Dear Reviewer,

We would like to extend our thanks for your constructive review of our manuscript. Below, we respond point by point to each of your comments.

Format and Structure

Comment 1: The formatting between sections 1 and 1.1 is unusual and should be revised for consistency.

To improve consistency, we have revised the structure of Section 1 to include subheadings for all content. Specifically, the general introduction text has now been incorporated into "1.1 Background", and the subsequent sections have been re-numbered to "1.2 Previous modeling studies" and "1.3 Research objectives".

Comment 2: Sections 3.4, 3.5, and the last paragraph of the Conclusion could be revised into a dedicated Discussion section. Currently, the discussion is fragmented and lacks cohesion. A clear and standalone discussion would improve the paper's overall impact.

The authors had discussed and agreed on combining results and discussions because the paper deals with 5 thematically distinct yet related topics. For each, the results are presented alongside their interpretation to maintain a logical narrative. We recognise that there are both advantages and disadvantages to this structure but decided that, in this case, it was better to combine results and discussion. We note that Reviewer 2 commented positively on the clarity of the paper's logic which we feel would be impacted negatively if we were to return to a more conventional structure. Therefore, while respecting the reviewer's comment, we prefer to keep the structure of the paper as is.

Comment 3: Consider incorporating a paragraph that synthesizes past findings and highlights how this study builds on previous work. This could serve as a conceptual model-like summary and strengthen the narrative arc from historical data analysis to future outlook.

We would like to note that a synthesis outlining how this study builds on past work is already integrated within Section 1.2 (now 1.3). A detailed review and comparison of previous modeling efforts, including differences in model structure, coefficients, and OUR is already provided in Supplementary Material Section S1. This section is explicitly referenced in Section 1.1 (now 1.2) of the manuscript to guide readers seeking further background and context.

Purpose and Abstract

Comment 4: While the final paragraph of the paper presents a clearer articulation of the study's aim, this clarity is not reflected in the Abstract. The Abstract currently focuses too much on methods and lacks a high-level synthesis of the findings and implications. Revise the Abstract to emphasize better the study's scientific significance, including the conceptual contribution and potential applications.

We revised the Abstract to more clearly highlight the study's scientific significance and broader implications. We believe that the updated Abstract now better reflects the scope, findings, and applied relevance of the manuscript.

Discussion

Comment 5: Some explanations—for example, differences in coefficients used in prior studies—are mentioned but not thoroughly discussed. Please elaborate on the potential consequences of these coefficient differences on the model output or interpretation.

We agree that transport parameter assumptions can significantly affect modeled biogeochemical rates. In response, we added clarification to the final paragraph of Section 3.3 to emphasize that the choice of transport parameters has a strong influence on rate estimates. In our along-channel framework, faster

advection or greater diffusivity reduces residence time which, in turn, requires higher remineralization rates to reproduce the observed along-channel concentration gradients. We now explicitly state that this sensitivity helps explain the wide range of OUR estimates in the literature and show how recalculating previous studies using the tracer-constrained advection speed from TReX (5×10^{-3} m s^{-1}) yields rates more consistent with our findings.

Comment 6: A more comprehensive discussion could highlight the novelty of this long-term dataset, its value for understanding biogeochemical cycling, and its implications for future monitoring or management.

We completely agree that the long-term dataset is a core strength of this study. To highlight this, we added a sentence in the Conclusion to emphasize its uniqueness and relevance to understanding long-term change in the Gulf and St. Lawrence Estuary. We would like to note that this dataset is only a subset of a larger, integrated, biogeochemical time series spanning the St. Lawrence Estuary, Gulf, and Saguenay Fjord. This full data-product, that includes 20 years of oxygen, carbon, nutrient, transient tracer, and stable isotope measurements is archived through the Canadian Integrated Ocean Observing System – St. Lawrence Global Observatory (CIOOS-SLGO) and will be formally released following the submission of a descriptive/instructive companion data paper to *Earth System Science Data* later this year. The full scope and significance of the data product will be described in greater detail in that publication.

Comment 7: Line 424. This appears to be the key assumption of the entire study. Could the authors comment on any potential side effects or unintended consequences associated with this assumption?

We presume the reviewer is referring to the assumption that pure oxygen from industrial sources could be injected and retained within the deep inflow at Cabot Strait. This assumption and its limitations are addressed in the revised manuscript in the final paragraph of Section 3.5. There, we detail the simplifications of our model, including the assumed 100% oxygen retention and the preservation of the water column stratification. We also clarify that the goal of this study is to provide a first-order conceptual estimate of re-oxygenation potential. Finally, we state that a more complete evaluation of the remediation strategy (including injection efficiency, plume dynamics, and system feedback) would require a 3D framework.

