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Interactive comment on “Benthic fluxes of dissolved organic nitrogen in the Lower St. Lawrence Estuary and implications for selective organic matter degradation” by M. Alkhatib et al.

Anonymous Referee #1

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The title of this manuscript sounds promising, but I was disappointed after finished reading it. Only two parameters, porewater DOC and DON, were measured in sediments from 9 locations along the St. Lawrence Estuary. Based on these profiles, the diffusive fluxes were estimated and gave some interesting correlations with POM reactivity and oxygen exposure time. The origin of the latter information is not clear, but it is certainly not from this study (most likely from the authors own cited papers). Particularly the origin of the “Chlorin-” and “amino acid-“based degradation indices seems obscure. It appears to me that the DOC and DON porewater data are leftovers from a larger project that did not fit into the other papers shown in the reference list (Alkhatib et al., 2012a,b) – but the authors still want to publish them. In my mind, it makes an

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incomplete report and requires so much data to be delivered from the published results that the present study hardly can be considered original. If the authors want to write a review of their findings in the area, then it must be stated clearly – and not be masked within a primary research paper.

Since only DOC and DON are measured in this study, it is a pity that no Rhizon data on DOC are reported. No reason is given for that – they are just missing.

The English language can be improved. The writing style is rather complex with long and often hard to understand sentences. The authors should be aware that they can easier get their message through to readers using a relatively simple language. By doing so, the readers are only required to focus on the scientific part.

I also find the paper excessively long. Particularly the discussion seems to be forced in length by unnecessary and speculative statements. It is also somewhat excessive using estimated DON fluxes from 9 locations in the St. Lawrence Estuary to extrapolate a total budget for continental shelves globally.

Besides these major points, I have addressed several other concerns in the list below:

Page 7918, lines 24-26: This sentence is poorly written and is almost impossibly to understand. Page 7919, line 1: What does “sedimentary archives” mean? Is it from the literature? Page 7919, line 10-11: I disagree that estuarine sediments always exhibit uptake of dissolved inorganic N. In fact, most studies have shown a release of DIN from sediments. Page 7919, lines 17-23: This sentence is very long and difficult to understand. Page 7920, line 2: Not all forms of DON can be used by microorganisms for biosynthesis. Page 7920, lines 7-8: First, delete “bottom” because it is where sediments always are. Second, does “. . .remineralized N as DON.” mean that DON remineralized to DIN is re-assimilated and released as DON. This is true and not surprising as the gross mineralization in sediments usually is much larger than the measured net mineralization measured from DIN fluxes. Page 7920, line 15: What does “. . .bioavailability of DOC from sediments with respect to DON. . .” mean. It is

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hard to understand. Page 7922, lines 2-3: Oxygen loss is not only through aerobic microbial respiration. Chemical oxidation processes driven by anaerobic respiration can in certain areas contribute as much or even more than aerobic respiration. Page 7922, lines 9-10: Where do these degradation indices come from? They are shown in Table 1, but there is no indication of the origin of the data. It is only referred that the indices are developed by Schubert et al. (2005) and Dauwe et al. (1999). These two authors have not presented the St. Lawrence data. Page 7922, line 12-14: How can we tell from Table 1 that the terrestrial content of OM decreases with distance from the head? No data is provided!! Page 7922, line 16: Change “multiple” to “eight” Page 7923, line 9: Wouldn't it be better using “DIN” here instead of “nutrient”. Then any misunderstandings involving P can be avoided. Page 7924, lines 16-24: How reliable are these flux estimates? Precise diffusive flux calculations can only be obtained from high resolution profiles (μm -scale). Furthermore, it is not possible from the current profiles to observe if any consumption or production of DOC and DON occurred near the sediment-water interface. There is often a dramatic shift in reactions when moving from the anoxic to the oxic surface layer. Some considerations justifying the approach are required. It is not enough stating that others have done the same. Page 7925, lines 16-17: Which sampling approach provided these results? Page 7926, lines 4-8: There is no explanation for the missing Rhizon DOC data. There must have been samples available since DON was analyzed. What have happened to those data? Page 7926, lines 9-11: How was the DOC/DON ratio calculated? As average of samples from the upper cm or was it as a plot of DOC versus DON results from the upper cm. The latter provides good evidence for the production ratio because it automatically excludes the overlying water concentration. The authors should consider the best way to do this. Page 7926, line 15: Change to “Rhizon-based DON fluxes” Page 7927, lines 9-10: Bottom water DOC/DON ratios only increased from station 25 to station 20 and then decreased to station 16. Page 7928, lines 1-7: These lines repeat what has been said before and could be omitted. Page 7928, lines 8-29 & page 7929, lines 1-9: The authors spend too much space here on sampling artifacts. This research was not



designed for testing the sampling technique. If so, much more focus should have been on this from the start, and the approach should also have been different. The missing Rhizon DOC results are this respect puzzling. Did DOC not show the expected results or are the results just missing. The statement on page 7929, line 8-9 adds to the mystery. How can the authors conclude that Rhizon DOC results are best below 2 cm when there are no results? Page 7930, line 5-9: The data used to make these regressions shown in figures 6 and 7 (sediment particulate C/N ratios, degradation indices, dissolved oxygen concentration and oxygen exposure time) are not available from this study. Where are they from then? And why are they not made for DOC? Page 7931, lines 27-28 and page 7932, lines 1-2: This statement is unjustified and only relies on speculation. Should be omitted. Page 7932, lines 13-16: This statement tells nothing. Which “DOC-mineral interactions” played a role here? Page 7932, line 28: Only CI is shown in figure 7!! Page 7933, lines 3-19: These lines are speculative and not supported by the data. Should be omitted. Page 7934, line 5: Where are the DIN data shown? Page 7934, lines 17-18: We have been told earlier how the present fluxes compare with other studies. Please omit. Page 7934, line 19-27 and page 7935, line 1-2: This extrapolation to global continental shelves is too excessive. It is based on the unjustified assumption that the St. Lawrence Estuary is representative for all shelves, which is most unlikely. More data from different shelves and regions are required to do such extrapolation. Table 3: Such table providing a literature review of fluxes is not relevant here. The needed comparisons are given in the text. Please omit. Figure 3: The sampling technique used is not mentioned in the legend.

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