

This response includes the replies to the second round of revision of Reviewer #2, and M. Jutras. Additionally, it includes the minorly revised responses to comments from Reviewer #1 made during the first review iteration.

Response to Referee #2

We thank Referee #2 for the constructive and detailed second review of our manuscript. We have substantially revised and restructured the manuscript in response. In particular, we clarified the focus of the study, combined the Results and Discussion sections, and addressed concerns regarding interpretation and robustness. Below, we provide a detailed point-by-point response to each comment, with our replies presented in **bold**. An overview of the major structural changes is provided following the general comment.

Dear Authors,

Please find below my second review of your manuscript titled "The changing composition of the Gulf of St. Lawrence inflow waters observed from transient tracer measurements." I appreciate your efforts in addressing some of the concerns raised in my initial review. Below, I offer my comments on this new version of the manuscript.

In my first review, I recommended a substantial re-structuring of the study, if not outright rejection. While I acknowledge that some changes have been made, I feel that the manuscript remains quite similar to the previous version and still requires further revisions. I am aware that the other reviewers recommended only minor revisions, which may influence the overall direction of your decision-making. However, in light of the current state of the manuscript, I would still suggest a more in-depth rework.

With that in mind, I recommend rejecting the manuscript at this stage, as I believe it still does not meet the high standards expected for peer-reviewed publications. Specifically, the manuscript contains several repetitions, some unresolved questions, and areas where further clarification or refinement is needed. I also have concerns about the overall contribution of the study to the literature. Below are my detailed comments.

*** What is this study about? ***

The primary contribution of this study is to analyze water masses using the mean age of water parcels, determined from transient tracers, in addition to temperature and salinity. While other studies have used non-conservative tracers (e.g., O₂, nutrients) for similar analyses, the use of transient tracers offers a novel perspective. The scientific basis seems sound, and the methodological approach is valid.

*** What are the main findings? ***

The study appears to have one key finding: that the NACW fraction of the water entering the Laurentian Channel has not yet reached 100%, as previously believed, but is still increasing. This is effectively demonstrated in Figure 6b.

The authors' own response to my earlier comments highlights those key findings

"The main novelties of this study, extending existing GSL research, are: 1) The observed ongoing shift towards increased NACW contributions to the deep water, as of 2022, which had been observed to have fully happen prior to our analysis. 2) The use of transient tracers in water mass analysis, to include insights on circulation and mixing processes." [quote from their response]

*** Are these findings sufficient to justify a peer-review publication? ***

While the scientific results themselves are valuable, the discussion section remains rather thin. This is not unexpected, given the limited scope of the findings. In fact, there is considerable repetition in both the Results and Discussion sections. For instance, the following sentences essentially repeat the same idea (and are located within just a few lines on page 19):

L. 390: recent studies, such as Jutras et al. (2023b), suggest that since 2021 the deep water within the Laurentian Channel has been almost entirely composed of NACW.

L. 413: The LCW and NACW fractions estimated from earlier studies (e.g. Gilbert et al., 2005; Jutras et al., 2020, 2023b) reported ongoing variabilities in the composition of the entering deep water up to a complete disappearance of LCW in 2021.

L. 415: In Jutras et al. (2023b), LCW fractions at Cabot Strait were reported as 0 % in 2021.

L. 393: This highlights that the transition towards NACW dominance is ongoing rather than complete

L. 401: As of 2022, NACW influence is continuing to rise throughout the Laurentian Channel...

L. 405: ... the ongoing trend of increasing NACW influence remains identifiable.

L.415: Our observations, however, indicate that this transition is still ongoing as of 2022.

These are important points, but they say the same things: we thought we had 100% NACW in 2021, but this new study shows that it was not the case and that the NACW fraction is still increasing. These findings are repeated one more time in the conclusion.

So, are these findings sufficient to justify a peer-review publication? I honestly don't think so... I feel that the entire study could hold in 2 pages.

*** Concerns About the Robustness of the Findings ***

In addition to the relative lack of new findings, I am afraid that some of those findings are not robust and the authors did not address very surprising results. I will try to go through a few of them related to Issues with LCW Fractions

How can the LCW fraction be 100% at several stations in the western Gulf and estuary (Figure 5a)?

The authors acknowledge this as a potential issue (L. 336), but the explanation—uncertainty in fraction calculations—seems inadequate. The authors mentioned that their uncertainty on LCW fraction is 21% (thus $100\% - 21\% = 79\%$ LCW), but this still seems problematic. The largest LCW fraction observed was believed to be $\sim 75\%$ in 1930 and has decline since. How can the authors be comfortable with their results that re-write our understanding of the deep Laurentian channel waters given those surprising results?

In addition, how do the authors explain that the LCW fraction went from $\sim 75\%$ in 1930 to $\sim 50\%$ in 2000 (70 years); but then the same magnitude of change ($\sim 75\%$ to $\sim 50\%$) would have occurred in only 4 years between 2018 and 2022. Is this reasonable? Does it call into question previous research?

I would expect that such surprising results would be discuss further and, as mentioned earlier, I am afraid that those results may bring more confusion in the literature if the paper is published. Those results may be well related to mesoscale activity and the uneven mixing happening near Cabot Strait, but the current explanations are not

convincing to me at this stage.