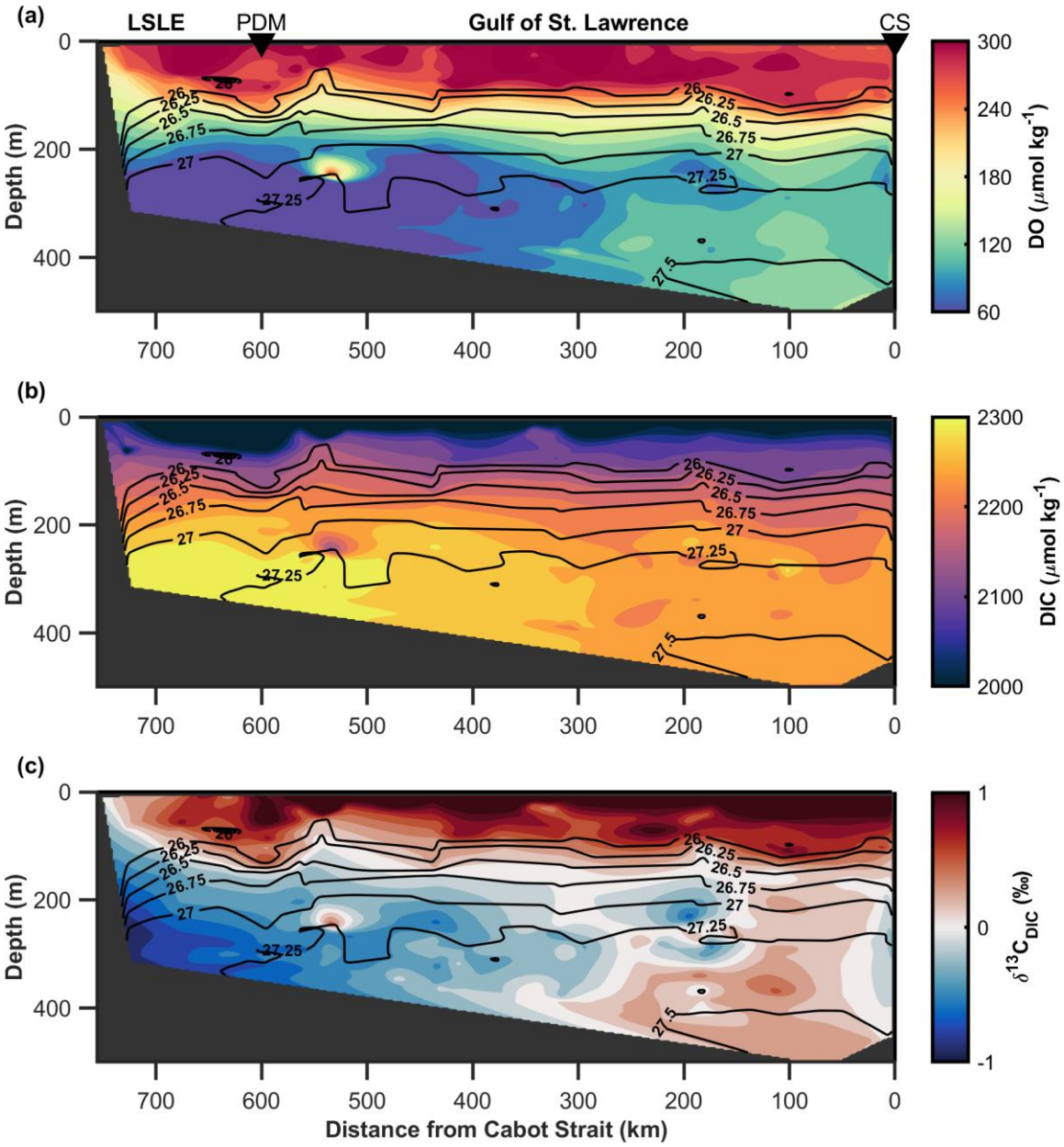
Comment 8: Model Limitations are mentioned in lines 493-497. Although the authors include error estimates, the limitations of the numerical model are not clearly discussed. Given that short-term variations (e.g., tidal or diurnal changes) may exceed long-term trends in certain locations, the authors should discuss how these short-term dynamics may impact model accuracy and interpretation.

We agree that short-term and spatially localized dynamics, such as tidal mixing and slope-driven variability, can influence dissolved metabolite distributions in ways that are not captured by a 1D model. We added a couple of paragraphs to Section 3.2 that provide estimates of potential vertical DO fluxes (using Fick's Law) under a range of plausible diffusivity scenarios. We compare these values to the modeled OUR and demonstrate that, even under elevated mixing assumptions, vertical diffusion is a secondary contributor to the DO budget on a regional scale. We also noted that localized mixing and cross-channel transport may redistribute metabolites laterally, and that these effects cannot be resolved in our 1D framework. This limitation underscores the need for more comprehensive 3D modeling approaches to fully evaluate short-term and spatially variable dynamics.

