Biogeosciences Discuss., 7, C1125–C1129, 2010
www.biogeosciences-discuss.net/7/C1125/2010/

Biogeosciences
Discussions

Interactive comment on "Methane oxidation in permeable sediments at hydrocarbon seeps in the Santa Barbara Channel, California" *by* T. Treude and W. Ziebis

H. Roy (Referee)

hans.roy@biology.au.dk

Received and published: 26 May 2010

General comments:

The manuscript by Treude and Ziebis address methane seeping and oxidation in a challenging and dynamic environment. Thus, any useful information extracted will be a valuable addition to our understanding of shallow marine gas seeps. Unfortunately the main conclusion from the data is that the target process was missed because the sediment was cored too shallow. The manuscript does have an interesting discussion on why the methane oxidation did not occur at the expected rates in the investigated surface layers. But the focus is placed on assumed high rates below the investigated

layers. This gives the manuscript an unfortunate character of listing stray data with little direct link to the conclusions.

The manuscript span the ecology of anaerobic and aerobic methane oxidizers, the prevailing transport properties of coastal sands and the geochemical imprint that methane oxidation leave under those conditions. It would have strengthened the study if the dynamic solute transport had been considered during the measurements of ephemeral compounds such as methane and oxygen. The impact of the transition from advective transport in situ to stagnating condition during measurements is not discussed and the details about the condition during incubation (MOX) and measurement (oxygen) is not given so that the reader can asses it independently. The large drop in oxygen concentration in the overlying water during profiling indicates that there is a problem concerning depletion. Thus, it is also hard to infer the transport properties from the shape of the oxygen profile.

A substantial part of the data is presented as 2D plots while the authors discuss that data as individual vertical profiles. Thus, it would be better to present the data as such. That would also resolve a number of technical issues concerning gridding and extrapolation.

The significance of aerobic methane oxidation and absence of ANME clusters in the surface layer is similar to the methane seeps in the Wadden Sea. The sediment properties at the two sites is also comparable. Thus, the conclusions of the present study can be supported by comparison to the studies of the methane cycle in the sand flats of the German Wadden Sea (http://www.watt.icbm.de/).

Specific comments:

P. 1907 L. 26-29: The data do not indicate the deep AOM, that's the authors' assumption.

P. 1909. Information on agitation of the water in the cores during recording of the micro profiles is missing. So is the time span from coring by the diver until measurement. This is critical because the driving force behind advective mixing is later discussed based on the profiles. It is well known that a core form an advection dominated environment will change dramatically within minutes when removed from the seabed (e.g. MEPS 145 63-75). Especially in a seep enviroment (e.g. Limnol. Oceanogr., 50(1), 2005, 113–127). The oxygen concentration apparently changed rapidly in the overlying water prior to profiling. It must have changed even faster in the pore water.

P. 1910 L. 25: Give time span from sampling the core until the sediment sample was fixed and sealed.

P. 1910 L. 18-22: What was the content of organic carbon and fine fraction? Those are critical for the permeability. The permeability of the sand bed is a key issue and I am surprised to find its determination based on a correlation to size fractions without further discussion (still I will not be to surprised if the correlation will work better than direct measurements due to the challenging sampling of sandy cores).

P. 1911 L. 15-17: How much pore water was extracted, in mL and in % of the total pore water volume?

P. 1913 L. 14: An alternative explanation is that the core was messed up during recovery. Did the core have time to recover? Note the catch 22. If you measured immediately (within a few minutes) you must prove that you did not disturb the cores. If you measured later you must have provided a realistic advective regime including the source of electron donors from below.

P. 1913 L. 16: This can only be true if profiling was started on the first core within minutes after the diver capped the core. Was that the case? The oxygen depletion in the water column should be used to calculate a minimum flux to see if the explanation is plausible.

P. 1914 L. 3: Or the cores have now been standing long enough for the pore water to

be depleted for oxygen.

P. 1915 L. 9: The data is not gridded by nearest neighbor. Looks more like a "Triangulation with linear interpolation", which is, by the way, the correct method. The data should be blanked outside the measured points. This can be done with blanking files (described in Surfer help) or with photoshop. Use the same distance between isolines and the same color map for all plots of the same parameter. Otherwise it is impassible to compare patterns by eyeballing.

P. 1915 L. 15: This appears to be an artifact from the gridding. The peak is a gost of the 0-cm-N produced by extrapolation.

P. 1916 L. 8-11: The pattern will look much different when extrapolated data (the lower left corner) is removed.

P. 1916 L. 25: The permeability was not measured. Is the method used able to pick up those changes?

P. 1919 L. 1-10: The counting statistics is not provided (number of counted samples, number of counted aggregates). The S.D is not a useful description of the goodness of the estimate in a counted number, especially if the number is small. It is also misleading to place S.D. as a symmetrical error bar on a counted number. It would be elegant and easy to give confidence intervals on the numbers assuming Poisson distribution.

Figure 1: The order of the sub-plots does not help the reader grasp the figure and it is hard to read. I suggest to have the 3 columns of graphs corresponding to the 3 sites and leave blank areas for the missing graphs so that he reader can run a finger along a line and se the progressive change in one parameter. That appears to be the original idea but it did not work for me in the present form. The scaling of panels is different and this hides the differences between the plots (e.g A versus J).

Technical corrections:

P. 1907 L. 2 and 5: Use compatible units so that the numbers can be compared.

P. 1908 L. 23-24: Naming; avoid using “A” for both a transect and a vent.

P. 1909 L. 18: At what pH, how was pH controlled (buffered) and measured and what pKa was assumed?

P. 1909 L. 22: Move the instrument description up to the sensor description in line 10. The PA 2000 is not especially high sensitivity.

P. 1911 L. 1-5: Hard to follow.

P. 1912 5: ... aliquots of 1.5 kBq...

P. 1912 L. 10: or 20%

P. 1913 L. 14: Reword, the fluids are not transported by diffusion.

P. 1914 L. 4: The sulfate minimum is a single measurement. Most likely an outlier.

P. 1914 L. 16: In conclusion; this site is more stratified.

P. 1914 L. 19: Take the in situ pressure into account when discussing the methane concentration relative to saturation.

P. 1419 L. 21: I don’t see this that clear. It is supported by MOX, but isn’t that driven by the high CH4?

P. 1915 L. 20: $-43‰$ and $-54‰$ appears to be within the same error bar.

P. 1915 L. 24: The peak at 20 cm is a single point that sticks out on one profile.

P. 1916 L. 3: Again a single data point that sticks out.

P. 1920 L. 11-13: Rephrase.

P. 1920 L. 22-25: Redundant.

P.1921 L. 16-20: Sentence applies to vents, not to deep sea in general it is written.

Interactive comment on Biogeosciences Discuss., 7, 1905, 2010.