

## ***Interactive comment on “Paleo-environmental imprint on microbiology and biogeochemistry of coastal quaternary sediments” by M. Beck et al.***

**M. Beck et al.**

m.beck@icbm.de

Received and published: 8 November 2010

We would like to thank anonymous referee #1 for his generally positive comments. We appreciate his efforts in reading our manuscript carefully and giving fruitful suggestions. We hope that we were able to sharpen the focus of our manuscript.

Anonymous Referee #1

General comments

R1: The deeper motivation for this study was to explore processes, which could also operate in the marine deep biosphere, in the more accessible sediments of an intertidal flat. I think that this working hypothesis is a bit simplistic. . .

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



Interactive  
Comment

Reply: This topic was not the deeper motivation for the study, but a nice side effect. Therefore, we did not put too much focus on this issue and have even shortened the respective paragraph in the Discussion section and removed it from the Introduction to avoid duplications. We agree, this hypothesis is a bit simplistic, but our previous and current results nicely support this view.

R1: . . . other motivation of this work, which was to explore the imprint of past environmental conditions on present-day microbial community composition, biogeochemical processes, and porewater chemistry. This is the more interesting and relevant aspect of this work, but it is explored insufficiently. A broad set of methods and data was used to characterize the sediments chemically and microbiologically. . . To top it off, electron microscopy was used to analyze mineral shapes in order to infer their origin.

Reply: We have tried to elaborate this aspect more carefully throughout the manuscript (see comments below). The figure with the electron micrographs was removed.

R1: . . . I wish the authors would have focused their efforts better and provided more in-depth interpretations. Overall the fundamental new findings of this study fall short of the analytical effort invested to produce this dataset. . . The highlights do not receive the space they deserve in the discussion...

Reply: To focus more on our fundamental findings, we have changed the Discussion. First, we have integrated a new paragraph in the beginning of the Discussion section highlighting the nice fit between sediment structure, biogeochemistry, microbial abundance and activity. Second, we have shortened both, the discussion about spores and the paragraph concerning the relationship between shallow and deep sediments. Third, we have transferred the paragraph on methodological drawbacks of cell quantification to the Results section. Please see specific comments for details.

R1: . . . I suppose that the expectation was that the palaeo-depositional conditions would be strong determinants of the microbiology and biogeochemistry. This is not what was found, or at least not to the degree that might have been expected. If I un-

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

derstand the manuscript correctly, the results indicate that the recent hydrology of the intertidal system dominates the distribution and rates of present-day biogeochemical processes. The sedimentary imprint of past depositional conditions is, while recognizable, not very distinct.

Reply: You are right, we expected a stronger impact of paleo-depositional conditions. Due to your comments, we have rephrased our initial question and have tried to change the thread of the manuscript. Please compare specific comments.

R1: So the title, in a way, is misleading since there is not much palaeo-environmental imprint. So why then this title?

Reply: We have changed the title to: “Imprint of past and present environmental conditions on microbiology and biogeochemistry of coastal Quaternary sediments”.

R1: What is not emphasized very much in this manuscript and deserves more space in the introduction, is the aspect of how far microbial communities and associated biogeochemical processes had changed in response to changes in depositional conditions. Since the drilled cores reach back to the Pleistocene and are about 325000 and 135000 years old, they cover very different climate regimes. . . which all have the potential to change biogeochemical processes and the microbial community composition fundamentally.

Reply: We have added a paragraph in the Introduction section concerning coastal development and depositional regimes due to sea level changes.

R1: The outset of the study is therefore the analysis of material that accumulated in strongly varying depositionally regimes under different climatic conditions and the driving question should have been, how much (not if) these original depositional conditions were overprinted by the development of the recent depositional conditions. The manuscript should focus on this more detailed working hypothesis and use the data necessary to explore this point. Data that are peripheral to this working hypothesis

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



should be left out. My critique focuses on the writing, but also on some of the scientific results, which allow for only ambiguous interpretations given the scope of the manuscript.

Reply: As already stated, we have changed our initial question and the Discussion section to be more precise. To our opinion, all presented parameters are important for answering our initial question: The lithological profiles (Fig. 2) are needed to describe the different depositional conditions. Salinity and silica indicate terrestrial/marine influences. Sulfur species are coupled to present and past sulfate reduction processes. DOC, alkalinity, and ammonia are important for understanding organic matter degradation processes. The quantification of microbial cells and key genes for metabolic pathways is essential to link microbial abundance to biogeochemistry and sediment structure, which is supported by *ex situ* rate measurements.

R1: Despite the abundance of data, one important piece of information, rates of methanogenesis, were not measured. . . At least, porewater modeling of the gas concentrations should have been done to determine methanogenesis rates.

Reply: We agree that methanogenesis rates would be nice for core JS-A. We have tried this, but unfortunately we failed to relate the data to *in situ* conditions. However, the production of methane is reflected in the pore water methane profile and the consumption at both sulfate-methane interfaces is indicated by elevated *ex situ* AOM rates.

Specific comments:

R1: Abstract: How do you define a “transitional state” between the sediment surface and the deep biosphere?

