape of the isopycnals. We are however not aware of the existance of such a biogeochemical model that could reproduce the shape of the oxycline. As mentioned above, based on existing perpendicular (cross-channel) transects,we suspect that this approach will not significantly improve the accuracy of our estimate.
Note that we do not claim to determine the volume of hypoxic waters but the two-dimensional shadow that the hypoxic waters cast along the Laurentian Channel, irrespective of the depth at which the hypoxic waters are found. Figure 5 clearly illustrates that the tongue of hypoxic waters is typically centered between 250 and 300 m but hypoxic waters can be found at shallower depths and does not extend to the bottom as it spreads seaward.

RC1: 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.

The reviewer is right, most of the surveys were conducted in spring and summer, winter surveys were only conducted on three consecutive years in recent years (2018, 2019, 2020), and the data were aggregated per year. Nevertheless, beyond the mean annual variations, no distinct seasonal variations could be discerned. Although one might expect to find slightly lower bottom-water oxygen concentrations in late summer or fall, after the spring bloom autochthonous organic matter settles to the seafloor, the available data show no consistent seasonality (see figure below in which each coloured star represents a given year). This is not unexpected because the deep, hypoxic waters, are isolated from the atmosphere by the Cold Intermediate Layer.

We will explicitly indicate that the data are aggregated per year, and justify this approach by noting that available data show no clear seasonality and that this study focuses on yearly variations. To further justify our assumption, we will substitute Fig. 3 for a version in which the data are aggregated monthly instead of yearly.

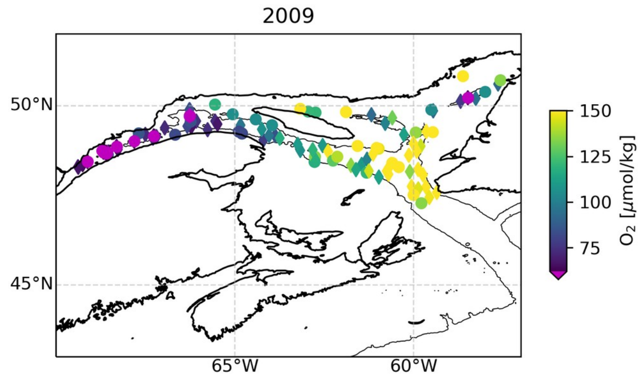


RC1: Finally, there seems to be cross-sectional variation in oxygen concentrations across the channel (cf. Fig. 2), so how was this taken into account when interpolating spatially along the axis of the channel? Was there always a clear spatial oxygen gradient or could there be cases, where hypoxia was 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.

Figure 2 shows lowest dissolved oxygen concentrations at each sampled station, along the Laurentian Channel, between 1995 and 2021. We aggregated the data for individual years, irrespective of the sampling times, to focus on the yearly trends. We show the lowest sampled oxygen concentration, for any station that samples deeper than 200 m. Hence, at some stations, the measurement shown might have been sampled above the hypoxic layer. The presence of some non-hypoxic concentrations (blue dots) between hypoxic measurements (pink dots) is a artefact of the aggregation of data from different cruises, i.e., waters sampled at different times of the year and at different depths, and of the fact that some stations close to the Laurentian Channel’s edges are shallower than the hypoxic zone.

First, we will modify Figure 2 to address this comment. We will use different symbols to identify samples shallower than 250 m, and thus highlight that the non-hypoxic water are found at shallower depths. For example, in the 2009 map shown below, most of the blue stations in the estuary are diamonds, which show samples between 200 and 250m, while circles show samples below 250 m. To better explain the data aggregation and to include this new

information, we will modify the caption of Fig. 2 to write the following:
"Maps of all oxygen samples collected in each year, since 1995, for every station sampled deeper than 200 m. The color indicates the lowest oxygen concentration sampled over the water column. The symbol indicates if the deepest sample is located between 200 and 250 m (diamond) or below 250 m (circle). Hypoxic waters are represented in magenta. The thin black lines delineate the 275 m isobath."



Second, we will rewrite the text to clearly explain that our estimate is for the observed **maximal hypoxic area reached in any given year**. It would not be possible to provide an average area for a given year, as the water column was not sampled equally every season.

RC1 : 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.

Most if not all these questions are answered in detail in Jutras et al. (JGR-Oceans, 2020). We feel that it would be unreasonable to repeat all this information here as it would unduly lengthen the manuscript. The main objective of the present paper is to present the temporal variations in spatial extent of the hypoxic zone. The other objective is to highlight the sudden drop in oxygen since publication of the Jutras et al. (2020), in which the most recent data are from 2018. Moreover, the mechanism that explains the 2018-2021 oxygen decline is the same as the one that Jutras et al, (2020) identified for the 2008-2018 decline. Hence, no new information would come from further discussion on these causes. Alternatively, we will provide a more detailed summary of the method (i.e., e-OMP), the properties of the source/parental waters to the bottom waters of the Laurentian Channel, how their relative contributions have varied over the last few decades and why, as well as the relative contribution of in-situ (pelagic or benthic) respiration as the bottom waters flow inland from the mouth to the head of the Laurentian Channel.

