

Interactive comment on “Microbial methanogenesis in the sulfate-reducing zone in sediments from Eckernförde Bay, SW Baltic Sea” by Johanna Maltby et al.

Anonymous Referee #2

Received and published: 15 June 2017

The work presented by Maltby et al. is really nice piece of study gathering results from several impressive campaign of sampling and involving different cutting-edge methods. Their findings give an interesting overview of biological processes and environmental factors controlling methane emissions from sediments and water column of a Baltic sea bay, well known for its importance in global methane emissions. The originality of their work lies in the demonstration of co-occurrence of sulphate reduction and methanogenesis in surface sediments. This co-existence is permitted by a mechanism developed by some methanogenesis microorganisms to escape from the strong competition with sulphate-reducing microorganisms: using (releasing? I did not find information on that) non-competitive substrates. The manuscript is overall well written except the abstract,

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see my comments below. I have only two main concerns.

First, the article is sometime written in a way that only initiates of the field may touch. The first sentence of abstract directly starts with the work done without putting the study in a wider context. The object you study is complex and well structured. We do not immediately understand the relevance of studying methanogenesis in the sulphate-reducing zone. We neither understand that you studied surface, deep sediment and the water column and not only surface (sulphate-reducing) sediment. The reference of “a non-competitive substrate” is not understandable. Which competition do you refer? Implying which organisms? In the introduction, it could be useful to build a synthetic figure summarizing the studied ecosystem including the different compartments, different organisms, interactions among organisms, exchanges of matters between these compartments.

Second, the (minor) contribution of surface methanogenesis to total methane emissions from this ecosystem is a bit hidden in the article. This contribution deserves to be clearly presented in the abstract. To my point of view, the minor contribution of this mechanism does not question the quality and relevance of this study, and is an important information. In the same vein, the statement that surface methanogenesis could play a key role in fueling the surface anaerobic oxidation of methane is speculative since this last process was not measured in the study.

Specific comments.

Line 30 suppress “in the manipulated experiments. L31-33 this new objective that pops up too late. Please gather your objective in one sentence L47 replace “makes an important contribute” by “substantially contributes to”. I did not understand the last part “as it could...” L78-79 and throughout the manuscript. The expression “Environmental control mechanisms” is a bit elusive. Do you mean “environmental controls” or “biological processes”? Try to better specify what should be better studied. L164 Rewrite your sentence to clarify. Could be “Biological activities of samples were stopped by

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the addition of mercury chloride solution..." L177 "extracted"? you mean "sampled" for analysis? L193-216 I am not expert in measurement of methane concentration in sediment, but I am wondering whether the fact of cutting sediment core in 1 cm sediment interval could release, at least a part of, the methane you wish to quantify. L236 Could you rapidly explain again what is the hydrogenotrophic methanogenesis? And what is the interest of measuring this in the context of your study? L389 Suppress "in" before september. L401 I guess you're talking about the C/N of particulate organic matter, but I am not sure. Please specify. L543 I propose you to replace the end of your sentence by "...the following observations that will be discussed in more detail in the following chapters". My first reaction was to try to understand your arguments before reading the following chapters. L569 Your explanations about the competition between sulphate-reducing and methanogenesis microorganisms, and the strategy of methylotrophic methanogens to escape from this competition, are very clear and convincing. Now I am wondering whether there is competition between hydrogenotrophic and methylotrophic microorganisms. And if yes, does this competition change with depth? L587-592 This sentence is too long. Split your explanations into 2 sentences. L614 If I follow well, you should add a "P" after "DMS". L605-631 Maybe this is a limit of your study of not having quantified some key non-competitive substrates in sediments and water. It could be discussed in a paragraph drawing next investigations that could be done. L640 What fractionation are you discussing? An isotopic fractionation? You must better explain. L488-489 Could you check whether such moderate isotopic fractionation (factor of 1.07-1.08) could explain an increase of delta of almost 200 per mille. I have a doubt. L644-645 This sentence is not clear. What would be the alternative explanation? L646 One bracket is lacking at the end of sentence. L684-694 I did not understand your explanations. Please try to reformulate and be more direct when you propose an interpretation. L706-707 Did you find results going in this way as well? L713-736 This section is really too long. Split it in two paragraphs, one focusing on the effect of POC amount and the other on C/N ratio. L830-831 I do not understand your interpretation of the positive correlation between surface methanogenesis and C/N ratio

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of POM. L805-809 and 832-834. This process of anaerobic consumption of methane (AOM) was not measured in this study making all these discussions around the key role of surface methanogenesis in fuelling AOM very speculative. I do not understand why deep methanogenesis, which contributes for the major part of methane emissions, does not contribute to AOM fueling. It sounds like you would absolutely like to give a central importance to surface methanogenesis.

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2017-36>, 2017.

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