Reply: The expression “transitional state” was deleted and the sentence changed to “Microbial and biogeochemical profiles are vertically stretched relative to 5 m-deep cores from shallower sediments in the same study area, but still appear compressed compared to deep sea sediments.”

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

R1: Line 9/10: Please check the English

Reply: The sentence has been changed.

R1: p.5465 l. 19-23: Please reduce the number of references to the essentials. This is not a review. I am also bothered by the number of self-references in the manuscript overall (23 out of 75 total references).

Reply: In the Introduction we presented the work previously carried out in the study area with relevance to the present study. Nevertheless, following the reviewer's recommendation, we reduced the number of references.

R1: 5466, l.4: Methanogenesis is not a carbon mineralization pathway, the end product is not a mineral ion (carbonate), but an organic molecule, methane.

Reply: Of course, the end product of methanogenesis is an organic molecule. Nevertheless, methanogenesis may be regarded as carbon mineralization pathway due to the consumption of carbon molecules such as acetate.

R1: 5466, l. 6-14. Although mentioned in the intro section, this is written like a results section and really not distinct from the results of this study.

Reply: In this section, former results are described which turned out to be similar to those of this study. The similarity of the results was not known at the beginning and was partly unexpected. To highlight the similarities later in the Discussions section, we think that the description of some former results is necessary in the Introduction.

R1: 5466, l. 23. The word "despite" implies a contrast. Where is the contrast here?

Reply: The sentence has been changed avoiding the word "despite".

R1: 5467, l.4 geologic history: Better "sedimentary history" l.7. This hypothesis is too imprecise to be tested with these methods. Specify more and adapt your methodology.

Reply: "Geologic history" has been changed to "sedimentary history". The last section

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



of the Introduction has been rewritten, defining more precisely the aims of our study: “The overall question was: Are the original depositional conditions still reflected in present microbiological and biogeochemical processes or are they superimposed by the recent hydrology of the intertidal system?”

R1: 5468, I.12. How soon were the CH<sub>4</sub> samples taken after core retrieval?

Reply: Methane samples were taken from sealed cores by drilling small holes into the liners before splitting the cores lengthwise. To specify, we have added the following sentence: “All cores were transported to a nearby laboratory where sampling was conducted within a few hours after core retrieval.”

R1: 5469, I.15. Visual inspection. What do you mean, under the microscope, on core halves? Specify. Isn't the outside of the core smeared?

Reply: We have specified that the core halves were inspected visually. The sedimentary structure was well visible.

R1: 5471, I.5. Why should the *mcrA* only be indicative of anaerobic methane oxidation? Why not of methanogenesis?

Reply: As stated in the original manuscript, the *mcrA* gene is a key gene for methanogenesis, but also indicative for anaerobic methane oxidation.

R1: 5472, section 2.6. The results of the spiking experiments are not described.

Reply: As this is not a major issue, the results of the spiking experiments were hidden in the Discussion section: “A spiking experiment with five representative samples per core and a defined number of cells indicated a recovery rate of 33-82% after DNA extraction and qPCR amplification (5481, I.19-21)”. In the present version of the manuscript, this section was transferred to paragraph 3.3.

R1: 5472, section 2.7. It is important to state that the AOM and SRR rates are only potential rates. Was a carrier used to determine 35S-SRR? It must have, because rates

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

are reported for the sulfate-free zone. I missed <sup>14</sup>C-methanogenesis rates measurements here, instead of many of the other measurements.

Reply: As described in the Methods section, <sup>35</sup>S sulfate (250 kBq) was used as tracer for measuring ex situ sulfate reduction rates (5472, l.15/16). The term “potential” would be used if we provide extra substrates and do short-term incubations e.g. add controlled methane pressures (usually methane) and sulfate (28 mM) to samples, wait some days and then measure the rates by tracer. Or “potential” would be used if a rate per cell would be assumed and the respective organisms would be counted. Instead, all rates were described as “ex situ” throughout the whole original manuscript. Concerning the ex situ methanogenesis rates please see our comment above.

R1: 5472, l.11, replace “columns” with “tubes”

Reply: Changed as suggested.

R1: 5473: Porewater modeling. This is an exaggerated term for what was really done here. This program yields an optimized polynomial curve fit on one data set and is not a model that links redox processes. It is therefore unconstrained and the rates it yields cannot be tested independently with accompanying data. In addition, the assumption of steady state condition is questionable. The steady state assumption violates the fundamental hypothesis of the study – which was to test for the imprint of changes in depositional conditions on biogeochemical rates, i.e., non-steady state.

Reply: Modeling of pore waters has been carried out based on measured sulfate profiles assuming steady-state conditions. This approach yields net microbial consumption rates. The steady-state is a reasonable boundary condition for the investigated system, considering that the biogeochemical pore water system will adapt much faster to the sedimentary system when compared to the sediment-building processes. Therefore, this approach is in fully agreement with our fundamental hypothesis. Further redox-processes have not been considered because our main interest was related here to net sulfate reduction. We agree with reviewer #1 that the application of our

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



approach to further reactions like metal cycling is of interested for future studies.

R1: 5473, I.9. Avoid repetitions.

