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Biogeosciences Discussions

Interactive comment on "Seasonal dynamics of carbon recycling in coastal sediments influenced by rivers: assessing the impact of flood inputs in the Rhône River prodelta" *by* C. Cathalot et al.

Anonymous Referee #2

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This manuscript presents a topical report on the oxygen uptake of the sediments in front of the Rhone river delta. The dataset consists primarily of O2 micro-electrode profiles (DOU), obtained both in situ and ex situ, as well as data on the total O2 uptake (TOU) obtained ex situ in whole core incubations. This O2 uptake dataset is further supplemented by porosity, Chl a and organic carbon content data.

There is nothing really wrong with this manuscript. The methods are state-of-the-art in terms of O2 uptake and seem to be well executed (only the application of the Eddy Correlation technique would have been a valuable complement to the current dataset, providing in situ TOU values, but the application of the Eddy technique cannot yet

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be considered routine). The resulting DOU and DOU dataset is carefully reported (including detailed info on porosity profiles, bottom water temp en O2 concentration, and sediment parameters). As a result this dataset will provided a valuable contribution to the existing global dataset on in situ DOU values.

Still, I can't say that I am terribly excited after having read the manuscript. There is little novelty or creativity in the way the data is interpreted. Some obvious questions concerning organic matter processing in deltas are not touched upon in the discussion. Moreover, the questions that are addressed in the discussion are treated in a rather "light" way, and so overall, the manuscript attains the character of a valuable data report.

Given the high quality of the dataset, I think publication in BG could be possible, but then the discussion and framing of the results should be improved.

Major comments

The text should be carefully screened and improved, because throughout, the manuscript contains improper grammatical constructions and incorrect expressions (a few are indicated below in the detailed comments).

The first sentence of the abstract reads: "the biogeochemical fate of the particulate organic inputs from the Rhone river was studied". Equally, the last sentence of the conclusion reads: "coastal sediments off the Rhone act as . . . a degradation centre for flood deposits". I'm not convinced this is truly what is this study shows. The question of how much of the mineralization (or the O2 consumption) is due to the input of riverine material on the one hand, and due to local primary production on the other hand, is left unanswered. More importantly, the question is not even posed. So: is the high Chl a and the high org C content in the river mouth stations due to riverine input or locally produced? To me the high Chla would indicate local production (stimulated by riverine nutrient input), but then one would expect a strong seasonal signal in the DOU and TOU (because of seasonality in production). Such seasonality does not appear to be

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there. This important issue should be further sorted out in the discussion.

There are two major conclusions regarding the O2 consumption in the Rhone prodelta sediments: (1) The absence of seasonality (2) The fact the O2 consumption decreases after a flood event and then goes back up to the pre-flood value The discussion of both topics is presently rather shallow, and could certainly be deepened and improved.

Absence of seasonality. - Is the present temporal resolution in sampling enough to determine that there is no seasonality? - What about primary production in the Rhone river mouth? (both yearly averages as well as seasonality). Primary production is not included in the discussion at all. There must be data on this. . . - There is remarkable little seasonal variation in the temperature and bottom water O2 (see table 1). Why is this? - There are a few previous studies that have investigated seasonality in DOU and TOU. Glud et al (2003) made a detailed study of seasonality in Aarhus Bay. One should compare to these studies in the discussion.

O2 consumption decrease after a flood event. The O2 consumption is (implicitly) used as a direct one-to-one proxy for mineralization rate. This assumes that sedimentary mineralization (CO2 production) always scales with O2 consumption. However, the question is whether this critical assumption holds in a transient situation as right after a flood deposit. There are (at least) two possible explanations for the O2 consumption decrease after a flood event: (1) It takes time to re-establish the microbial community that performs the mineralization. This would temporarily lead to a decreased mineralization (i.e. CO2 production) as well as decreased O2 consumption right after the flood. (2) It takes time to re-establish the gradients of reduced products from suboxic/anoxic diagenesis. This could temporarily decrease the O2 consumption right after the flood, but not the mineralization (i.e. CO2 production) Somehow the authors favor the second mechanism. But this choice is only speculative. There is no data to support this conclusion. Based on O2 consumption data alone (i.e. without CO2 production rates or other mineralization data), one cannot rule out the first option. Accordingly, the discussion on p 10793 about transient redox fronts as the cause of the decreased O2 consumption

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after the flood is speculative. Overall, the discussion of the decreased O2 consumption after the flood should be more thorough.

The dataset offers more topics for discussion than currently addressed: (1) An average TOU/DOU ratio of 1.2 is reported, but no further discussion is given what could cause the DOU to differ from the DOU, nor is any comparison made with TOU/DOU ratios in other coastal environments. Is there any information on bioturbation (eg 210Pb) or bio-irrigation rates, or info on the benthos at the sites (as a proxy for the intensity bio-irrigation and bioturbation)? (2) One recent topic of attention is the small-scale variability in DOU and OPD, as recently demonstrated in the deep sea (see Glud et al L&O 2009). Figure 10 hints that such variability is also present here (some profiles show a homogeneous consumption, while other profile at the same site show a paek consumption at the oxic-anoxic interface). How much variability is there in the DOU-OPD relation here (a DOU versus OPD plot might be useful)?

Section 4.1 discusses that ex situ DOU values are lower than in situ values for the stations in the river mouth displaying high activity (large DOU and small OPD). However, this discussion is rather unsatisfying. After a quite long and winding discussion, a boundary layer effect is hypothesized to be cause of the discrepancy. However, no further arguments are given to support this hypothesis. Firstly, the thickness of the diffusive boundary layer could be deduced from the upper part O2 profile (at least for some “nice” profiles). Secondly, one could vary the stirring rate of the lab incubators to examine boundary layer effects on DOU (this has been done in the past).

Figure captions and elsewhere (P10785). The porosity is not “calculated” but fitted to expression (2). Because of the freedom inherent in this fitting procedure (one can adapt the parameter m), one should expect a rather good agreement between data and fitted profiles. Accordingly, the correspondence between “measured” and “calculated” profiles is not as surprising.

Detailed comments

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I would suggest to split section 4.2 into separate sections - spatial variability in O2 uptake - seasonality in O2 uptake - effect of flood deposits on O2 uptake

P10790 An input of 3.5x10ˆ6 tons of and 80x10ˆ3 tons of org C provides an OC content of 2.29% in the deposit (solid sed density = 2.5 g cm-3; posrosity = 0.8). However, the data in figure 11 shows only an OC content of ∼1% in the deposit. Where does this discrepancy come from?

The origin/source/authors of the discharge and SPM data in Figure 2 is not mentioned.

The legends of Figures 3 and 6 are too small to be readable

P10777 L3 Organic carbon is not a chemical element Title and P10778L14. “carbon recycling” “organic carbon recycling”. More careful terminology needed. Organic carbon is not “recycled”, but mineralized or decomposed to CO2 (recycling implies the transformation of one form of organic matter into another).

P10778 “Brought insights on” -> “provided insight into” (there are more examples in the text of incorrect use of expressions)

P10782 L2 delete “steady state” P10782 L3 “Conservation of overlying water. . .” bad expression

P10785 “normal to flood condition”bad English

In station A the porosity drastically decreases right after the flood (to 0.6), but the grain size is really small. So this is not due to a sandy deposited, but due to unconsolidated mud.

10791 “impoverished bio-avialable compounds” what is meant by this

P10793 Units of O2 consumption rate: mmol O2 L-1 h-1. Is this per bulk volume of sediment or per volume of pore water?

P10793 L Wrong units: 30ˆ2/10-5 = 9x10ˆ7 s (which gives 1041 days or 2.8 years)

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