Paper title: Influence of a small submarine canyon on biogenic matter export flux in the Lower St. Lawrence Estuary, eastern Canada

Subject Editor: Kyung-Hoon Shin (Referee #1)

General comments: The paper investigates the impact of a submarine canyon system on the vertical export of biogenic matter in the Lower St. Lawrence Estuary, finding that sediment remobilization events and turbidity currents in the canyon influenced the flux of particulate matter and chloropigments in the water column. The study also reveals differences in diatom and dinoflagellate assemblages between the canyon system and an offshore site, with a diatom bloom occurring earlier at the canyon site. The presence of a potentially toxic diatom species was observed at the offshore site. I think this paper is a useful contribution to the literature and worthy to be published in Biogeosciences. However, I have some comments mainly about the clarity of the manuscript. I recommend the journal to accept this work after a moderate revision.

We thank the reviewer for their positive comments and for contributing to improving the manuscript. Your comments have been considered in the revised version. Please find below in blue a point-by-point response to all your comments.

Specific comments:

1. Line 44: (Normandeau et al., 2014, 2020, 2022) à (Normandeau et al., 2015, 2020, 2022)

We have added both 2014 and 2015 to the references, thank you.

2. Line 329: Calvin pathway Meyers, 1994; Macdonal et al., 2004) à Calvin pathway (Meyers, 1994; Macdonal et al., 2004)

We corrected the reference, thank you.

3. Line 347: Are there any other primary production proxies mentioned here besides chloropigments?

In our study, primary production proxies at the Pointe-des-Monts site include chloropigments, diatoms, and dinoflagellates. To avoid confusion, we have clarified this sentence in the revised version of the manuscript, which now reads "[…], but we note no correlation to other primary production proxies (i.e., fluxes of chloropigments, diatoms and dinoflagellates)."

4. Table B2: symbol check! in table B2

We have changed the * symbol to [].

Major comments:

1. What are the conditions for determining trap depth?

Trap depths were determined based on several considerations:

1) All traps were located within the deep layer.
2) The sediment traps PDM-224 and BC-133 were located 58 and 67 m above seafloor, respectively, which is well above the benthic nepheloid layer (i.e., layer of water above the seafloor that contains significant amounts of suspended sediment), which is estimated to be about 10 m above the seafloor in our study area (Bourgault et al., 2014; Casse et al., 2019).
3) In the main canyon system, the ADCP was located 27 m above the seafloor (156 m water depth) to be consistent with to methodology used in Normandeau et al. (2020). The PDM-154 sediment trap was placed at a similar water depth to the ADCP.
4) BC-133 was located at a comparable distance above seafloor as PDM-224 while also being at a comparable depth below the surface as PDM-154.

To clarify this point, in the revised version of the manuscript we have added:

Lines 120-121: "Traps BC-133 and PDM-224 were positioned 67 m and 58 m above seafloor, respectively, above the estimated benthic nepheloid layer in the LSLE (i.e., 250-310 m of water depth; Bourgault et al., 2014; Casse et al., 2019)."

Bourgault, D., Morsilli, M., Richards, C., Neumeier, U., and Kelley, D. E.: Sediment resuspension and nepheloid layers induced by long internal solitary waves shoaling orthogonally on uniform slopes, Continental Shelf Research, 72, 21-33. http://dx.doi.org/10.1016/j.csr.2013.10.019, 2014.

Casse, M., Montero-Serrano, J.-C., St-Onge, G., and Poirier, A.: REE distribution and Nd isotope composition of estuarine waters and bulk sediment leachates tracing lithogenic inputs in eastern Canada, Marine Chemistry, 211, 117-130. https://doi.org/10.1016/j.marchem.2019.03.012, 2019.

