

## ***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.***

**Anonymous Referee #2**

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The paper by Tesi et al., entitled Influence of distributary channels on sediment and organic carbon supply in event-dominated coastal margins: the Po prodelta as a case study, deals with the transfer of solid material from a large Mediterranean river (the Po River) to the Adriatic Sea. The authors focus on a large flood which occurred in May 2009 and was preceded by a spectacular series of 8 floods started in November 2008. The approach of the paper relies on a coupling of measurements on river particles, on suspended particles in the prodelta region and on sediment cores sampled before and after the flood. Overall it is a good paper where results from a previous flood in October 2000 which occurred in a totally different context are compared to the flood studied in this paper. The approach based on a combination of organic tracers (Org-C, TN,

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<sup>13</sup>C, <sup>14</sup>C, Lignin) measured on river and shelf particles and margin sediments brings interesting constraints on the system. Furthermore, the temporal approach which relies on samples taken before, during and after the flood provides insight on the dynamics of the flood delivery and deposition.

However, the paper has several major flaws which need to be addressed and corrected before the paper can be published in BG. On the science:

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.

2. Mixing model is certainly wrong as it contradicts <sup>13</sup>C or <sup>14</sup>C 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 autochthonous origin (60-70%). Yet this does not fit with the <sup>13</sup>C content of these samples which is around -24 to -25‰ and even less with <sup>14</sup>C (at least in sediments) which is around -200‰. Thus this material is far from the signature of marine phytoplankton (autochthonous OM) which should be around -20‰ in  $\delta^{13}\text{C}$  and 0-50‰ in  $\delta^{14}\text{C}$ . If the authors wish 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.

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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 <sup>7</sup>Be 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.

On the shape of the paper:

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 carrying 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.

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.

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

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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.

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.

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

Recommendation: The paper deserves to be published giving the number of very important information and insights on processes during major river floods. However, the large number of comments in my review indicates that large revisions both on the scientific arguments and on the format of the paper are necessary, but no further review is required.

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