Referee 1

Formal review

The manuscript is well written, scientifically sound and addresses relevant societal and scientific questions within the scope of Ocean Science. A nutrient budget analysis based on newly collected and previously published data sets is presented. Unlike previous results, which had suggested that vertical nutrient fluxes due to diapycnal mixing dominate the nutrient supply in the Lower St. Lawrence estuary, the authors conclusively show that fluvial advection of nutrient rich waters from the St. Lawrence River dominate the budget throughout most of the year. The results thus modify current understanding of relevant nutrient supply processes in the estuary.

The contribution is well structured and clear. The results fully support the authors interpretations and the description of experiments and calculations are sufficiently complete and precise to allow their reproduction. I also appreciated the open access of scripts and data. Below, I provide a few minor remarks with reference to lines in the manuscript that the authors may want to consider to improve the manuscript. Most of the remarks are related to the discussion of possible biogeochemical processes that could also impact the nitrate budget and the nutrient distributions shown. Throughout the contribution, the authors refer to nutrient "consumption" as the only biogeochemical process being relevant. Although not explicitly defined, it seems that this term refers to uptake of nitrate during primary production. On the one hand, I find the word "consumption" in this context rather unfavorable. In biological oceanography, consumption is widely used related to oxygen and describes the loss of oxygen due to respiration of organic matter, i.e. a chemical reaction. In the same context, nitrate consumption occurs in anoxic waters in the form of denitrification or ANAMOX where bacteria respire nitrogen nutrients instead of oxygen. However, these biogeochemical processes are very different from nutrient uptake during photosynthesis. Thus, I would suggest to replace "consumption" with "uptake" in most places of the manuscript. On the other hand, there is also a biogeochemical nitrate source term. During the degradation of organic matter, nitrification enriches inorganic nitrate concentrations in the water column. While box models suggest that this term in not dominating nitrate supply in the Lower St. Lawrence Estuary, it does seem to contribute between 10% and 20% to the nitrate budget (e.g. Jutras et al., 2020, Thibodeau et al., 2013) and should thus not be completely ignored when interpreting nutrient distributions and their seasonal variability. In my detailed remarks below, I am pointing to a few but not all passages which the authors may want to improve.

We thank Referee 1 for their feedback, and respond to their detailed remarks below. We have amended the manuscript by replacing the term "consumption" with uptake. As stated in the introduction and methods section, our manuscript focuses on the seasonality of the nutrient transported into the system via physical processes. Specifically, we wanted to draw attention to the large nutrient inputs from fluvial sources given the extensive summer literature that stipulates vertical mixing processes are a main source of nutrients into the surface layer. Besides, we are unaware of any published studies discussing the seasonality of nitrification and remineralization of organic matter. Most studies such as the one referenced by the referee (e.g., *Jutras et al.*, 2020) focus on the summer conditions.

Detailed remarks with reference to lines in the manuscript:

Line 31, "..., but the low nutrient consumption provided a better representation ...". I can understand this statement as far as nutrient uptake during primary productivity is concerned.

However, I wonder about the seasonality of biological nutrient sources due to processes such as organic matter remineralization with subsequent nitrification and nitrogen fixation.

As already stated in the introduction, our manuscript focuses on the physical transport of nutrients into the surface layer. Namely, we compare the fluvial nitrate inputs with those entering the box from vertical mixing processes. We now specifically state that our analysis ignores nutrient cycling in the system or exported nitrate (L60-62). This statement was initially in the methods section.

Lines 33-34, "upwelling" and "entrainment": I had difficulties understanding the two terms, here. To me, the term "upwelling" refers to vertical advection and involves vertical velocities (e.g. due to Ekman divergence). However, here, I think the authors associate "upwelling" to a vertical flux of nutrients due to diapycnal mixing that does not involve any vertical velocity. Furthermore, the term "entrainment" is unclear to me. How does it differ from mixing? Please clarify the processes that are referred to here.

We have replaced most instances of "tidal-upwelling" with "tidal-induced mixing" to reduce ambiguities with this term. We used "upwelling" to imply a vertical motion/transport of deep water towards the surface. At the head of the channel, this vertical motion is caused by the tides and the sharp sill, which in turn generates internal waves. We have modified lines 26-34 accordingly. Entrainment is caused by shear (velocity gradients) at interfaces, and mixes water masses (transfers material/water across the interfaces). We have now specified that these processes are associated with vertical mixing processes (L28-29).

Line 75: add a period after "saltier".

This typo has been amended.

Line 175, "The historical dissolved nitrate concentrations at Quebec City were digitized from published sources (Figure 4 of Hudon et al., 2017)." This is a bit unclear. I could not find any nitrate values in Fig. 4 of Hudon et al (2017). There, only the sum of nitrate and nitrite is shown. How were nitrate value derived from this graph? Were nitrite concentrations neglected here? Please clarify.

