

## ***Interactive comment on “Diagenetic alterations of amino acids and organic matter in the upper Pearl River Estuary surface sediments” by J. Zhang et al.***

### **Anonymous Referee #1**

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Review of “Diagenetic alterations of amino acids and organic matter in the upper Pearl River Estuary surface sediments” By Zhang et al. for Biogeosciences.

This manuscript presents a potentially interesting study. However, I do have some concerns about the study as well as the manuscript. These are my main comments:

#### **GENERAL COMMENTS**

-Size fractionation: Different sediment size fractions were analyzed. I am not sure whether one can compare size fractions of sediment with those in the water column (as done in the discussion). I can imagine that physical and chemical interactions working on sediment up to 8 cm deep result in different size fractions that those in the SPM

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in the water column. Moreover, how does freeze drying and sonication affect these size fractions ? These points should at least be addressed. On a more basic level, I wonder about the usefulness of studying different size fractions within the sediment. The rationale behind this size fractionation should at least be explained.

-A whole suite of amino acids was analyzed but the manuscript only deals with the D-AAs. The non-DAAAs actually provide a very useful diagenetic indicator, for example in the form of a degradation index (Dauwe et al. 1999) (as indicated in the introduction). Inclusion of these data would make the manuscript more complete and thereby stronger.

-Those data that are presented are not fully exploited. In particular the bacterial contribution to total sediment OM (based on D-AA data) can be quantified further (see comment below) and should be combined with the non-DAA data.

-The discussion is rather descriptive and the main findings and conclusion are not (or only loosely) derived from data presented in this manuscript.

-Related to the previous: The discussion and conclusions appear to rely quite strongly on findings by others. In particular data from previous studies in the studied system (section 4.1, page 3332) and findings and conclusions from other studies (in particular Tremblay and Benner 2009).

-Referencing to other studies is rather focused on studies by Benner and co-workers (e.g. Tremblay and Benner 2009, Davis et al. 2009) that primarily focus on dissolved organic matter in the water column. These studies are certainly relevant but given that the present study concerns total organic matter in sediment, there should be more attention to studies on (D-)amino acid diagenesis in sediments (for example: Keil et al. 2000, Grutters 2002, Lomstein et al. 2006+2009, Vandewiele et al. 2009).

## SPECIFIC COMMENTS

-Methods: I do not understand the choice of sampling locations. Sampled stations

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were all in the same part of the estuary within a relatively small area with similar characteristics (Table 1) and similar results. It would probably have been more informative/interesting to study a larger spatial scale (e.g. gradient from fresh water to the open sea).

-Results: Section 3.2 and Table 4: “D-enantiomers . . . were measured in some samples” > Does this mean that only a selection of samples was analyzed for D-AAAs or that concentrations were below detection limit in many samples? In the latter case, this should be indicated as such instead of presenting concentrations of 0.00 nmol mg C-1 in table 4 and text.

-Discussion: -Structure is not very clear. Sections 4.1 and 4.2 appear to overlap (sources of OM). -It should be made more clear which results are derived from from the present study and which are derived from other studies. -As mentioned above, the bacterial contribution to OM is not quantified while this was done in previous (comparable) studies (e.g. Keil et al. 2000, Grutters et al. 2002, Lomstein et al. 2009, Vandewiele et al 2009). -As mentioned above, the manuscript would be much stronger when non-DAA results are included (and compared with those for the D-AAAs, THAA yields, and C/N ratios).

-Concluding remarks: As mentioned above, few of the findings and statements in this section appear to be based directly on the data presented in this manuscript.

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**BGD**

8, C882–C884, 2011

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