L. 444, the authors say "Despite increasing mean ages and declining LCW fractions from 2018 to 2022 [note: we are talking about huge changes, from 80% LCW to <40%; Figure 6b], DO levels remained relatively stable, with only a slight decrease. This suggests that variabilities in oxygen concentrations might not be directly tied to water mass changes alone and are likely influenced by additional factors"

-> This is also quite surprising given that it is well known that the LCW and NACW are different DO properties. Could this suggest instead that your method is not working properly?

Response:

We appreciate the detailed comments of the reviewer and recognize that she/he remains unconvinced by the scope and framing of the previous version of the manuscript. We agree that the earlier version placed disproportionate emphases on water mass fraction estimates, which may have obscured the primary observational key findings of this study. In response, we have substantially restructured the manuscript. The abstract, conclusion, and overall narrative have been refocused on ventilation timescales derived from transient tracer observations, which represent the observational results of this work (Also the title has been modified in response). The Results and Discussion sections have been combined to first present and interpret the tracer derived mean ages, hydrographic parameters and their spatial appearance, before introducing water-mass analysis as a complementary and interpretative framework.

The water-mass analysis is now framed as an exploratory and supporting tool, reflecting the uncertainty and limitations associated with endmember definition and the difficulties of such an analysis in a changing environment. A key challenge is that the entering LCW and NACW are characterized by not only a broad range of tracer-derived mean ages, but also temperature and salinity span substantial ranges in the source water masses (e.g. 4.4 to 8°C for NACW and -0.7 to 3.2°C for LCW). As a result, a single endmember value can't fully capture the variability in the mixing water masses and introduces uncertainty. To address this directly, we now include a sensitivity analyses using two extreme endmember scenarios (Figure presented in the Supporting Information) and demonstrate that, while absolute fraction values vary substantially, the qualitative spatial gradient and temporal trends are robust across all tested scenarios and across two independent approaches (linear fraction and 2-IG-TTD).

With respect to the appearance of locally high LCW fractions (approaching 100%) near the Lower St. Lawrence Estuary, we agree that these values should be interpreted with caution. We have raised this explicitly in the revised manuscript and discussed that the values are highly impacted by the endmember choice and minorly reflect vertical mixing, enhanced at these longitudes, including shoaling of deep waters shown in Figure 4. Similarly, the apparent rapid changes in the proxy time series in LCW fractions between 2018 and 2022, now include the variability in endmember choice and show the difficulty in estimating exact fraction values. We explicitly state that the fraction values should not be interpreted as exact quantitative measures, but rather the apparent gradient as indicator of evolving source water influence, which is consistent with the tracer-derived ventilation signal.

Finally, we acknowledge the reviewer's point regarding repetition in the previous version. In the in-depth restructured manuscript, we have removed redundant statements and avoid repetitions with the combined Results and Discussion section.

We believe that the new tracer-derived evidence of ongoing ventilation changes, and a cautiously applied water-mass analysis, provide meaningful and timely contributions to the literature. In this framework, tracer-derived mean ages add value by integrating information on ventilation history that cannot be resolved from hydrographic parameters alone.

*** Other Comments ***

L. 20: "The tracer data reveal an unexpected age distribution, with 'older' deep waters present near the Gulf's entrance, whereas 'younger' water is found further inshore, contrary to the expected estuarine circulation pattern, which transports deep water inland (increasing age along the flow path)."

-> I already commented about this in my previous review, yet this mis-leading statement still remains. This is not unexpected if the ratio LCW:NACW is changing and maybe not even surprising. Stating that this is "contrary to expected estuarine circulation patterns" is extremely mis-leading and should be removed from the abstract.

To continue on this comment, while the mean age can be used in the water mass analysis, I would not present it as a result because of this confusion that it brings. I would focus on the LCW:NACW fraction instead.

Response:

We thank the reviewer for raising this point again and agree that the wording could be misleading. As we have substantially rewritten the abstract, removed this statement entirely, and put focus on not mentioning contradictory statements.

L. 31: It describes the transport of surface waters to the interior of the ocean, *where the age of the water refers to* the time since a water parcel was last in contact with the atmosphere.

-> When one says "where ... refers to..." we understand that the concept has been introduced before, which is not the case here.

Response:

We rephrased this sentence in the introduction, not needing to have the concept introduced before. It now reads: 'It describes the transport of surface waters into the ocean interior, and vice-versa, a process commonly characterized using the concept of water-mass age, defined as the time since a water parcel was last in contact with the atmosphere.'

L. 42: By combining measurements of both tracers enables to resolve water mass formation, mixing, and circulation patterns, which are key to understanding distribution of biogeochemical properties, such as oxygen.

-> problem with this sentence.

Response:

We revised the sentence to clarify that the tracer measurements provide constraints on 'water mass formation and mixing processes, which in turn support interpretation of circulation patterns and distribution of biogeochemical properties, such as oxygen.'

L. 103: The cold (< 1 °C) and *saline* intermediate layer...

-> Is *saline* a good qualification for the CIL?

