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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.

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Received and published: 28 June 2013

This is an interesting study examining DON and DOC fluxes from estuarine sediments. Not a lot has been published on this topic in recent years, so these results are a welcome addition. These results also illustrate some interesting and intriguing contrasts between DOC and DON fluxes. Overall, the results seem robust and worthy of publication. There are a few sections that could use a little tightening up (indicated below), but overall the manuscript is well-written. With modification I recommend publication in Biogeosciences.

1. (7918, 9) - DON fluxes varied inversely with OET not positively (in Fig. 6, OET goes

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up and DON fluxes go down).

2. (7920, 22-24) – This sentence is phrased in a somewhat confusion manner. I would argue that refractive OM by definition has a “lower potential for remineralization . . .” as opposed to suggesting that because it is refractory it has “a lower potential . . .”. Maybe I’m getting caught up in semantics but this seems redundant as written.

3. Section 2.1 – I assume (hope) these sediments are not bioturbated and/or bioirrigated? If they are bioturbated and/or bioirrigated, that’s a game-changer in terms of any interpretation of diffusive fluxes as is done here. Please state this in this section.

4. (7922, 12-14) – What parameter(s) were used to determine the terrestrial content of the organic matter? This is not clear from Table 1.

5. Some of the discussion in section 4.1 could be cut back and/or better focused.

6. (21,7928) – How high is the pressure exerted by the WCS relative to the original in situ pressure the cores experiences before being brought to the surface? Does this have an impact on the discussion here? Also does high pressure really favor DOM desorption?

7. (7928, 29) – Is there data in the Lalonde et al. (2012) paper on concentrations of nanophase ferrihydrite – I don’t recall seeing it, but maybe I missed it (I have a similar concern about line 10, p. 7932).

8. (7930, 8) – Rather than say DON fluxes correlated linearly with OET, tell us there was an inverse relationship. The inverse relationship is far more important, interesting and relevant to the discussion here than simply knowing that DON flux and OET correlate with one another.

9. Another possible explanation for the links between DON fluxes and redox controls involves possible redox controls on DON (or DOC) consumption, regardless of (or in addition to) any redox controls on DOC production during the initial stages of POM degradation. I talk about this in my 2001 Org. Geochem. Paper as well as my 2002

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review article in the Hansell and Carlson DOM book. This might be worth considering here.

10. (3, 7932) – After reading this paragraph I wondered why surface reactions might only affect DOC molecules and not DON molecules. Also, if DOC molecules do preferentially stick to sediment particles is this consistent with the close tracking of the different elemental ratios shown in Fig. 5?

11. section 4.3 – If the explanations here involves differences in the reactivity of N-rich (versus N-poor) POM or DOM, it may be better to frame the discussion in terms of comparisons of N/C (rather than C/N) ratios (see, for example, Perdue and Koprivnjak. 2007. Est. Coastal Shelf Sci., 73: 65-72). I also wonder if revising this discussion in this way helps to better explain some of the uncoupling in the behavior of DOC and DON benthic fluxes from St. Lawrence estuarine sediments (which in my mind, is a very interesting aspect of these results).

12. (3, 7933) – Again this may be an issue of semantics, but the discussion here gets a little muddled with regards to terminology. At least from my perspective, the “initial depolymerization” of sediment POM can occur in either two ways: hydrolysis or oxidative cleavage. Given this perspective, some of the text here needs to be cleaned up.

13. (5, 7936) – I assume you mean here water column inorganic N pools? If so, you might be specific.

14. In Table 1 is the Corg flux the carbon rain rate to the sediment surface or the remineralization flux (i.e., the depth-integrated rate of organic carbon remineralization)?

David Burdige

Interactive comment on Biogeosciences Discuss., 10, 7917, 2013.

BGD

10, C3097–C3099, 2013

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