Detailed comments:

RC1: 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!

The sentence will be modified to avoid potential misinterpretations. We propose the following substitution: "Hypoxia and anoxia occur naturally in many coastal environments with restricted circulation, such as fjords and embayments, but hypoxia in more open coastal and estuarine areas appears to be on the rise due to anthropogenic nutrient loading and coastal eutrophication (e.g. Saanich Inlet in British Columbia, Bedford Basin in Nova Scotia, Chesapeake Bay in Maryland, shelf region of the northern Gulf of Mexico, the Kattegat in the Baltic Sea, the Bengali Current in western Africa, and the coastal area of the Changjian River/Estuary in the East China Sea; Bindoff et al., 2019; Breitburg et al., 2018; Gilbert et al., 2010; Rabalais et al., 2010; Li et al., 2002)."

RC1: L. 92: 'distinct' should be 'discrete'.

A much better choice of word. It will be substituted in the revised manuscript.

RC1: 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.

Figure 4 reports the minimum dissolved oxygen concentrations (DO) observed near the head of the LSLE. Until the early part of 2000, and apart from data near Cabot Strait, no other historical data were available for the deep waters of the Laurentian Channel. The early measurements (1935-1980) in the LSLE are scarce and were made only in summer at depths exceeding 250 m but not necessarily at the same depth. Hence, we agree with the reviewer that the regression is a very crude estimate of the average rate of DO decline for the period 1935-1980. We will caution the reader about the reliability of the regression in the revised manuscript by noting: “This historical reconstruction offers a crude estimate of the trend in deep oxygen concentration, given that the exact sampling depth, although always below 250m, might have varied over the decades”. We will also add the following at the end of the method section: “Finally, we use oxygen concentration measured in the 1930s by clerics from Université Laval to extend the time series (Figure 4).”

For as long as we have a historical record, the salinity and temperature of the bottom waters of the Laurentian Channel have increased steadily as the relative contribution of the parental waters (Labrador Current and North-central Atlantic Water) have evolved in time. We do not discuss these variations here because they were previously described in Gilbert et al. (2005) and in Jutras et al. (2020b). In these papers the authors show that the temperature and salinity do not directly track the DO concentrations because eutrophication and microbial respiration contribute to the deoxygenation. We will add this information at L158. Transects of the two parental water masses along the Laurentian Channel are shown in Jutras et al. (2020b). It is, however, interesting to note that, despite changes in bottom-water temperatures and salinities, the depth of the oxygen minimum has not changed considerably over the last 90 years and sits at approximately 250 m depth or the 27.25 (kg m^{-3}) isopycnal. Finally, we focus on the head of the LSLE because the lowest DO concentrations are found in this region where the strongest consequences on this marine ecosystem are likely to be observed.

RC1: 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.

As noted above, we feel that it would be unreasonable to repeat all this information here as it would unduly lengthen the manuscript and because the goal of the present manuscript is not to discuss in detail the deoxygenation mechanisms but rather to highlight the sudden decline and estimate the change in the areal extent of the hypoxic zone.. Alternatively, we will provide a more detailed summary of the method (i.e., e-OMP), the properties of the source/parental waters to the bottom waters of the Laurentian Channel, how their relative contributions have varied over the last few decades and why, as well as the relative contribution of in-situ (pelagic or benthic) respiration as the bottom waters flow inland from the mouth to the head of the Laurentian Channel. The most recent field data are presented in Figure 6 and results of the e-OMP analysis of these data are presented in Figure 7.

RC1: Figure 6: What are the observations showing, i.e. are they observed measurements or means from several cruises or ….?

Results presented in Figure 6 are a compilation of overlapping field measurements conducted in 2021: 1) from the head of the LSLE to the entrance to the Gulf of St. Lawrence near Pointe-des-Monts from August 25 to 30, 2021 and 2) from Rimouski and throughout the Laurentian, Anticosti and Esquiman Channels, all the way to Cabot Strait from October 23 to 29, 2021. This information will be added to the figure caption, and different symbols will be used to identify the two different surveys.

RC1: 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.

As noted above, we feel that it would be unreasonable to repeat all this information here as it would unduly lengthen the manuscript and because the goal of the present manuscript is not to discuss in detail the deoxygenation mechanisms but rather to highlight the sudden decline and estimate the change in the extent of the hypoxic zone. Alternatively, we will provide a more detailed summary of the method (i.e., e-OMP), the properties of the source/parental waters to the bottom waters of the Laurentian Channel, how their relative contributions have varied over the last few decades and why, as well as the relative contribution of in-situ (pelagic or benthic) respiration as the bottom waters flow inland from the mouth to the head of the Laurentian Channel.

RC1: L. 175: Should be ‘NACW’.

Thanks for picking this up, it will be corrected in the revised manuscript.

RC1: L. 187: ‘temperature’

Thanks for picking this up, it will be corrected in the revised manuscript.

Finally, we noticed that the data above 150 µmol/kg are presented in magenta instead of yellow on Fig. 2. This error will be corrected in the revised manuscript.