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Biogeosciences Discussions

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 #3

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GENERAL COMMENTS

The study presented in this manuscript does not provide any substantially new concepts, ideas or methods. The title of the manuscript sounds promising; however, the announced topic is not really addressed. Most of the discussion is based on carbon and nitrogen concentrations and ratios (see comments below) and concentrations and yields of D-amino acids. Diagenetic alteration of other amino acids is not included, although data are available. Surface sediments in this study include the upper 8 cm of sediment, irrespective of sedimentation rate, sediment mixing, and thus sediment age (diagenetic stage). The potentially interesting approach of size fractionation is most

likely biased by the sample preparation. Large parts of the discussion and respective conclusions are based on unpublished data and data from previous or related studies.

SPECIFIC COMMENTS

- Introduction:

The introduction is very general and includes aspects that are not addressed in the presented study, e.g. diagentic indicators based on the composition of THAA. There are very few places linked to the present study, e.g "few studies are available on the diagenetic state of sediment OM of different sizes, especially in estuarine sediments" and "the bacterial contributions to sediment OM of different size fractions and the respective diagenetic status are still poorly understood". The authors should elaborate more carefully on these aspects, which are meant to be the key aspects of the manuscript. Why is it interesting/relevant to know? What should we expect? What is the research question/hypothesis behind? The last paragraph sounds promising. If data on THAA composition were included, the study would have the potential to address these questions.

- Materials and methods:

2.1. Site description: The site description is very detailed, but it is not explained why this site was chosen for this study and the discussion never refers to any of the information given here. The authors should check section 2.1 and Table 1 for relevant information. There is a reference missing page 3327, line 14-17.

2.2. Sediment sampling and sample processing: Water column samples are mentioned but not included in the following manuscript. Sample processing might bias sediment fractions (see below).

2.3. Sediment fractionation: I have major concerns regarding the processing and fractionation of the sediments samples. Samples were freeze-dried and homogenized in an agate mortar (gives a fine powder), and afterwards dispersed by sonication. After

fractionation CPOM and FPOM were oven-dried. This procedure should have an effect on the size distribution of OM in the sediments. The authors definitely have to address this point and assess a possible bias. It would be interesting to know the yields of the individual fractions and how they vary for the investigated samples. This could also be compared to untreated sediment. The description in 2.3. does not match the scheme in Fig. 2 (e.g. filtration steps). The sediment fractions should be defined more exactly.

2.4. Elemental analysis: The information is not sufficient to reproduce the analysis. How much material was used for the analysis? What was the precision and the detection limit? How many replicates were analyzed and what was the deviation? The concentrations reported in the manuscript are quite low and display strong variations. Total N includes inorganic and organic N. Is there any information on the fraction of inorganic N? Since large parts of the discussion are based on C/N-ratios and N-yields of amino acids, this aspect should be elaborated and discussed carefully. There is a reference missing page 3328, line 20.

2.5. Amino acid analysis: Is there co-precipitation of amino acids during neutralization of the hydrolysate? Reference missing page 3329, line 8. What is the procedural blank of the method, including the two filtration steps? What filters were used (resistant to 6 N HCl)? Individual amino acids were analysed – where are the data?

- Results:

The results part is extensively descriptive, there are far too many details, e.g. page 3330, line 26 to page 3331, line 21. All the data are given in tables 3 and 4. The authors should highlight the most important findings and trends, rather than repeating all the numbers given in the tables. Concentrations and yields of 0.00 are below the detection limit or just <0.01?

- Discussion:

The discussion (and results) is restricted to the size fractions. Intersite differences are

not discussed at all. What was the rational for sampling different sites?

4.1. Sources of CPOM, FPOM and UDOM: The first part of this section describes findings and interpretations of previous studies without linking them to the present study. In the second part, there is a clear statement that CPOM is mainly composed of “soil sand-associated terrestrial OM and floated vascular plant fragments” (page 3333, line 10). However, it is not clear how this conclusion was reached or whether it was taken from literature (in the latter case, a reference has to be included). There is a back and forth of arguments, partly from literature, partly referring to data of the present study. Same holds for the discussion of FPOM and UDOM sources. A key conclusion (“contribution of bacterial DOM to UDOM”) is based on unpublished data (muramic acid). Large parts of the source discussion are based on C/N-ratios. It is very likely that N is not entirely organic – this has to be considered when evaluating the ratios.

4.2. Diagenetic trends for sediment OM of different size classes: The first part of this section summarizes and extends the information given in the introduction. There is no discussion included in these 16 lines. The remaining section is rather descriptive. Findings of earlier studies are not well linked to the present study. Conclusions are rare and poorly supported by unambiguous data. The “size-reactivity continuum” model is mentioned but not explained and not well linked to the present study. It should clearly be stated that the applied diagenetic indicators are well-established in organic matter research and not the novel outcome of the present study (e.g. page 3337, lines 3-5).

4.3. Qualitative bacterial contribution to sediment OM of different sizes: I cannot follow the argumentation, neither the conclusions reached in this section. What is the message?

- Concluding remarks

The conclusions are not well constrained with data and discussion (see comments above).

- Tables:

1: Most of the data presented in table 1 are not relevant for the study and not included in site description or discussion.

2: Why is this gradient table shown? The authors should give the reference for the original method and describe important modifications in the text.

3: I wonder how many decimals can be justified for the different parameters.

4: See comment above: 0.00

- Figures:

1: It would be helpful for readers to have an idea about the location in China, e.g. where is the ocean?

2: The scheme contains information that is not mentioned in the text. The figure caption mentions TOC and Total N, which are not in the scheme.

3-6: It is more appropriate to show the data range as average and standard deviation (or even as box and whisker plot). Bars do not show the minimum values.

6: Standard deviation should be included.

7: See comments above: N might partly be inorganic. The regression in 7a is mostly determined by 3 data points with high total N concentrations, almost 100 mg N per 100 mg C (obvious inorganic contribution). And I do not see the value of the figures. All axes are normalized to 100 mg C. The more N in the sediments, the more N in THAA. Is this the message here?

Interactive comment on Biogeosciences Discuss., 8, 3323, 2011.