2. What is the reason for the decrease in water temperature and salinity in December at both sites?

The sediment traps BC-133 and PDM-154 are within the deep layer and temperature and salinity fluctuations are not directly associated with surface conditions. We have added a few sentences to discuss the possible reasons for changes in water temperature and salinity in the revised version of the manuscript (lines 203-208), which reads:

"We note that since the sediment traps BC-133 and PDM-154 were moored within the deep layer, the recorded fluctuations in temperature and salinity shown in Figs. 4-5 may be due to: 1) a change in the contribution from the Labrador Current (lower temperature and salinity) and North Atlantic waters (higher temperature and salinity) or, 2) vertical mixing of the cold intermediate layer (lower temperature and salinity) with the deep layer (higher temperature and salinity). However, the data that we collected as part of this study are not sufficient to determine the cause for the changes in the properties of the water masses."

3. Are the large waves that occur every 2-3 years related to global ocean-atmosphere climate phenomena?

Normandeau et al. (2020) reported that the large turbidity currents that occur every 2-3 years were initiated during storms that exhibited sustained (>7 h) high windspeeds (>60 km h^{-1}), which caused large storm waves. At or near low tide, these storm waves subsequently triggered a turbidity current (lines 312-313). In the context of global climate warming, it can be expected that a shortening of the sea ice season and increased storm frequency may favor the development of sediment remobilization events (lines 398-399).

4. What is the time lag for organic matter produced in the surface layer to reach the depth of the sediment trap? Can I consider that the sinking particles obtained in each month fell during that same month?

We thank the reviewer for raising this important point and have added in the revised version of the manuscript a brief explanation at the beginning of section 5.3 to address this:

"The sinking rate of biogenic matter is highly variable depending on shape, size, density, aggregation, remineralization, etc. For example, sinking rates of individual diatoms range from 0.1 to 10 m d^{-1} (Smayda, 1970) while aggregates can sink at rates of 88 to 569 m d^{-1} (Iversen et al., 2010). Sinking biogenic matter generally form aggregates ("marine snow") to facilitate vertical fluxes of pelagic particles, therefore, we can assume that the sinking particles obtained in the sediment traps are representative of their respective sampling time periods."

Iversen, M. H., Norwald, N., Ploug, H., Jackson, G. A., and Fischer, G.: High resolution profiles of vertical particulate organic matter export off Cape Blanc, Mauritania: Degradation processes and ballasting effects, Deep-Sea Research I, 57, 771–784, doi:10.1016/j.dsr.2010.03.007, 2010.

Smayda, T.: The suspension and sinking of phytoplankton in the sea, Oceanography Marine Bulletin, 8, 353–414, 1970.

5. Why does diatom bloom occur first, followed by the dinoflagellate later?

Diatoms are the most abundant phytoplankton in the Lower St. Lawrence Estuary and almost all diatom taxa are autotrophic, whereas dinoflagellates are the second most abundant phytoplankton and approximately half the dinoflagellate taxa are heterotrophic (micrograzers). Diatoms bloom first due to higher growth rates, lower light requirements (particularly for pennate diatoms which bloom before centric diatoms), and higher nutrient requirements, particularly nitrate and dissolved silicon. The diatom bloom will end when the surface mixed layer becomes limited in nutrients and dinoflagellates will then bloom because the heterotrophic taxa are able to feed on diatoms and other primary producers. We very briefly mention this pattern in the second paragraph of section 5.3. This has been well established in the region therefore we do not believe it is necessary to explain in depth in our manuscript.

6. Lines 386-390: Isn't the depth of the mixed layer greater during the fall and winter months rather than the impact of resuspension? What do you think/ or other possibility?

We thank the reviewer for raising this point. The mixed layer deepens from fall into winter and then becomes shallower again in the spring. In lines 386-390, our intention was not to address the question of why primary production is lower in the winter, but rather to evaluate if resuspension events could affect the surface mixed layer when primary production would be more impacted. We have rephrased this in the revised manuscript (lines 423-427) to increase clarity:

"However, our data do not permit to evaluate if lofted sediments also reached the surface layer where they would have decreased primary production. Additionally, turbidity currents and other sediment remobilization events appear to be more frequent from late fall through winter, therefore we cannot determine if sediment lofting would play a key role in this system throughout the entire year, particularly from late spring through early fall, when primary production is greatest."