Reply: The sentence has been changed.

R1: 5473, I.18. replace “between” with “from”

Reply: Changed as suggested.

R1: 5475, I.22-6576, I.2. Discuss the high CRS in more detail. These are unusually high CRS concentrations. What do they represent, what is the importance of these layers for the whole core porewater biogeochemistry?

Reply: The accumulation of chromium-reducible sulfur is associated with enhanced organic matter contents in the muddy sediment sections, and at depth in particular with the occurrence of peat layers. These sediment sections were already pyritized upon very early diagenesis during development (Böttcher et al., 2000; Dellwig et al., 2007) and keep their original, partly very high, pyrite contents. The diagnostic features for describing the sedimentary environment will be discussed in an accompanying publication (Köster et al., in prep.). As shown by the SEM pictures, however, we can demonstrate that iron sulfide formation may also continue later upon diagenesis leading to slight modifications of the original sedimentary signal.

R1: 5476, I.20-22. The last sentence in the Si section reads too much like an undeveloped thought. If diffusional transport needs to be taken into account for an interpretation, then this should have been done.

Reply: Holocene deposits can be distinguished from non-marine deposits by using Si concentrations. We have stated that an exact assignment of silica concentrations to discrete lithologies is hampered as boundaries are blurred by diffusional exchange.

R1: 5477. I.1-24 and 5478. This is probably the most interesting section of the manuscript. I wish the authors would make this part more prominent, in particular

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper





section 5478, line 15-19 is very short.

Reply: We did not change the Results section concerning this point. However, we made this part more prominent by adding a introductory paragraph to the Discussion section.

R1: 5478, I.12 “higher”. Shouldn’t this be “lower”? Which particular section do you refer to? The SRR vary significantly.

Reply: The section we refer to are the top 6 meters of cores JS-A. The modeled maximum volumetric rates for this section were indeed higher than the ex situ rates. Even though both approaches give divergent results, both are significantly lower than rates determined for highly active tidal flat margins (Beck et al., 2009) and slightly higher than those detected in the deep seafloor (Parkes et al., 2005).

R1: 5479, I.8-I.13. This section is awkward. Rephrase “enrichment of remineralisation products”. I guess you mean high alkalinity and ammonium concentrations. There is also something wrong with the following sentence: “The current sulfate reduction rates are much lower due to the lack of sulfate.” Lower means that there is still sulfate reduction, although there is no sulfate in this section! Please clarify.

Reply: We have clarified: “For example within mud-dominated sediments of site JS-A (depth interval of 5 to 11 mbsf), this preservation is reflected by high alkalinity and ammonium concentrations which are indicative for remineralization processes. Currently, sulfate reduction rates in these layers are not measureable due to the lack of sulfate and labile organic matter (Fig. 6).”

R1: 5479, I.14. “Although sediment age has an impact on microbiology. . .” This is imprecise, as it is also possible that sediment type and original organic mater composition has an impact on microbiology, irrespective of sediment age. In addition, replace “microbiology” with “microbial community composition”?

Reply: We have included your suggestions: “Although sediment type and original or-

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



ganic mater composition has an impact on the microbial community composition in core JS-B, the imprint of the paleo-environment on microbial processes is less pronounced than expected.”

R1: 5479, l.16 correct to “a relationship between lithology, microbial abundance, and activity...”

Reply: Changed as suggested.

R1: 5479, l.18-24. This is another awkward paragraph, in particular this sentence: “The lithological structure influences the exchange”. Why not talk directly about porosity and permeability, which influence diffusion and advection?

Reply: We have changed the term “lithological structure” to “porosity and permeability”.

R1: 5479, l.20-24. The authors should put more effort to the interpretation of the SRR pro- files, since these provide direct evidence for the interpretation of decreasing bioavail- ability/increasing carbon recalcitrance. Again, methanogenesis rate measurements would also help here.

Reply: The following sentence was added: “Even though sulfate can still serve as electron acceptor for microbial respiration below a depth of 3 m, sulfate reduction rates are almost not measurable indicating the lack of a suitable electron donor.” We think that methanogenesis rate measurements would not help to draw more precise conclusions. First, as long as sulfate is available, methanogens cannot compete with sulfate reducers as long as they do not use alternative substrates such as methylated compounds. Second, there was no methane detected along the depth profile of core JS-B.

R1: 5479. heading 4.2. The heading doesn’t fit the contents of this section very well. The third and forth paragraph discuss a very different subject. Spores are introduced, but no data are shown here. This discussion should not be in this manuscript, if the data aren’t even shown.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



Reply: As already stated, we have transferred the paragraph concerning the spiking experiment to the Results section. We also have shortened the discussion about the role of spores.

R1: 5482, section 4.3 I suggest to omit this section and to focus on the sedimentological effect on biogeochemistry and microbial composition instead.

Reply: We have shortened this section to make it less predominant. Nevertheless, we think it is not only important for scientists working on coastal sediments, but especially for those who are interested in deep subseafloor sediments.

---

Interactive comment on Biogeosciences Discuss., 7, 5463, 2010.

**BGD**

7, C3669–C3679, 2010

---

Interactive  
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

