

To the  
Editorial Office  
Biogeosciences (BG)

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Wilhelmshaven, June 16, 2010

Reviewer's comments to the manuscript

"On the origin of highly active biogeochemistry in deeper coastal sediments—inverse model studies" by J. M. Holstein & K. W. Wirtz

We thank the referee for the stimulating comments. A major concern was related to the "broader relevance" of the study insofar a seemingly local phenomenon is dealt with. We agree that in the current version generalizing aspects come short. To underline the relevance of our conclusions on larger scales, we will discuss (in a revised version of the paper) observations of rapid sediment accumulation and thus accretion in various tidally influenced systems worldwide. In conjunction with occasional massive deposition of organic material such events may have created similar subsurface zones of high remineralization activity. We also aim to better clarify the added value of the inverse modeling approach. In doing so, we will in particular address the broadened interpretation frame and the constraining of biogeochemical parameters that are difficult to measure.

We are thoroughly revising the abstract and parts of the manuscript with special emphasis on clarity, model description (see Reply to review 2) and general outcomes.

Please find brief responses to the specific comments below.

1. **Referee 1:** *The title is misleading. The word deeper is ambiguous and may be better replaced with something that would indicate that the study deals with sediments in a region with active sediment transport and that the 'normal' diagenetic cycle is disturbed and organic-rich layers are rapidly buried deep into the sediment*

Agreed. New title is: Organic matter accumulation and degradation in subsurface coastal sediments: A model based comparison of rapid sedimentation and aquifer transport

2. **Referee 1:** *A good effort is made to distinguish the contributions of POC vs. DOC, and the paper may benefit from more discussion of their relative roles. Also, the profile of TOC (which is a proxy for POC?) in Fig. 6 perhaps could be referred to earlier, in the part of the text that discusses properties of the cores, such as in Fig. 2.*

The discussion of the specific roles of POC and DOC, especially with respect to transport, is given more room. It is clarified that TOC (in dry matter) is a proxy for POC.

3. **Referee 1:** *Fig. 2: The profiles show SO<sub>4</sub> peaks at around 2 m depth that could be mistakenly interpreted as evidence for the production of SO<sub>4</sub>. Whereas Fig. 9 gives hints to their origin, the authors may wish to emphasize that these features are transient and would not be sustainable in steady-state diagenesis*

We revise the scenario descriptions and mention that both scenarios studied, the SO<sub>4</sub> peak at around 2 mbsf is transient and would eventually disappear.

4. **Referee 1:** *The values of only 9 'most sensitive' parameters are specified. Are the rest of the 84 model parameters the same as in Holstein and Wirtz (2009)? If yes, it should be stated; if not, the complete model parameterization has to be given, perhaps as an annex*

We indicate that model parameterization is the same as in Holstein and Wirtz (2009).

5. **Technical corrections:**

**Referee 1:** *p. 2072, line 7: 'supposedly' rephrase*

ok.

6. **Referee 1:** *Line 20: 'gouverning' ? 'governing'.*

governing!

7. **Referee 1:** *p. 2073: '..full value at biomixing depth' specify this depth.*

rephrased with reference to Tab. 1, where the parametrization of the target parameter "biomixing depth" is given.

8. **Referee 1:** *Fig. 6: Indicate in the caption or legend which line is which.*

ok.

On behalf of all authors

Jan Holstein