7. Why do they exist only as cysts for *Brigantedinium* spp. and *Selenopemphix quanta*?

Only about 20% of total dinoflagellate taxa are known to produce organic resting cysts. Of the 16 motile dinoflagellate taxa observed in our study, four are currently known to produce resting cysts: *Pentapharsodinium dalei*, *Protoperidinium americanum*, *Protoperidinium conicum*, and *Protoperidinium* spp. In the present study, we observed cysts of the last three taxa but did not observe cysts of *P. dalei*, which we know are abundant in the regional surface sediments. We do not believe that preservation is an issue because other sediment trap studies using formalin as a preservative have observed cysts of *P. dalei* (e.g., Heikkilä et al., 2016). It is possible that we did not observe cysts of *P. dalei* because they were outnumbered by other dominant taxa (below detection).

Heikkilä, M., Pospelova, V., Forest, A., Stern, G. A., Fortier, L., and Macdonald, R. W.: Dinoflagellate cyst production over an annual cycle in seasonally ice-covered Hudson Bay, Marine Micropaleontology, 125, 1-24, http://dx.doi.org/10.1016/j.marmicro.2016.02.005, 2016.

Subject Editor: Anonymous (Referee #2)

General comments: Sharpe et al. explored the vertical transport of organic matter in the St. Lawrence Estuary under the influence of a small near-bed submarine canyon system. Submarine canyons are established as important conduits for sediment accumulation through features like episodic turbidity currents, and thus play a key role in material exchange between upper and deep ocean. The authors presented a 1-year continuous record of export fluxes close to the small canyon feature, comparing observations to a distal reference station outside the influence of canyon processes. Results showed that the organic particle fluxes were anomalously greater at the canyon station following sediment remobilization events, providing strong evidence that the canyon system impact existed. In addition, primary productivity appeared to be suppressed by the small canyon processes likely due to sediment lofting causing declined light availability. The authors also speculated that the declining sea ice cover and prolonged ice-free season could cause increased sediment remobilization events but uncertain impact on the ecosystems, pointing to the necessity for more frequent observations. The topic of this study is interesting because the impact of smaller submarine canyon systems on the water column biogeochemistry has not been well documented, and I believe this study would help address this knowledge gap and potentially be of interest to the readers of Biogeosciences. I also think the experiment is nicely designed and the manuscript well written in general. I only have a few minor comments, and I would like to recommend this manuscript for publication if those minor technical comments were properly addressed by the authors.

We thank the reviewer for their positive comments and for contributing to improving the manuscript. Your comments have been considered in the revised version. Please find below in blue a point-by-point response to all your comments.

Specific comments:

1. Line 56: the Baie-Comeau station. If BC-133 is not affected by any sediment remobilization events whatsoever as the "control" of this study, I think the authors should state it somewhere at the beginning.

We have clarified this in the revised version of the manuscript (lines 65-67), which now reads "To identify canyon-specific processes, a sediment trap was also deployed offshore Baie-Comeau (BC), a site not affected by canyon-related sediment remobilization events, to contrast with biogenic matter export in the LSLE (Fig. 1c)."

2. Line 176: add a "." after "(Fig. 3)".

We corrected the sentence, thank you.

3. Line 203: delete "slightly" or replace "slightly" with "evidently". It is problematic to say PDM-224 is only "slightly" higher than PDM-154, at least not in terms of chloropigments in Fig. 5.

We have removed "slightly" so the sentence now reads "Overall, fluxes of chloropigments exhibit similar patterns at both depths, with higher flux values at PDM-224."

4. Line 289-290: replace "are likely occurring" with "likely occur".

We corrected the sentence, thank you.

5. Line 383: "limiting light availability": what makes it different in larger submarine canyon systems from smaller ones in terms of phytoplankton growth-limiting factors (such as conditions with light and nutrient)? I.e., Why do turbidity currents enhance productivity in large canyon systems despite reduced light availability?

We thank the reviewer for raising this point. In addition to the size of the canyon system, there are other factors that need to be considered.