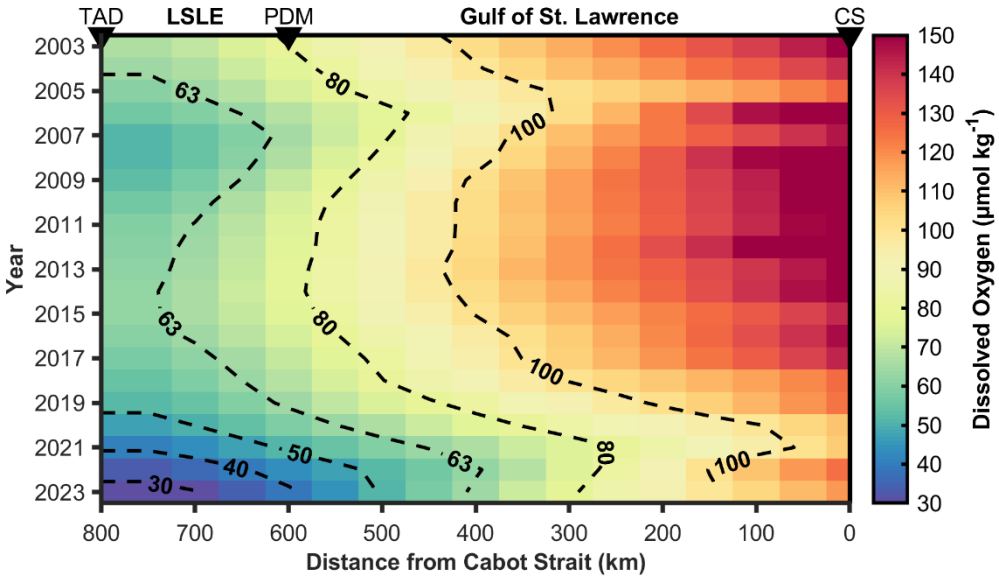
Figures

Comment 9: Many figures are overly crowded. For instance, Y-axis labels are dense, and contour lines frequently overlap with annotations.

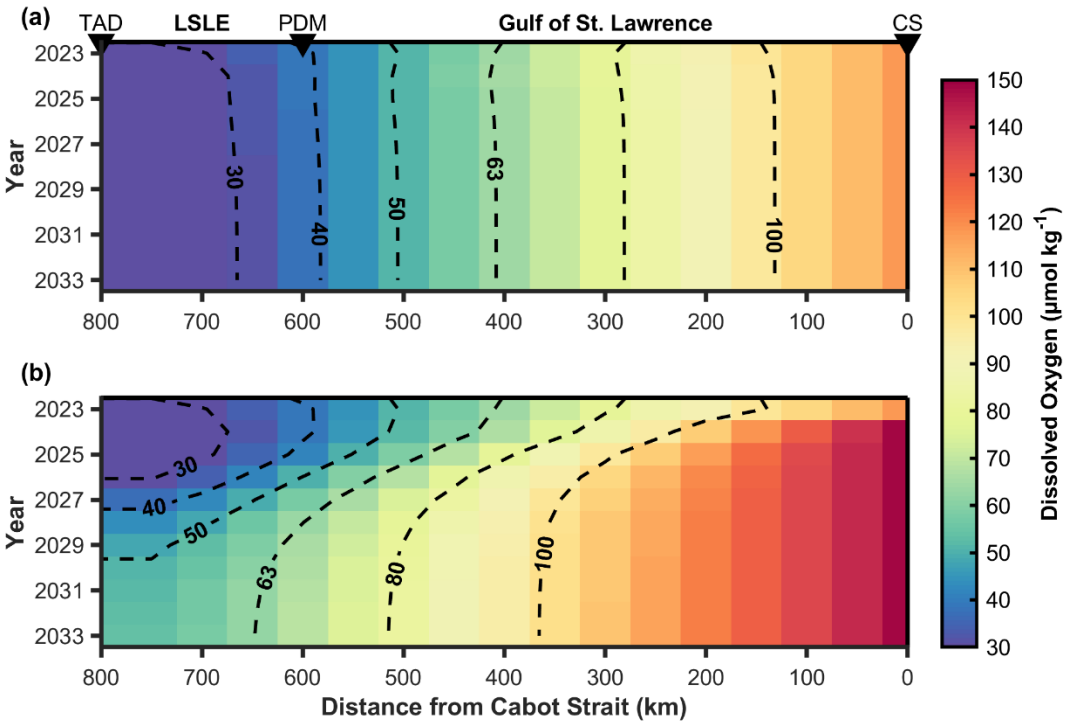
We carefully examined all figures and chose to revise Figures 3, 5, and 6 to improve readability. For figure 3, contour labels were decreased in font size and frequency to avoid overlap while contour lines were thinned out and smoothed. For figures 5 and 6, the y-axis was decreased to count in 2-year increments and the x-axis in 100-km increments to decrease clutter. We believe these changes improve visual clarity without compromising the presentation of key information. The revised figures are presented below.



Revised Figure 3



Revised Figure 5



Revised Figure 6