Response:

We thank the reviewer for raising this point and have, in response, removed 'saline' from the sentence, as temperature is the determining parameter.

L. 183: We focused specifically on data collected after 2010 in the region near the mouth of the Laurentian Channel, where the deep water enters, while still representing LCW and NACW as distinct unmixed water masses (see Figure 4b).

-> The fact that LCW and NACW are un-mixed when they enter the Laurentian channel is news to me and goes against previous understanding that those waters are mixed outside the channel. For example: Brickman, D., Hebert, D., & Wang, Z. (2018). Mechanism for the recent ocean warming events on the Scotian Shelf of eastern Canada. *Continental Shelf Research*, 156, 11-22.

Response:

We thank the reviewer for highlighting this and directing our attention to this paper. We have revised the manuscript to clarify that LCW and NACW mix at the tail of the Grand Banks (south of Newfoundland) with these anomalies penetrating into the Gulf of St. Lawrence via the Laurentian Channel. The Brickman et al. (2018) reference has been added to support this statement and Figure 1 was updated to label the Grand Banks.

Section 3.4 - Deep water time proxy at Cabot Strait

"A *proxy time series* at Cabot Strait was constructed to better resolve temporal changes in mean age and water mass composition and potentially related water mass properties, such as oxygen."

-> A proxy of what?? the entire sections says nothing about what kind of proxy is this. Are we talking about a *proxy for mean age at Cabot Strait* (e.g. Figure 6A)? Then just say it.

The same applies for L.384 where the authors mentioned "proxy time series" without explanation.

Response:

We updated the 'proxy time series' to 'proxy time series of mean ages at Cabot Strait' in section 3.4 and also later in the revised Results and Discussion section.

Table 1: Looking at the table, one can estimate that a 50:50 mix between NACW (~86

years) and LCW (~12 years) should lead to a ~50 years water mass. This seems clear from Figure 6 and in my view the repetition of figures for age and fraction is just a repetition of the same information (e.g. Figures 4a and 5a show mirrored observations).

Response:

We thank the reviewer for this comment to emphasize our attention on reducing repetitions. In the revised manuscript, we have updated the figures in the results section to avoid repetition of information, moving figures with repetitive information to the supporting information as well.

L. 411: "therefore, the measurements are solely influenced by deep water entering through Cabot Strait"

-> Are you sure? Then how do you explain 100% LCW in the western part of the gulf?

Response:

We thank the reviewer for raising this point. In the revised manuscript, as mentioned in the general response, we clarified in detail that a few samples, showing near 100% LCW fractions, should be interpreted with caution, as they are unlikely to represent purely LCW deep water. These values may be influenced by overlying younger, colder, and fresher waters (see Figure 4 in the new version) and by vertical mixing, but largely depend on the choice of endmember values.

L. 421: The limitation of relying on, e.g., oxygen as an indicator for water mass analysis are evident in our proxy timeseries at Cabot Strait (see Figure S2), where only minor overall changes are detected despite high variability.

-> It is not evident to me what the authors refer to.

L. 440: "However, this small apparent decrease should be treated with caution, as the time series is statistically insufficient to confirm a significant trend."

-> A time series cannot be "statistically insufficient".

Response:

In the revised manuscript, we removed the proxy time series for oxygen at Cabot Strait, as it was potentially misleading and the slightly negative slope was not statistically significant (response to M. Jutras comment during the first revision).

We focus the revised manuscript on the deep water ventilation results from the transient tracer observations and only mention the following impacts on deep water oxygen distribution.

L. 465: "The observations show a distinct pattern: near the Gulf's entrance, a signal of older, warmer, more saline NACW, while further inshore along the Laurentian Channel, younger, colder, less saline LCW is evident."

-> We don't see LCW or NACW. It is always a mixture with different LCW:NACW ratios.

Response:

We thank the reviewer for this clarification. We agree that the deep waters always consist of a mixture of LCW and NACW, with varying relative contributions. In the revised Conclusion we focused on replacing any of this language and explicitly mention contributions of the different water masses.

While the study presents interesting data, there remain significant concerns regarding the robustness of the findings, the redundancy in the manuscript, and the interpretation of certain results.

I hope these comments are helpful, and I am sorry that I cannot be more supportive for the moment.

Best regards.

Response to M. Jutras

We thank M. Jutras for the constructive additional comments provided in this second review of our manuscript. We have substantially revised and restructured the manuscript in response to the comments and concerns raised by Reviewer #1, as well as those provided by M. Jutras. Below, we provide a detailed point-by-point response to each comment, with our replies presented in **bold**.

I would like to thank the authors for taking into consideration my comments provided through the BGS interactive discussion. I here provide additional comments in response to the changes made following my comments.

My main comment is about the oxygen trend on the considered isopycnal. The figure provided by the authors in the response to my earlier comment shows that the time series at Cabot Strait is highly skewed based on location, with all earlier data being located on the west side and all later data being located on the east side. Since the circulation, water properties and origin are strongly different on both sides of the channel, it appears important to take this into consideration in the interpretation of the temporal changes, not only for oxygen but also for age and LCW fraction. In fact, the spatial bias would affect the trends shown in Fig. 6. The authors could for instance try to find a year for which data is available on both sides to quantify the spatial gradient and offer a correction on the data.