Large submarine canyon systems are not located within estuaries, which are typically rich in nutrients. In the Lower St. Lawrence Estuary (LSLE), upwelling of nutrient-rich waters at the head of the Laurentian Channel, subsequent mixing in the surface layer, and strong estuarine circulation provide an important supply and distribution of nutrients year-round. Freshwater inputs from the Saguenay, Outardes, Manicouagan, and St. Lawrence rivers additionally introduce nutrients to the system. Light is therefore the most important variable controlling phytoplankton growth in the LSLE (Therriault & Levasseur, 1985).

Globally, there are many triggers for turbidity currents, which are almost always triggered in canyon systems with important sediment supply from rivers of longshore drift; thus the "sediment-starved" canyon system at Pointe-des-Monts is an exception (Normandeau et al., 2017 and references therein). The sediment supply and trigger influence the frequency, seasonality, and amplitude of turbidity currents, which will ultimately determine the impact of these events on regional productivity.

The results presented here do not allow to determine the different impacts of small and large submarine canyon systems, but rather provide insight into a singular annual cycle for a small submarine canyon system in the LSLE.

To clarify this, lines 415-421 of the revised version of the manuscript now read:

"However, phytoplankton fluxes do not provide evidence that this applies at PDM, a relatively small and shallow submarine canyon system located within a nutrient-rich estuarine system. Instead, annual fluxes of diatoms and dinoflagellates were lower at PDM-154 compared to BC-133. At the PDM site, in addition to the absence of direct riverine input and differences in the structure of the water column, increased sediment input from the coast and seafloor remobilization by canyon processes may have hindered primary production by limiting light availability, which is the most important variable controlling phytoplankton growth in the LSLE (Therriault & Levasseur, 1985)."

6. Line 406: Section 5.5. This section could have been written in a more organized way. It would be less confusing for the readers if the variables in comparison and the corresponding numbers were presented in a dedicated table. For example, Line 413-414, what are "much greater", and what are the two numbers respective to?

We have included the annual fluxes of chloropigments and particulate organic carbon measured in the present study to increase clarity and highlight that they are much greater than those measured in Genin et al. (2021), as well as included the trap and water-column depths. Lines 455 to 458 of the revised manuscript now read: "Annual chloropigment (70 to 120 mg m^{-2} yr^{-1}) and POC (11 to 19 g m^{-2} yr^{-1}) fluxes measured here were much greater than those measured near Cabot Strait (35 mg m^{-2} yr^{-1} and 1.1 g m^{-2} yr^{-1}, respectively; 100 m trap depth, 461 m water-column depth; Genin et al., 2021)". We would like to avoid including a table with the values, as this section is not a focus of the study, but rather a brief discussion of previous sediment trap studies to provide a regional context to our values.

Ji-Hyung Park
Associate Editor, Biogeosciences

Both reviewers recognized the novelty of your work and agreed that the manuscript should be conditionally accepted. Therefore, I recommend conditional acceptance with the assumption that some moderate revision will be needed to bring the manuscript up to an acceptable level and clarity. When you revise the manuscript to incorporate the comments and suggestions offered by the two reviewers, please also take into consideration the following editorial points:

We thank the Associate Editor for their positive comments and for contributing to improving the manuscript. Your comments have been addressed in the revised version. Please find below in blue a point-by-point response to all your comments.

Some of your responses to the reviewer comments are not clear enough as to whether you would incorporate the addressed issues in the revised manuscript. For instance, do you mean in your response to the second major comment by the first reviewer that you would discuss these two potential causes in the manuscript? Please articulate in your responses (by showing the revised text or indicating the line numbers) how you would incorporate the addressed issues in the revised manuscript.

We have updated the responses for the first reviewer's second and third major comments and the second reviewer's fifth comment (all updates highlighted in yellow).

1. Line 22: Is this 'the distal PDM site'? Please use consistent naming for the two PDM sites throughout the manuscript.

We added the mooring IDs to the abstract to increase clarity in the revised version of the manuscript (lines 16, 18, 19, 22).

