Biogeosciences Discuss., 7, C4698–C4703, 2011
www.biogeosciences-discuss.net/7/C4698/2011/

Biogeosciences Discussions

Interactive comment on "Influence of distributary channels on sediment and organic carbon supply in event-dominated coastal margins: the Po prodelta as a study case" *by* T. Tesi et al.

T. Tesi et al.

tommaso.tesi@bo.ismar.cnr.it

Received and published: 11 January 2011

1. The particularity of the flood (end of a long series) is presented by the authors in the first section but totally forgotten in the discussion and conclusion. The main problem in discussing the results is the flood characteristics: this flood was preceded by 8 other events. I have the feeling that the succession of flood which occurred during winter-spring 2009 "cleaned" the river bed from deposits, and certainly changed the quality of the transported material. This may have influenced the way this material is transported. I recommend the authors to give more argument (if existing) on the fact that the May flood is equivalent to other autumn floods or to discuss this particularity

during the paper (discussion) and in the conclusion.

We concur with the reviewer and we did mention this potential issue (see page 7868 line 27). Indeed, there is the possibility that channels and river bed were cleaned up because of this series of floods. On the other hand, the river sampling, carried out a few days before the May 2009 flood, still shows relatively high suspended sediment concentrations consistent with the Po river rating curve (Syvitski, J. P. M., Kettner, A. J., Correggiari, A. and Nelson, B. W.: Distributary channels and their impact on sediment dispersal, Marine Geology, 222, 75-94, 2005). Therefore, there are not enough evidence that proof depletion of sediments in the Po river bed.

2. Mixing model is certainly wrong as it contradicts 13C or 14C results. The mixing model which is exposed in the discussion section (page 16, figure 11; it should first be described in the method section) is certainly wrong. The authors have based the model on lignin and C/N ratio, this latter being known to vary with degradation state of the material. From Figure 11 and 12, the impression is that a majority of the organic matter is from autochtonous origin (60-70%). Yet this does not fit with the 13C content of these samples which is around -24 to -25‰ and even less with 14C (at least in sediments) which is around -200‰Ź Thus this material is far from the signature of marine phytoplankton (autochtonous OM) which should be around -20‰ in d13C and 0-50‰ in D14C. If the authors whish to maintain their mixing model, they should mix more sources and constrain their model with their entire data set. As this model does not bring, to my opinion, major information on the system, it is not a problem to remove it; if the authors choose to.

We think the reviewer completely missed the point. Rivers are usually supersaturated in CO2 because of intense decomposition of terrestrial biomass. This biomass-derived CO2 is isotopically depleted compared to the atmospheric CO2 (both δ13C and $\triangle$14C) (Mook W.G. 13C in atmospheric CO2. Netherlands Journal of Sea Research, 20, 211-223, 1986). For any given seawater and freshwater mixing, the isotope composition of CO2 in solution changes. As a result, phytoplankton growing in prodelta areas exhibit a isotopic composition relatively depleted and variable compared to phytoplankton characterized by a typical marine-like signature (Chanton & Lewis, Plankton and dissolved inorganic carbon isotopic composition in a river-dominated estuary: Apalachicola Bay, Florida, Estuaries, 22, 575-583, 1999). Only in regions of the ocean where the sea-air exchanges of gasses are not affected by advection of depleted CO2, phytoplankton exhibit a modern radiocarbon age and typical δ13C signature of mid-latitude (-19\-21‰. For example, similar mechanisms occur in upwelling region where aged water masses supply isotopically depleted CO2 to the surface ocean (Eglinton, T. I. et al. Variability in radiocarbon ages of individual organic compounds from marine sediments. Science 277, 796–799, 1997). In fact, it is worth to highlighting that our mixing model is not between marine and terrigenous OM as the referee said, but between AUTHOCTHNOUS and ALLOCHTONOUS material (see Figure 11 and 12 and the text). We actually thought we gave a detailed explanation in the text (Page 7875 from line 4 through line 13). We have now expanded this part adding more details and references.

3. Potential resuspension of material during the winter-spring season is not quoted in text but could explain lack of recent deposit in sediment. The authors mention (section 5.2 page 11 and 12, ligne 490 to 520) that the deposits of the first eight floods of winter–spring 2008-2009 are not visible in cores taken before the May flood, neither on X ray images nor on 7Be data. They conclude that the deposition was not significant for "moderate" flow (4 times average river flow!!). Yet they never quote resuspension in the Adriatic as being a process which could transport away deposition of previous floods. As most of their cores are located above 20 meters of water depth, and weather can be rough (as quoted in text with 3 meters high waves at the end of April "which resuspended sediments" line 362), resuspension is certainly a major processes in modifying these deposits. This should be discussed in section 5.2 with wind/wave data over the period.

