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Biogeosciences Discussions

Interactive comment on "On the origin of highly active biogeochemistry in deeper coastal sediments – inverse model studies" *by* J. M. Holstein and K. W. Wirz

Anonymous Referee #1

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General comments

The paper presents a thoughtful and carefully executed study of coastal sediments at a site where sediment transport disrupts the normal biogeochemical cycling in the sediment, resulting in the burial of organic-rich layers several meters below the sediment surface. Results are discussed in detail and interpreted from several perspectives. Particular emphasis is given to diagenetic modeling, which is used not only to reproduce observations but also to investigate scenarios and estimate errors. The model is not particularly complex but very appropriate for the situation, given the large number of simulation runs. I find the study interesting and see no major flaws. Its main weakness, in my opinion, is that the conclusions are site-specific and little effort is made to demonstrate broader relevance (the Abstract, in particular, can be substantially improved). Similarly, the employed approaches are interesting in themselves, especially the modeling ones, and emphasizing their generality may add to the broader relevance of this work.

Specific comments

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.

2. 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.

3. Fig. 2: The profiles show SO4 peaks at around 2 m depth that could be mistakenly interpreted as evidence for the production of SO4. 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.

4. 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.

Technical corrections

p. 2072, line 7: "supposedly" – rephrase.

Line 20: "gouverning" $\rightarrow$ "governing".

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7, C623–C625, 2010

Interactive Comment

p. 2073: “..full value at biomixing depth” – specify this depth.

Fig. 6: Indicate in the caption or legend which line is which.

Interactive comment on Biogeosciences Discuss., 7, 2065, 2010.

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7, C623–C625, 2010