Response:

We appreciate this comment by M. Jutras and would like to clarify a potential misunderstanding regarding the spatial distribution of the data and the interpretation of the proxy time series.

All observations used in this study were collected in 2022 and are located west of Cabot Strait, along the Laurentian Channel. The spatial gradients discussed in the manuscript are therefore strictly along-channel (from Cabot Strait toward the LSLE) and do not involve comparisons between the eastern and western sides of Cabot Strait.

The proxy time series shown (now only presented in the Supplementary Information) is constructed solely from these along-channel spatial observations made in 2022, combined with an assumed mean transit speed (see Section 3.4). As such, and as no multi-year transient tracer data are available, the apparent temporal evolution reflects a space–time transformation along the advective pathway of the deep inflow, rather than a measured temporal record at Cabot Strait. The fact that earlier inferred times correspond to stations located further inland in 2022 is an inherent feature of this approach and does not represent a spatial sampling bias in the conventional sense.

If this comment refers to potential differences between measurements west and east of the eastern tip of Anticosti Island, we note that the analysis focuses on the deep-water layer, which is generally understood to advect along the Laurentian Channel. To our knowledge, there is no observational evidence for

systematically different deep-water circulation regimes on either side that would affect the along-channel gradients considered here.

In the revised manuscript, we no longer present or interpret oxygen data, and the primary conclusions are drawn from robust spatial gradients in ventilation age along the Laurentian Channel. The proxy time series of mean ages and LCW fractions at Cabot Strait is retained only in the Supplementary Information and is explicitly presented as an illustrative extension of the spatial observations.

Finally, while lateral variability, isopycnal shoaling, and limited vertical mixing toward the LSLE are discussed based on the added along-channel profile analysis (see Figure 4 in the revised manuscript), these effects are interpreted to only minorly effect the along channel ventilation.

Here are additional comments following on earlier comments:

New figure 7 nicely shows the vertical structure of the system. However, I would suggest to discuss this structure in section 4.3 when it is mentioned that the LCW is only present at the head of the LC, and where a reference is made to Figure 4. As we see on Fig. 7, LCW is present all along the channel, but simply at different depths. Overall, throughout the text, it should be clear that the presence of LCW only at the head of the LC is due to time variability in the supply of LCW, for instance at L466-467 (track changes document).

For turbulent mixing, my comment was rather referring to a methodological question related to the tracer data. Given this, and since we see some isopycnal surfaces disappearing as we move inland which would dynamically imply vertical mixing unless the waters are not moving, the additional discussion provided by the authors might not be necessary, and I suggest removing it. (I thank the authors for citing my paper, but I do not think it demonstrated specifically the impact of vertical mixing.)

Response:

We thank M. Jutras for these additional comments and suggestions.

We agree that the vertical structure shown in Figure 7 (now Figure 4) clearly illustrates that LCW is present along the entire Laurentian Channel, but at different depths and with different contribution factors, rather than being restricted to the head of the channel. In the revised manuscript, we have clarified this point throughout the text and removed wording that could be interpreted as implying that LCW was only present at the LSLE.

Regarding vertical and turbulent mixing, we appreciate the clarification that this comment referred to a methodological interpretation of the tracer data. The paragraph in the previous version that explicitly invoked vertical mixing has been removed. In the revised manuscript, the discussion of vertical mixing is limited to observations from the deep-water profiles shown in Figure 4 (previously Figure 7) and to a cautious interpretation of possible interactions with overlying deep waters near the LSLE as the channel shoals.

Response to Referee #1

We thank Reviewer 1 for the constructive and helpful comments and by addressing these, we believe, the manuscript improved in quality and clarity. Below, we provide a point-by-point response to each comment, with our replies presented **bold**.

Review of Gerke et al.'s "The changing composition of the Gulf of St. Lawrence inflow waters observed from transient tracer measurements" (manuscript #: egusphere-2025-3999)

General comments:

The authors of this manuscript assess the mean ages of the waters in the Gulf of St Lawrence and suggest that there has been a gradual increase in the proportion of North Atlantic Central Waters from inshore areas to the entrance of the Gulf of St Lawrence is evidence of a shift towards deep waters dominated by North Atlantic Central Waters since 2022. They use transit-time distributions to derive the mean ages and then integrate them into a water mass analysis to analyze the water mass composition of the region. The authors use transient tracer measurements collected and described in Stevens et al. (2024) and use the same density surface to represent the core of the deep water inflow. CFC-12 has seen its maximum concentrations in the atmosphere, but SF6 continues to increase; CFC-12 is useful for water masses between 23-85 years in mean age whereas SF6 is useful for younger water masses and in helping to resolve the ambiguity in mean age estimates using the Inverse Gaussian transit-time distribution approach (e.g., Guo et al., 2025). Although, it seems like the authors here just relying on SF6 according to their Appendix B and back-calculate CFC-12, which makes me wonder what information the authors are getting from CFC-12. There may be

data issues with regions lacking SF₆ and in the regions where waters approach the time scale of CFC-12 beginning to be emitted, there are signal issues. I appreciate the trend analysis and other detailed efforts that went into this manuscript, especially to the end that the relative proportion of waters is changing, but the authors need to perform some additional analyses to convince me of their interpretation of the data beyond that. I suggest minor revisions. Specific comments are listed below:

Response:

We thank the reviewer for their constructive comments, which we believe will help improve the manuscript. While we will respond in detail to the specific points raised, we would first like to clarify a few possible misunderstandings.

