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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 #2

Received and published: 18 May 2010

General comments:

In their study, Holstein and Wirtz try to unravel the interplay of different reaction and transport processes that results in a distinct sulfate minimum zone and an ammonium peak in deep backbarrier tidal flat sediments (German Wadden Sea). They use a one-dimensional model to test and compare two different transport scenarios for the transfer of reactive organic matter into deeper sediment layers. The first scenario assumes a rapid burial of reactive organic matter associated with tidal flat propagation, while the second scenario investigates the plausibility of lateral advection in an

aquifer or due to tidal pumping. Reaction and transport parameters are determined on the basis of an inverse modeling study. Simulation results reveal that the lateral transport scenario is rather unrealistic, while the burial scenario is more consistent with observed morphodynamic features.
The biogeochemical dynamics of intertidal sediments is generally characterized by a very complex interplay of a large number of different reaction and transport processes that act on very different scales. Field observations from sediment cores only provide snapshots of the local, instantaneous dynamics. This study could potentially show how diagenetic models and inverse modeling complement field data by testing the plausibility of different scenarios and quantifying reaction and transport processes. Yet, the conceptual idea is not very well presented. The model set-up is not clearly outlined and justified. Especially the choice of the two transport scenarios, the parametrization of the organic matter model and the inverse modeling require a much better justification. In addition, the paper does not read fluently and is badly structured. Therefore, the manuscript needs to undergo a careful and major revision before an eventual publication.

Specific comments:

-Title:
The title is very vague and does not properly reflect the content of the presented manuscript.

-Introduction:
The introduction is badly structured. The scientific question is not clearly defined and it is not put into a broader context. Single paragraphs or subsections are not clearly connected. For instance, the connection between tidal flat morphodynamics and organic matter degradation remains unclear. In addition, the introduction provides a

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lot of detailed information that is not directly relevant to the questions addressed in the manuscript. Therefore, the introduction needs to be rewritten.

-Modeling approach & model structure:
These sections are not well organized and, therefore, hard to follow. For instance, the description of the two model scenarios provided in section 2.3 (p. 2074 16-22) should be moved to section 2.2. Furthermore, the general model description (section 2.3) should be provided before the two scenarios are described (section 2.2).

-Modeling approach:
The two transport scenarios are not very well explained and their choice is not justified. The authors should provide a better justification for the choice of these two scenarios. Isn't it possible that other biogeochemical processes could cause the observed depth-profiles? And if not, why? In addition, the names "advection" and "burial" are not appropriate, since both processes are advective.

-Data:
The authors need to include a short description of the data collection and measurement methods. How critical is the time difference of 1 to 3 years between the core retrieval at site NN1 and NN2?

-Model structure/ Transport:
I have doubts concerning the suitability of the model. I doubt that the one-dimensional model can be applied to the two-dimensional horizontal advection scenario (scenario B). The authors argue that this treatment is justified if horizontal gradients are negligible. However, the depth-pofiles from NN1 and NN2 show that there are strong horizontal gradients. The authors have to provide the comparison between their 1D approach and the 2D simulations they performed to verify this approach.

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The transport scheme has a comparably coarse resolution (10 cm). Yet, the model accounts for bioturbation and bioirrigation, which affect the uppermost centimeters (<10 cm) of the sediment. In addition, rapid sedimentation or erosion events will probably exert an important effect on the biogeochemical dynamics in the upper sediment. Therefore, the authors must provide a better justification of their model choice. They need to show that the model is suitable to address their questions in order to increase the confidence in their results.

-Model structure/ Organic Matter Model:
The parametrisation of the organic matter model and in particular of the quality classes exerts an important influence on the results since the amount of reactive or intermediately reactive organic matter that reaches the deeper sediments drives the biogeochemical dynamics at these depths. Yet, the authors do not provide any explanation for the distribution of bulk organic matter into different quality classes. I am surprised that critical parameters, such as the distribution among the quality classes are not included in the inverse modeling approach.
In addition, the initial conditions for scenario A and B are very different. Why? And how are they chosen?

-Inverse Modeling:
The inverse modeling is only based on two profiles (SO4 and NH4). How confident are the authors in their results? Why did they not include other depth-profiles?

-Results:
The authors should also provide the complete set of simulated depth-profiles for their most plausible burial scenario to increase the confidence in their results. I would like to see the simulated methane, sulfide, oxygen, nitrate and rate profiles. They could compare these results with available field data or published data from similar sites.

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Technical comments

p.2066, l. 3: SO4 and NH4 are not defined

p.2066, l. 6: the authors refer to organic matter as OM, TOC, POC or POM throughout the manuscript. They need to be more consistent.

p.2066, l. 9: Capital S

p.2066, l.17: The term "specific assumptions" is unclear.

p.2066, l.22: propagation

p.2067, l. 1-11: This paragraph is obsolete.

p.2067, l. 19-24: What is the connection to the scientific questions addressed in this study?

p.2067, l.25: unusually

p.2068, l.2-3: The term "relevant estimates for local deposition" is unclear.

p.2068, l.22: replace "chiefly" by "mainly"

p.2068, l.24: longterm

p.2069, l.13-24: This paragraph has nothing to do with "the fate of oragnic matter". The introduction needs a better structure.

p.2069, l.27-29: Sentence unclear.

p.2070, l. 7: Capital S in "sedimentary"

p.2070, l.13: Rephrase. Don't start sentence with "40 m".

p.2071, l.1: remove "is"

p.2071, l.10-12: Move this paragraph to the introduction.

p.2071, 17: differentiating

p.2073, l.2: replace "diffusion acting on" by "a diffusive processes for both"

p.2073, l.4: Add reference for "with exponent 2/3".

p.2073, l.16-22: Move paragraph to section 2.2.

p.2074, l.4-12: Move paragraph to section 2.2.

p.2075, l.11: "accurately fitting" is very vague.

p.2077, l.8: Why do the authors include bioturbation if there are no bioturbation structures?

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p.2077, l.15-16: Sentence unclear.
p.2077, l.29: What is a soft peak?
p.2078, l.19-20: Sentence unclear.
p.2080, l.16: Sentence unclear.
p.2089, Fig.1: Increase size, show zoom into study area and indicate site location
p.2091, Fig.3: Scenario A is unclear.
p.2096, Fig.8: Increase size.

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