Indeed, our observed nitrite concentrations were very low (2-3 orders of magnitude lower than nitrate). We thus relied on nitrate concentrations for our turbulence analysis. We could have used instead the sum nitrate+nitrite, which would have yielded the same results for the fluxes. We have updated the text on L167-170 to stipulate that Figure 4 of Hudon et al. 2017 showed nitrate+nitrite, and that our measured nitrate concentrations are representative of the sum of nitrate+nitrite.

Line 192, functional dependence of N: The Greek symbol rho is not introduced and should be potential density (otherwise, compressibility needs to be account for in the equation).

We have modified this line by introducing ρ as potential density.

Line 213: add potential before density.

We have removed the mathematical variable, and added the word potential.

> *Line 222, "indicating that nitrate was being consumed": I would suggest to rephrase this sentence to include biological production of inorganic nitrate, e.g. "indicated that nitrate uptake exceeded biogeochemical nitrate sources" or "indicated a net nitrate loss through biogeochemical processes".*

We have rephrased this sentence with "biogeochemical processes caused a net loss of nitrate in the Upper Estuary" [L216-217].

> *Line 229, "Nitrate concentrations in winter ...": I think the statement made in this sentence also applies to nitrate distributions during the other seasons.*

Indeed, this statement may be applicable to other seasons. This statement, however, introduces the topic of the paragraph i.e., to describe the water masses during winter.

> *Line 291, "higher consumption": I think that the authors solely refer to nitrate uptake during phytoplankton growth here. However, there are also other nitrate sinks such as denitrification. What may be the seasonal variability of these processes? Furthermore, I would suggest to use "biological uptake" instead of consumption.*

We have changed the word consumption to "biological uptake", but have not discussed other losses such as denitrification since the manuscript targets the physical inputs of nutrients into the Lower Estuary.

> *Line 306, "Hence, the minimum nitrate load required to ...": This statement is incorrect, as it neglects local nitrate sources e.g. due to aerobic remineralization of organic material (nitrification).*

This statement is correct as it stipulates that local processes and export (e.g., advection out of the region) are ignored, and so the amount stipulated is the minimum load required. To remove ambiguities, we now state that the nitrate in the upper 75-m increased by 195 mol/s instead of stating this layer requires a minimum load of 195 mol/s [L302].

> *Lines 326-327, "... period when biological consumption is greatest": See comments to line 291 above.*

As requested above, this was changed to "biological uptake".

> *Line 373-408, discussion section: I think that adding a few sentences on the relative importance of biogeochemical flux contributions to the nitrate budget would strengthen this work even further.*

We have not incorporated this suggestion other than add some new references on global distribution of nitrate fluxes (e.g. *Mouriño-Carballido et al.*, 2021). An earlier draft included a brief overview of the budget, which we removed because of the lack of winter observations for the other processes (remineralisation, sedimentation, etc). Published biogeochemical models for summer yield vastly different estimates for the nitrate inputs via vertical mixing processes than those we observed. These models often use much higher inputs for fluvial nitrate loads. Thus, we preferred discussing only deviations in the nitrate inputs into the Lower Estuary from physical processes among studies rather than commenting on the other nitrate sinks/sources within the system.

> *Figure 3 caption: Add "winter" between the and field in the first line. In the text before referencing to Figure 3 for the first time you were mentioning historic data and it is thus somewhat unclear which data are shown.*

We have added the word "winter" as requested.

References

Cyr, F., D. Bourgault, P. S. Galbraith, and M. Gosselin (2015), Turbulent nitrate fluxes in the Lower St. Lawrence Estuary, Canada, *J. Geophys. Res. Oceans*, *120*(3), 2308–2330, doi:10.1002/2014JC010272.

Jutras, M., A. Mucci, B. Sundby, Y. Gratton, and S. Katsev (2020), Nutrient cycling in the lower st. lawrence estuary: Response to environmental perturbations, *Estuarine Coastal Shelf Sci.*, *239*, 106,715, doi:https://doi.org/10.1016/j.ecss.2020.106715.

Mouriño-Carballido, B., et al. (2021), Magnitude of nitrate turbulent diffusion in contrasting marine environments, *Sci. Rep.*, *11*(1), 18,804, doi:10.1038/s41598-021-97731-4.

Savenkoff, C., A. F. Vézina, P. C. Smith, and G. Han (2001), Summer transports of nutrients in the Gulf of St. Lawrence estimated by inverse modeling, *Estuarine, Coastal and Shelf Science*, *52*, 565–587, doi:10.1006/ecss.2001.0774.

Sinclair, M., M. El-Sabh, and J.-R. Brindle (1976), Seaward nutrient transport in the lower St. Lawrence Estuary, *Journal of the Fisheries Research Board of Canada*, *33*, 1271–1277, doi:10.1139/f76-163.

Villeneuve, V. (2020), CaractÃ©risation des variations saisonniÃ¨re et spatiale des Ã©lÃ©ments nutritifs et de la prise de l'azote dissous dans lâestuaire du fleuve saint-laurent, MSc thesis, UniversitÃ© Laval.