We did analyze the gradual increase in the proportion of North Atlantic Central Waters (NACW) by assessing mean ages, and we interpret this as evidence of an ongoing shift in deep water composition as of 2022. However, previous studies (Gilbert et al., 2005; Jutras et al., 2020) also observed this shift but suggested that the transition to 100% NACW had occurred prior to 2022. This analysis is focused more on ventilation timescales, assessed from transient tracer observations, in the newest version of the manuscript showing this ongoing proportion increase in NACW.

For our analysis, we relied on transient tracer data (CFC-12 and SF₆), as described in this study, not as in Stevens et al. (2024). Stevens et al. (2024) focused on the tracer (CF₃SF₅), which had been released in 2021 on the core deep water isopycnal of $\sigma=27.26$ kg/m³. Because all three tracers were measured simultaneously from the same water sample, and the main objective of these cruises was to track the released tracer (i.e. to analyze the deep water spread and dispersion over time in the Gulf of St. Lawrence), the sampling strategy was centered on this isopycnal, resulting in a high density of measurements at this depth.

This said, within the Gulf we have measurements of both SF₆ and CFC-12 at all locations. Only outside the Gulf, in the North Atlantic, there is a lack of SF₆ measurements (Data from GLODAP).

In response to these points, we added clarity to the manuscript in Section 3.2 outlining the observations included in this study and by updating the methodology in Appendix B.

Specific comments:

Line 36: You should add “abiotic” in front of “transient tracer concentrations” here; you could say “passive” but that would exclude radioactive transient tracers

Response/Suggested changes in the manuscript:

Thanks to the reviewer for pointing this out and we added ‘abiotic’ in the revised manuscript.

Line 151: I’m not sure why a Delta/Gamma ratio of 1.2 was chosen; Ebser et al. (2018), which a co-author on your study was also a co-author on, found different ratios for different water masses (e.g., 0.5-0.6 where Labrador Sea Water dominates and 0.9 where North Atlantic Deep Water dominates). Please say more about Figure B1, which seems to be where 1.2 came from. Please also explain why you don’t consider using a different ratio of mean age to half-width for North Atlantic Central Waters as opposed to Labrador Current Waters.

Response:

We thank the reviewer for drawing our attention to Ebser et al. (2018). That study focuses on samples collected in the Eastern Tropical North Atlantic, off the coast of northwest Africa. It reports Δ/Γ values of 0.5-0.6 for the Labrador Sea Water and $\Delta/\Gamma=0.9$ for the North Atlantic Deep Water. However, these are different water masses than the North Atlantic Central Water (NACW) and the Labrador Current Water (LCW). The LCW represent near surface waters within the Labrador Sea and the Newfoundland shelf area, which subsequently mix with Labrador Sea Water found at depths of 1000-2000m. North Atlantic Deep Water (present around 3000m depth) is also distinct from NACW. Notably, Ebser et al. (2018) mention Atlantic Central Waters (depths above 800m) having a $\Delta/\Gamma=1.0$, which likely better represents NACW conditions.

In our study we select $\Delta/\Gamma=1.2$ for the Gulf of St. Lawrence deep water and adjacent regions near its entrance for several reasons. First, we used SF₆ and CFC-12 concentrations sampled and measured simultaneously at the same location, applying the assumptions outlined in the manuscript. Following a standard approach to gain information on the Δ/Γ ratio, we compared the mean ages derived from the individual tracers under varying Δ/Γ ratios. Agreement between the two tracer-derived mean ages (yellow lines in Figure B1a) indicates that the chosen Δ/Γ reflects local advective and diffusive transport characteristics. Since perfect agreement was not achieved across all ratios, and CFC-12-based ages were generally higher than those derived from SF₆, we

examined whether this discrepancy could be related to the atmospheric decline of CFC-12 since 2002. To test this, we compared measured and calculated CFC-12 concentrations (again under varying Δ/Γ ratios) relative to observed SF₆ (e.g. Figure B1b). The calculated values were consistently slightly higher than the measured values across the full range of SF₆ concentrations (2-8ppt), with no systematic trend towards higher SF₆ values representing recently ventilated waters. We therefore concluded that the atmospheric decline of CFC-12 is not a relevant factor for our TTD analysis in this region.

As shown in Figure B1a, the tracer mean ages converge toward the 1:1 line as Δ/Γ -ratio was increases. However, Stöven et al. (2015) concluded that ratios approaching 1.8 make the age estimates highly sensitive to tracer saturation and age deviations. Consequently, we selected $\Delta/\Gamma=1.2$ as an optimal balance: it reflects slightly diffusive dominated transport ($\Delta/\Gamma>1$) while avoiding excessive age deviations ($\Delta/\Gamma>1.6$).