Our conclusions are quite different from what the reviewer is saying. We actually agree with the reviewer and we think that wave-supported resuspension dramatically affect

flood deposits. This is what we said. In our opinion, strata preservation is the result of the balance between deposition and disturbance. Whereas strata formed after the flood of the century were thick enough to compensate erosion/bioturbation (strata are still recognizable years after their emplacement, Page 7869 line 24), preservation of moderate flood events occurred from 2008 trough 2009 were not observed in the sediment record. Indeed, we did mention several times the potential effect of wave-driven resuspension in both discussion and conclusion. We are not rejecting the occurrence of resuspension processes at all. We are actually saying that thicknesses are not sufficient to compensate post-depositional processes including resuspension (Page 7870 line 6; Page 7870 line 9).

4) The paper is not enough concise especially the discussion section. The discussion section is not concise enough and does not go straight to the point which is the difference between the northern channel and the other channels in caryring particles to the Adriatic. The authors should integrate their different results (river, suspended particles and sediment) in order to come with a web of argument on the relative role of distributary channels. With the present splitting in paragraphs, the target is not reached, which makes the discussion difficult to read.

We believe that the paper is well structured as pointed out by reviewer #1. The main argument we see is the feature of our data extremely multidisciplinary as it includes both biogeochemical and sedimentological results. It was written for both biogeochemists and sedimentologists and throughout the text we provided the basic information to help readers having different background. It might be a bit long as pointed out by both reviewers. This is why we have shortened the paper avoiding redundant information.

5) The structure of the paper mixes discussion and results. Figure 7 is hardly presented in the result section and Figures 8&9 are not presented at all, but introduced in the discussion. These data are very important to the discussion of the fate of particles between the distributary channels and should be presented in the result section.

Data showed in Figure 7,8,9 were presented in the result section using exhaustive tables (Table 7 and 8). However, the aforementioned figures were included in the results as asked by the reviewer.

6) Some errors are included in some Tables and Figure which should thus be all checked carefully before publication. In Table 4, the average of OC for top and bottom during moderate discharge does not match the numbers (6.1, 1.5, 1.4, for an average of 6.9; and 0.9, 2.9, 3.4 for an average of 4.1). Figure 5 which shows alongshore transects at peak discharge should show a red minimum of transmittance near Tolle (transect B) according to Figure 4 but it is just pale yellow (not red as expected). The authors should check this graph.

Errors were corrected and tables checked for consistency. The differences between Fig. 4 and 5 are likely caused by data interpolation. While the surface counter maps were made using only surface data, transects consider the whole water column. Therefore, differences might occur because of subdata sets used by the software to generate contour maps.

7) The abstract does not refer to distributary channels, except in the last sentence " channel network". The term "distributary" does not appear in the abstract. Yet this is the main scope of the paper. The abstract should be completely re-written in order to reflect the scope of the paper.

Reading this comment and comment #4 we had the impression that the reviewer missed the main topic of the paper. Our paper focuses on the land-ocean exchange in a multi-channel setting. Previous studies showed deposition in shallow regions of the Po prodelta because of the buoyancy-driven transport. Indeed, in the introduction (see Page 7852 line 21) and throughout the text we extensively discussed about the limited transport capacity of a multi-channel setting that causes deposition in shallow regions even during flood events. Differences between channels is definitely another important question that we wanted to investigate but it is not the only focus of the paper. How-

ever, we adjusted the abstract because somehow the word "distributary channel" was missing and this aspect the referee is definitely right.

9) Figure 6 has no depth scale which makes it difficult to read. I also suggest adding the 7Be data superimposed on this Figure which will complement it very well. The 7Be data are not shown in this paper.

The initial version of Figure 6 had both y-axis and 7Be profiles. However, the figure resulted really small and too confusing. In particular, laminae and internal structures of radiographs were hard to see. Therefore, we decided to make a separate table containing 7Be data and give a scale bar in the Figure as reference.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/7/C4698/2011/bgd-7-C4698-2011-supplement.pdf

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Interactive comment on Biogeosciences Discuss., 7, 7849, 2010.