2. Line 25 "biogenic matter export": The preceding descriptions on the monitored phytoplankton data (lines 21-24) need to explain how the canyon system and associated local bloom patterns influence this matter export (vertical or longitudinal), but it is difficult to read out the key message from the current sentences. As you indicated in Discussion (lines 376-377), if the original hypothesis ("canyon-related hydrodynamics would stimulate phytoplankton production at the PDM site") is not valid, I wondered what alternative mechanism you wanted to suggest here. Please make your point clearer here in the abstract and also in the Conclusions (lines 425-428).

We thank the Associate Editor their comment. The abstract has been updated to increase clarity, with lines 20-28 of the revised version of the manuscript now reading:

"Concurrent elevated fluxes of total particulate matter, particulate organic carbon, particulate nitrogen, and chloropigments showed that these events left a signature in sediment traps PDM-154 and PDM-224 located >2.6 km further offshore, by enhancing lateral dispersion of resuspended sediments. The composition of diatom and dinoflagellate assemblages was similar in the canyon system and offshore BC, but the diatom bloom occurred two weeks earlier (in mid-April) at the PDM site. A bloom of the potentially toxic diatom *Pseudo-nitzschia seriata* was also observed during the second half of September 2021 at the BC site. Annual diatom and dinoflagellate fluxes were almost 2 times lower at the PDM site than at the BC site, possibly due to differences in riverine input and the structure of the water column, as well as increased sediment input and resuspension at the PDM site, leading to limited light availability."

The conclusion has also been updated, with lines 470-477 of the revised version of the manuscript now reading:

"However, our data show that annual phytoplankton fluxes were lower in the canyon compared with background LSLE values as recorded at Baie-Comeau. This may be attributed to differences in riverine input and the structure of the water column, in addition to increased sediment input and resuspension at the PDM site, collectively contributing to limited light availability, the most important variable controlling primary production in the LSLE."

3. Line 26: The 'regional ecosystem' is a vague term. It would be more appropriate if you specify the impact area that the three monitored sites could cover.

We have updated the sentence to read "[…] thereby directly influencing the ecosystem offshore PDM."

4. Section 3.2: Given the usual range of estuarine POC concentrations (no concentration data provided in the manuscript), a 1-3 mL sample volume seems too small to ensure an accurate analysis of POC in the filtrate. It would be helpful if you provided more analytical details, including analytical limits of the used method and instruments and any employed QC measures.

In this study, POC and PN were not measured in discrete water column samples but in the cups of sediment traps where particulate organic matter is highly concentrated. The cup samples were filtered on precombusted Whatman GF/F filters. Filters were then dried, acidified, and pelletized in thin capsules. POC and PN weights were quantified with a DeltaPlus XP Continuous-flow Isotope Ratio Mass Spectrometer. Analytical detection limits were 0.1 mg C for POC and 0.02 mg N for PN.

In the revised manuscript, we wrote: "For particulate organic carbon (POC) and particulate nitrogen (PN) determination, the filters were pelletized in tin capsules, and combustion was performed with an Elementar PYRO Elemental Analyzer, and detection with a ThermoScientific DeltaPlus XP Continuous-flow Isotope Ratio Mass Spectrometer (ISMER-UQAR). Acetanilide (B2000, Elemental Microanalysis) was used as a calibration standard for the analysis. Instrument blanks (empty tin capsules) were performed during calibration to stabilize and establish a baseline for the instrument. Analytical detection limits were 0.1 mg C and 0.02 mg N for POC and PN, respectively." (lines 151-157).

5. Lines 315-320: Why don't you compare the POC:PN ratios of sediments with those of the collected phytoplankton biomass?

The POC:PN ratios recorded here are not compared to the fluxes of diatoms and dinoflagellates because the POC and PN encompass much more, such as other plankton, fecal pellets, and resuspended matter. If we were to study sediment traps with POC and PN more reflective of phytoplankton export (e.g., higher in the water-column, in a region not influenced by submarine canyon activity) it would be more beneficial to compare POC:PN ratios with collected phytoplankton biomass.