We consider this choice consistent with conditions in LCW and NACW in the North Atlantic, where water residence times before entering the Gulf are relatively short. Unfortunately, only CFC-12 was measured in these regions, preventing us from computing local Δ/Γ ratios.

In addition to further assess the plausibility of our chosen ratio, we examined Δ/Γ results derived from a one-dimensional Gaussian pipe model from the spreading of the CF₃SF₅ tracer analyzed in Stevens et al. (2024). In this model, Δ/Γ is calculated from the advective ($\Delta=ut$) and diffusive ($\Gamma=(2kt)^{-1/2}$) terms. These resulted in values for the two surveys of 1.2 and 1.5, respectively, well within the same range as the ratio applied in our study, thus providing additional support for our choice.

To clarify this in the manuscript, we updated the Appendix B, outlining more clearly the methodology for the TTD analysis, and how we justified the use of the Δ/Γ -ratio across all regions.

To include Δ/Γ values reported in Ebser et al. (2018), as suggested by the referee, we evaluated how the inferred LCW fraction would change when applying individual Δ/Γ -ratios for LCW (0.5) and NACW (0.9), when computing mean ages and mean age endmembers of the water masses. This analysis yielded even higher LCW contributions than those already inferred in our study and we therefore did not include this in the revised version.

Lines 167-168: According to Guo et al. (2025), your estimates of the mean age are likely biased wherever you only use CFC-12 and no SF₆ so did you see any spatial discontinuities or other signs that your estimates were different where you have SF₆ vs where you do not? You can evaluate the bias you would have in regions where you have SF₆ measurements by doing the mean age estimation with both CFC-12 and SF₆ and again with only CFC-12 to assess the bias. Analysis was done to corroborate the CFC-12 measurements with the back-calculated CFC-12 concentrations in Appendix B where there are SF₆ data but it's unclear to me what information CFC-12 is then providing.

Response:

We thank the reviewer for raising this point. As explained in our response to the previous comment, the simultaneous sampling of CFC-12 and SF₆ throughout the Gulf ensures that Δ/Γ can be chosen consistently, and that we have both tracers sampled at all locations. Thus, there are no discontinuities in mean age estimates between sites with and without SF₆ measurements within the GSL and LSLE.

In the adjacent Atlantic, SF₆ measurements are indeed lacking, and as Guo et al. (2025) have shown, mean ages based solely on CFC-12 can be biased when applying a fixed Δ/Γ without local determination. In our analysis, however, we use the Atlantic data only to calculate multi-year average mean ages, where the associated uncertainty in the averages is weighted more heavily than the exact choice of Δ/Γ .

In response to this comment, besides clarifying the sampling strategy and methodology (see Appendix B), we added a comment on the uncertainty arising from only measuring one tracer in the North Atlantic (see Section 3.2). This uncertainty explanation was also included in Appendix C, outlining general uncertainties for the mean age estimates (including Guo et al., 2025 as a reference).

Lines 174-177/Equations 1-4: Is the water in the Gulf of St Lawrence exclusively composed of LCW and NACW? There's also the cold intermediate layer and surface/warm slope water, I thought. Also, while the mean ages of two IG TTD for LCW and NACW would linearly sum to a new mean age, the resulting TTD will not be IG. So are you assuming that the TTDs for LCW and NACW are not IG but their sum is IG (in which case the TTDs for LCW and NACW will still need to have their means linearly combine)? Or are you going to use a sum of two IGs as your TTD?

Response:

We thank the reviewer for this comment. The deep water we focus on in this study does consists solely of LCW and NACW. While intermediate and surface waters are present in the Gulf, they do not occur at the $\sigma=27.26$ kg/m³ isopycnal. At this depth mixing with surface and intermediate waters is highly unlikely, as also presented in Jutras et al. (2020) ‘While the contribution of the CIL is important for the intermediate waters of the Laurentian Channel (100–150 m depth), the deep waters (below 150 m) are composed almost exclusively of a mixture of LCW and NACW (see Section 3.1).’. In the revised manuscript we do add a potential of limited vertical mixing present near the LSLE, as our deep water profiles show shoaling of the water through disappearing isopycnal surfaces. However, the focused deep water isopycnal is present at depths between 250-300m, closer to the bottom of the channel, very unlikely to be affected by the CIL or even the surface layer.

We appreciate the reviewer raised the point about the combination of two IG-TTDs. As shown by Stöven and Tanhua (2014), a 2-IG-TTD approach can be applied, in which mean ages from two different water masses are linearly combined using a mixing factor α (see the following equation).

$$\Gamma = \alpha * \Gamma_1 + (1 - \alpha) * \Gamma_2$$

For our case, assuming $\Delta/\Gamma=1.2$ for both NACW and LCW, the average mean ages are $\Gamma_1=86.5$ years and $\Gamma_2=12.5$ years, respectively. The mixing factor α can then be determined for each computed mean age value within the Gulf. The analysis yields results, that are consistent with those obtained from our water mass analysis based on temperature, salinity and mean age (see Figure below).

