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Discussions

Interactive comment on "Limitations of microbial hydrocarbon degradation at the Amon Mud Volcano (Nile Deep Sea Fan)" *by* J. Felden et al.

Anonymous Referee #2

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Review of "Limitations of microbial hydrocarbon degradation at the Amon Mud Volcano (Nile Deep Sea Fan). J. Felden et al., Biogeosciences

This study is centered on resolving the question of why the activity of anaerobic hydrocarbon degraders in the mud volcano center is low, despite high energy supplies, i.e. the availability of electron donors (methane) and acceptors (sulfate). Through describing the biogeochemical setting in detail, the authors pose several hypotheses, and test them with the available data and kinetic considerations. They eventually conclude that the recent heat and mud expulsion was too soon for the AOM microorganisms to colonize and reflect their biogeochemical potential. On the other hand, the authors conclude that in the periphery of the mud volcano center hydrocarbon transport limits the biogeochemical AOM activity.

There are two main positive aspects of this work: First, the data are of utmost quality and are from a newly explored environment. The dynamics of this type of seafloor environments are crucial to understand how the seafloor-ocean interface regulates fluxes of key greenhouse gases, such as methane. Second: The discussed hypotheses are interesting and the authors satisfactorily substantiate their choice of one of these hypotheses. Thus, this well-written biogeochemical account of a mud volcano environment deserves publishing after minor-moderate revision.

Still, a few improvements are possible. I am aware that the study of short-term (days-weeks) dynamics may not always be possible in deep-seafloor studies. However, I am not sure about the changes between 2006-2009 are due to some long-term dynamics. Not much can be done in this respect since the authors present no data, but local spatial variation and some possible short-term temporal changes need to be discussed. At least, the comparisons between 2006 and 2009 should be done in a most conservative way.

Below are some more specific suggestions referring to location in the MS:

P337, L4 I think it's more accurate to say "sulphide oxidation is used as energy source".

P337, L10 This expression is pretty strong. Does AOM always control methane emission "wherever sulphate and methane meet"? Actually this study itself is a good example of the fact that just he co-existence of sulfate and methane is not sufficient for AOM to control the fluxes.

P337, L15. Eight references to prove one point is too many, please just cite the most relevant works.

P344, L18. "Height" to be corrected as "heigth"

P347, L7. AOM decreases the pH, but why should the pH decrease downcore only show the absence of AOM? This sentence needs to be revised.

P348, L5-15. This experiment is very interesting but I think this paragraph should

be moved to section 4.1, where different hypotheses are discussed, perhaps under a heading such as “inhibiting substance in the fluids”.

P350, 4.1.1. It seems like the biogenic muds are low in methane and sulfide, and oxygen as well. Is this observation close to a mud shrimp burrow? Maybe due to the mud shrimp activity this area is rich in oxidized forms of metals, such as iron(III) minerals, which can in turn titrate any sulfide fluxing from deep. Even though the sediment was not black does not mean that there is no FeS or metal sulfide formation. Actually nowhere in the manuscript the effects of metals on mud volcano biogeochemical processes are discussed. Are mud volcanoes low in metals? If not, their presence would significantly affect sulfide concentrations, among other things. This point needs to be elaborated and the spatial heterogeneity in this zone needs to be taken into account.

P353, L8. The issue of temporal change between 2006 to 2009. The authors here and elsewhere in the manuscript seem to imply that this environment changes over the scales of years. My feeling is that shorter-term changes are also possible, due to sudden changes in subsurface conditions and other related factors. Hence the change observed may be a short-term pulse. Also, spatial changes within each “zone” needs to be accounted for. Each zone has its peculiar heterogeneity, in one zone this is driven by heterogeneous outflow of seep fluid, in another by the mud shrimp burrows. I suggest that at the end of discussion part the authors need to elaborate on short-term temporal variation and spatial heterogeneity – and put the observed changes in 2006 and 2009 in this context.

Interactive comment on Biogeosciences Discuss., 10, 335, 2013.