2nd Review "Annual Variability and regulation of methane in sulphate fluxes in Baltic Sea estuarine sediments." by Joanna E. Sawicka and Volker Bruechert

Major comments:

Some of my previous comments have been answered in the response letter, but no appropriate changes have been made in the manuscript reflecting this answer (e.g. line 159: It still lists 2 hours drying time even thought the author answered that the actual drying time was longer; line 168: still says "exactly 2.5 mL" even though the author state it was changed; Line 170: the authors still do not cite a reference for fixation of the samples with 5M NaCl, even though they say it is a widely used method; Line 171: The authors agree that an hour is very likely does not get all the adsorbed methane. In the next answer, however, they state that the hour is actually sufficient and instead of clarifying that, the authors delete the information altogether and only state it in the response letter; Line 267: answering my comment, the authors state that they did some replication on methane profiles, but do not say anything about the fact or the results in the manuscript; Line 354: yes, there is the possibility of a cryptic sulphur cycle. But there is not a good reason to expect this cycle not to go on above. Thus, if you keep this information in there, you need to discuss it in more detail and add the information to the legend of the graph; line 367: please discuss the effect of the profile resolution on the modelling results…)

Unfortunately, the manuscript still in not organized all the way through with the same order for the stations in the results (e.g. line 300-304). Also, sometimes it seems that the authors are discussing a different data set (e.g. line 307: the graph shows the lowest sulphate penetration and steepest gradient in August; Line 318: Graph shows 16, 14, and 10cm (8cm in Figure 3); line 338: there is no sulphate in August below 10 or 8 cm, depending on the graph, so there can not be a peak in sulphate reduction, only in potential sulphate reduction)

The rates in table 2 are sometime negative and some positive. At the same time the direction they are described in change. A negative TOU means that there is oxygen diffusing out of the sediment. A positive CH4 flux out of the sediment that there is methane diffusing out of the sediment, a negative SO4 into the sediment means that there is sulphate diffusing out of the sediment. Please correct as mentioned before.

Line 486: you say that iron and manganese reducers in your sediment outcompete sulphate reducers (in your response, but again not in the manuscript, where you also do not mention that usually they do not). You cite Downs and Bruchert Goldschmidt 2013 and Bonaglia etal. 2014 Biogeochemistry (which both do not say anything about sulphate reduction). What do you base your statement on?

Comment to previous comment line 319: a 100-500um oxygen gradient from bottom water to 0 gives a high flux, which could be coupled to methane oxidation. Additionally, bioturbation can provide oxygen even below this depth.

Comment to previous comment line 465: Line 503 does not state what you mention in the answer, and I cannot find it. Additionally, it would be much better to get some numbers instead of just “fit well”.

Comment to previous comment line 497-503: The authors agree that if it is hydrostatic than it is not seasonal. Additionally, they only have the data for one year, so it is hard to say which of the changes are clearly seasonal and which just variations over time. But they still argue for a seasonal signal.

Comment to previous comment line 519-520: You data does not “indicate that hydrostatic changes or changes in pore water advection may have a considerable influence”. Your data indicates that the parameters you measured do not alone control the variations and you discuss, without presenting data or modelling, that it is hydrostatic changes and pore water advection.

Minor comments

Introduction

Line 60: remove “potentially”

Line 66: In the water methane can not be produced in the sediment, please rephrase.

Materials and Methods:

Line 113 and 423: you state that 23% of the freshwater is from a river, but also that there is no important river entering the system.

Line 134: where are the April 2013 data?

Line 155: what depth are the corg data from

Line 156/7 delete one mention of “freeze dried sediment”

Line 168: delete “exactly”

Line 170: now it sounds you measure the sample immediately after injection of the brine.

Line 218: switch the order

Line 242/3: move “without headspace”, as now it sounds that the “50ul of 50% ZnCl2” did not have a a headspace.

Line 255: which salinity did you choose the ß for?

Results

Line 283: “sediment organic carbon”

Line 300ff: what is the trend in B1 and how is February B1 an exception to the H6 trend?

Line 307: the steepest gradient in the graph is shown in August.

Line 314: what is the trend in April and February?

Line 318: check the depths.

Line 322-34: state that August is much lower than April and October

Line 335: Keep the order the same as in the previous paragraph.

Line 338: Please state how you define the sulphate penetration depth as otherwise your rates below it do not make any sense.

Line 338: use the abbreviations you introduced e.g. SRR

Line 352: no change in sulphate concentrations argues for nor organoclastic just as much as for no AOM sulphate reduction.

Line 367: indicate which direction the methane flux is going.

Line 374: It is hard to discuss seasonal variation with 4 data points from one year.

Discussion

Line 383-391: Please link the statements directly to your data.

Line 397: there can also be imitations that occur at different temperatures, might be indirect though increased rates and thus increased competition, that can influence it. It does not only have to be a direct temperature effect.

Line 402-407: the argument is not convincing. For such small temperature differences, you do not expect to see changes in the membrane composition related to changes in temperature adaptation. And also closely related microbes can be psychrophiles and mesophiles and thus have a very different temperature

adaptation. Your data indicate that there is not such a tight coupling between sulphate reduction and methanogenesis.

Line 426: the percentage likely changes with season with resuspension being highest in fall, fresh organic matter being highest after a bloom and such. Thus arguing with general numbers for the whole year does not do justice to a potential effect of seasonality on the organic matter input. Also, your indicated bioturbation depth can supply fresh organic matter to deeper sediment depth, thus influencing rates not only on the surface.

Line 460 – 461: Please provide reference or calculation.

Line 485 – 488: that is not really what you see. The SRR below 10cm is lower than in other times, and thus, the effect on methanogenesis should be lower.

Line 491-494: First you argue that winter means higher bioturbation and higher (advective) methane flux, than you argue that winter means lower flux and concentration.

Line 517: What SRR can you sustain by the differences in the two CH4 fluxes? How does it compare to your rates?

Line 531: you ignore the second peak with this formular.

Line 542-544: Can you show the data.

Line 588-589: based on what? Please model to back up.

Tables

Table 2: how did you determine the AOM zone?

Figures

Map: the map still is not very helpful if plotted in black and white

Figures 2. 3. 5: Please delete the repeat mentioning of the months in the middle.

Figure 2: please provide legend showing which is CH4 and which is SO4. Change CH4 scale in (h) so the trend is more clear.

Figure 3: (a) and (b) seem to show a second peak around 8cm.