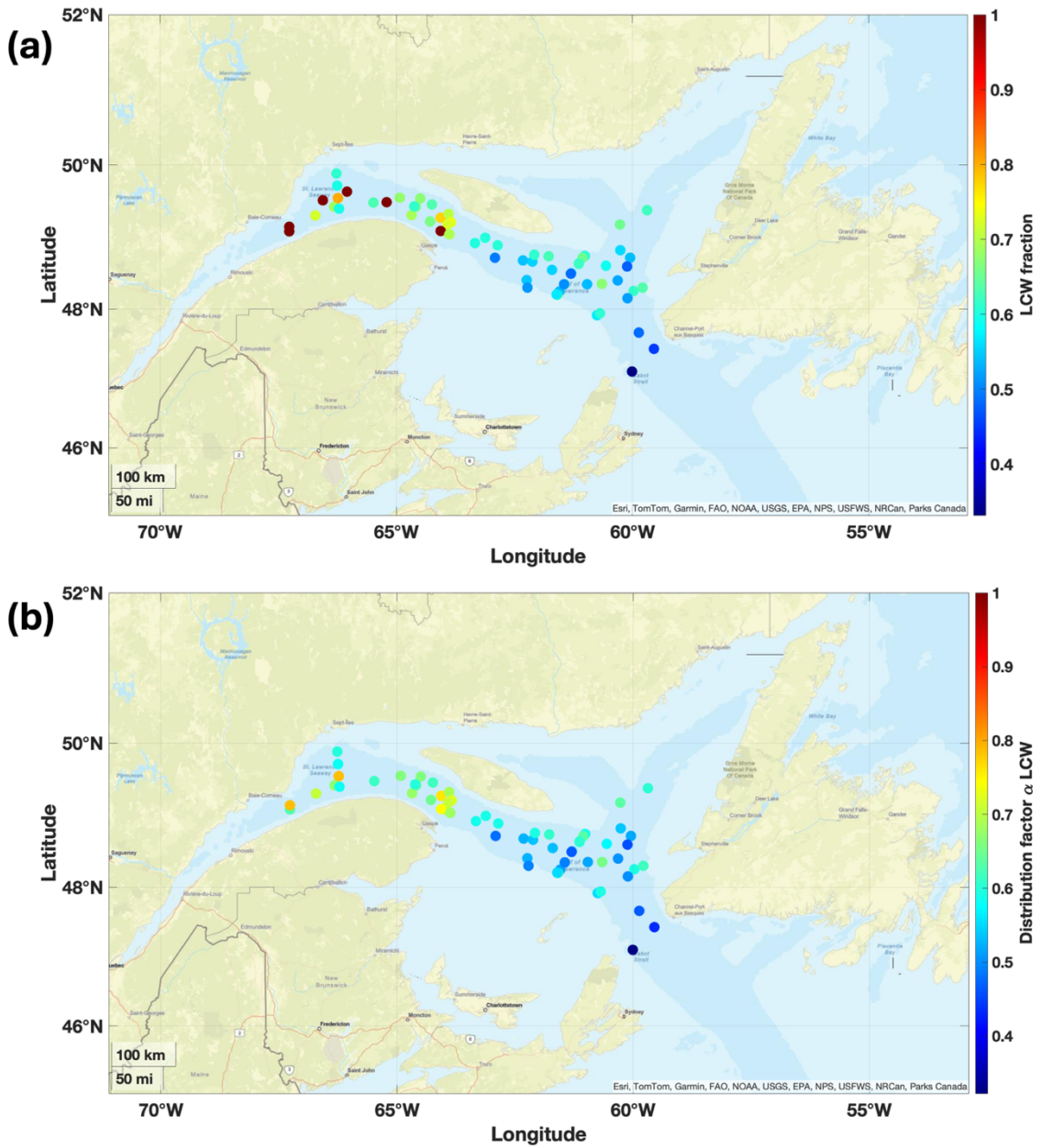


Figure: a) LCW fraction on the $\sigma_\theta = 27.26 \text{ kg/m}^3$ isopycnal deep water in the Laurentian Channel from θ , S_p and mean age (Γ) observations plotted on a map. The endmembers used in this calculation are provided in Table 1. b) LCW mixing factor on the $\sigma_\theta = 27.26 \text{ kg/m}^3$ isopycnal in the Laurentian Channel from the 2-IG-TTD analysis, using observed mean ages (Γ), plotted on a map of the GSL. All the data presented here was collected during the two TReX surveys in 2022.

In the revised manuscript, we have now included the 2-IG-TTD analysis in the Method section (see Section 3.3.2), as well as in the Results and Discussion. The results from the 2-IG-TTD are mentioned in section 4.2, with the figures presented in the Supplementary Information.

Lines 246-247: Is the sudden discontinuity in temperature and salinity at the eastern tip of Anticosti Island physical or actually due to the availability of SF₆ on one side and lack of SF₆ measurements on the other?

Response:

SF₆ and CFC-12 were measured simultaneously at all locations within the Gulf of St. Lawrence. Therefore, the observed discontinuity in temperature and salinity near the eastern tip of Anticosti Island is not related to tracer availability and is most likely of physical origin.

