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Biogeosciences Discussions

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

Anonymous Referee #1

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Beck et al. On 'Paleo-environmental imprint on microbiology and biogeochemistry of coastal Quaternary sediments

General comments:

This is a very data-rich manuscript, which describes the microbiology and biogeochemistry of two (18 m and 19 m long) percussion cores drilled in the German Wadden Sea, a large intertidal system on the southern North Sea coast. 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, but it doesn't take away from the 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 chem-

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Interactive Discussion

Discussion Paper

istry. 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. The sedimentology of the cores was described in detail, and accompanied by a broad set of porewater and solid-phase chemical data. The microbiology was quantified by using Q-PCR of functional genes in the sulfur and methane cycle, and further quantified using radiotracer incubation methods to determine anaerobic methane oxidation rates and sulfate reduction rates (14C and 35S). To top it off, electron microscopy was used to analyze mineral shapes in order to infer their origin. Clearly, the presented data are very diverse, but the overall data quality seems very good. The methods are, for the most part, well described and clear. The data ultimately deserve publication, but 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. There are some real highlights in this study, but these are buried underneath a whole set of accompanying data that are only marginally important for the overall interpretation. The highlights do not receive the space they deserve in the discussion, e.g., the nice fit between the Q-PCR data and the 14C-methane oxidation rates, the 35S-sulfate reduction rates, or the high concentrations of CRS in some layers. I had the impression that the work was guided by the drive to measure what could be measured with available methods by the participating groups, and the hope that, if put together, some picture would emerge. This is a completely inductive approach with all the flaws that come from imprecise working hypotheses. 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 understand 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. So the title, in a way, is misleading since there is not much palaeo-environmental imprint. So why then this title? What is

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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, and because the changes in sealevel between glacial and interglacials were so significant, the sediments were deposited under a range of different conditions - fluvial, salt marsh, intertidal low and high energy, which all have the potential to change biogeochemical processes and the microbial community composition fundamentally. 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 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. Despite the abundance of data, one important piece of information, rates of methanogenesis, were not measured. This is very unfortunate, because large sections of the drilled cores are sulfate-free so that methanogenesis as the dominant indicator of anaerobic carbon degradation, is expected to occur here. This is even more important because deeper parts f the cores were deposited under non-marine conditions, where methanogenesis should have been more prominent. At least, porewater modeling of the gas concentrations should have been to determine methanogenesis rates.

Specific comments:

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

Line 9/10: Please check the English

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).

5466, l.4: Methanogenesis is not a carbon mineralization pathway, the end product is not a mineral ion (carbonate), but an organic molecule, methane. 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. l. 23. The word 'despite' implies a contrast. Where is the contrast here?

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.

5468, l.12. How soon were the CH4 samples taken after core retrieval? 5469. l.15. Visual inspection. What do you mean, under the microscope, on core halves? Specify. Isn't the outside of the core smeared?

5471, l.5. Why should the mcrA only be indicative of anaerobic methane oxidation? Why not of methanogenesis?

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

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

5472, l.11, replace 'columns' with 'tubes'

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 questionnable. The steady state assumption violates the

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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.

5473, l.9. Avoid repetitions.

5473, l.18. replace 'between' with 'from'

5475, l.22-6576, l.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?

5476, l.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.

5477. l.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 section 5478, line 15-19 is very short.

5478, l.12 'higher'. Shouldn't this be 'lower'? Which particular section do you refer to? The SRR vary significantly.

5479, l.8-l.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.

5479, l.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'?

5479, l.16 correct to 'a relationship between lithology, microbial abundance, and activity

. . .'

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?

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

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.

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

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