In the revised manuscript we added clarity in the ‘Observation parameters’ section (Section 3.2), that both tracers (CFC-12 and SF₆) were measured simultaneously and throughout the entire Gulf of St. Lawrence and Lower St. Lawrence Estuary.

Figures 4-5: When I see waters with mean ages of 60+ years using the tracer-based constraints you have, there becomes a signal detection issue because of the very low concentrations of CFC-12 in its first couple of decades of being emitted and the fact that you used a backwards calculation to infer the CFC-12 concentrations from the mean ages that you got from SF₆ measurements (lines 450-452). Your Figure 4d makes it look like this generally is reflected in your uncertainties, but your Figure 4b has mean ages of up to 100 years, which shouldn't be detectable using CFC-12 and/or SF₆. Also, your Figures 4a-b makes it look like waters are being ventilated after mixing with waters coming from the St Lawrence River in the western part of the Gulf of St Lawrence and there is a barrier for younger waters southeast of the Gulf of St Lawrence to get into the Gulf there through the Laurentian Channel, which leads to an increase in age as the waters reside for longer within the southeastern portion of the Gulf. Your interpretation is that the younger waters in the western portion of the Gulf are due to a higher portion of LCW mixing with the other waters there but is the mix of high and intermediate proportions of LCW shown in Figure 5 in the western portion of the Gulf with large variability over a small spatial distance due to potential data issues such as the ones I've pointed out in this comment and others? For example, you tend to have higher proportions of LCW where you don't look like you have SF₆ measurements in the western part of the Gulf.

Response:

We thank the reviewer for pointing this out, and we agree that more explanation is necessary. Again, SF₆ and CFC-12 were measured simultaneously at all locations

within the Gulf of St. Lawrence, and the backward-calculated CFC-12 concentrations were only used for the TTD analysis, not for any direct data evaluation.

The fact that mean ages exceed the atmospheric age of a tracer (e.g., 85 years for CFC-12) arises from the interpretation through the transit time distribution (TTD). Mean ages do not simply reflect the time since a water parcel last contacted the atmosphere (tracer age), when analyzing the tracer concentration. Instead, it accounts for the distribution and mixing of water masses. The tail of the TTD represents older water, so the mean age can exceed the tracer's atmospheric lifetime, especially when tracer concentrations are low. As noted by Guo et al. (2025) as well, 'for water with an ideal age under 200 years, the CFC-12-based IG-TTD can provide meaningful mean ages up to this limit, despite the tracer's shorter ~80-year atmospheric history.'

In the revised manuscript we added clarity on the sampling of both tracers simultaneously (see Section 3.2) and added an explanation why the mean ages might exceed the tracer age (including also the reference by Guo et al., 2025) (see Section 4.1).

The deep waters at $\sigma=27.26 \text{ kg/m}^3$ are separated from surface waters by the cold intermediate layer, so young waters from the St. Lawrence River are unlikely to influence them. The lower mean age values towards the St. Lawrence Estuary are instead due to a higher fraction of LCW. As our analysis focuses on data east of the Lower St. Lawrence Estuary, before any upwelling of deep water towards the surface occurs, influence of surface St. Lawrence River seems unlikely at these depths (As also stated by Jutras et al. (2020), and outlined in the previous comments). Given the general circulation pattern of deep water into the Gulf and surface water out, the observed mean age variability primarily reflects processes within the deep water of the Laurentian Channel.

Following a comment from M. Jutras, we also examined other isopycnals along the Laurentian Channel in the deep water and observed some limited vertical mixing with younger LCW from shallower deep-water regions. This is added to the discussion as a potential limited contributor to the high LCW fraction values presented.

In the revised manuscript, we added an explanation on the limited influence of the surface or intermediate waters via vertical mixing, but potential upper deep water affecting the isopycnal values, in response to the vertical structure of mean ages and LCW fractions in the deep water (see Figure 4 in the revised version).

Figure 6: I'm not sure what the purpose of showing the relative stability of the oxygen concentrations is here because oxygen concentrations can change due to respiration changes, which isn't part of your analysis here. If you use your TTDs to calculate the preformed oxygen, on the other hand, then that may be worth showing. This figure, on the other hand, does show a trend in the variables that support your interpretation of the relative proportion of NACW vs LCW changing.

Response:

We thank the reviewer for this comment. The respiration rate was analyzed in detail in Nesbitt et al. (2025) and had been considered in our analysis. However, following the second round of revisions, we have removed the proxy time series of oxygen at Cabot Strait. Within the text, however, we still mention the effect on oxygen caused by the ongoing compositional change in NACW contribution.

Additionally, we analyzed if the limited amount of oxygen data showed a statistically significant trend, which was not the case, in response to M. Jutras first revision.

Lines 327-330: Where was this shift previously reported to be occurring, specifically?

Response:

We thank the reviewer for this comment. Previous studies report this shift for the deep water of the Gulf of St. Lawrence, specifically at Cabot Strait and throughout the Laurentian Channel.

We added this information to the revised manuscript and moved the section on historical water mass analysis to the introduction, only referring to it in the updated Discussion now.
