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Biogeosciences Discussions

Interactive comment on "Methane and sulfate dynamics in sediments from mangrove-dominated tropical coastal lagoons, Yucatán, Mexico" *by* P.-C. Chuang et al.

Anonymous Referee #1

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«Overall comments» In general, I feel positive about the overall contribution of the paper. The topic is interesting and relevant to the goal of Biogeosciences. The approaches that the authors adopted are interdisciplinary and provide educative information to this topic. The data and interpretation are mostly convincing with several points that I request for further clarification (see comments below). The authors need to improve the presentation a lot as figure 2 is hard to read, some of the references cited are out of date, a few sentences are quite awkward to read, and Table 2 needs more polishing. I've included my detail suggestions for these technical issues in the pdf file. Despite these minor flaws, I strongly encourage to publish this paper after all of my concerns are addressed.

«Detail comments» My major concerns about the paper are as follow:for analytical and model approach-

1) In quite a few of samples, the sulfate concentrations are over seawater value (28mM). The authors explained this as dissolution of anhydrite. The alternative explanation will be re-oxidation of hydrogen sulfide in the porewater samples after they were collected. In the sampling procedure the authors described, I do not see any description such as flushing the porewater samples with N2 gas or fixing sulfide with Zn(OAc)2 solution to get rid of sulfide. Some clarification about how this is of concern should be addressed.

2) The authors modeled the system for 1 Myr to reach steady state. I wonder if this is a reasonable assumption to make in this case? From the high sedimentation rate (0.25-0.35 cm/yr) of these cores, the age of the sediments investigated is not older than several years. Besides, this environment must be very dynamic with episodic input of water from different sources, bioturbation, and even sediment reworking. Why not simulate the system only to their real age, say 1-5 years? I believe this will significantly impact the results.

3) I find it difficult to understand the reactions described in the appendix: a. Page17931, line20-22: "Since AOM may play a minor role in the methane and sulfate rich sediment and RAOM was included in the net reaction rates of methane and sulfate this is justified." I don't understand at all what does this sentence mean. AOM should play an important role when you have abundant methane and sulfate isn't? What is justified? By what? b. Eq. A6: so you exclude entirely AOM when SO4-dep is positive? I thought SO4-dep>0 means active removal of sulfate? Not by AOM? d. Eq. A7: I understand you related Rpoc to Rsr assuming all sulfate reduction is organoclastically. Again, is this a good assumption? What's the role of AOM in sulfate reduction? I think you are right that organoclastic SR is important here but you need to explain this better. e. Eq. A12: How does Rcorganic different from Rpoc? How does the comparison of these two rates like? From table 2, I see them can be orders of magnitude different (e.g.

1CH_Dec00). Why?

4) Refer back to my comment (2), time scale of your model is really important. It determines the scale of your kinetic constants. For example, you use 0.01 1/yr for your kcorg. It may be a lot different if you only run the model for 5 years and.

for scientific interpretation/discussion- I think the experiment and model results support most of the interpretation by the authors. I however feel that the authors should extend the discussion a bit more from the following prospects:

1) Maybe my biggest concern for the paper is the assumption of steady state. The authors should provide good reasons why they think this assumption is adequate as the system is so dynamic.

2) The authors presented tremendous amount of temporal/spatial porewater data in this paper but did not spend much effort in discussing these. The groping of data is based on the shape of profiles and thus their dominate reactions. Do these groups correspond to any particular location or season that might explain the such dominance in terms of biogeochemistry?

3) Results from incubation experiments are one of the highlights in this paper but the authors only mentioned it briefly in 5.1 section. I wonder are the authors able to derive some rates from the experiments that can be compared with the rates estimated by modeling. Also, how do all these rates compared to other similar environments? I feel like the authors should put their results in a larger global context to reveal the significance of their data.

4) The authors introduced the different seasons of this area and the potential impact to the sediment and porewater systems. However, I do not see further discussion about how their results reflect such seasonality. I feel a great pity that the authors did not translate the "numbers" they got from their modeling and experiments into something helpful to understand the spatial and temporal heterogeneity of the environment.

«Minor/technical comments» 1) My biggest comments on the technical part of the paper is its presentation. The lead author tend to use long sentences with many clauses. I would suggest split the long sentences into shorter ones which will be more understandable for readers who know nothing about modeling especially.

2) The authors also need to consider more recent literatures. When the hypothesis was built solely based on some 80' and 90' papers, it's hard not to think there may be different views in the current research.

3) The Figure 2 is small and difficult to read. You need to figure out a different way to present these.

4) I have a few comments for Table 2. You need to be more careful about the significant digits. I don't think the model can give that many meaningful digits. The use of "F" at header row is confusing. I know you explain below but it is intuitionally awkward especially when you mixed the real fluxes with depth-integrated rates. The negative sulfate depletion rates and sulfate reduction rates are also awkward. It makes no physical sense unless you meant the reactions are reversible, which I think are not.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/12/C8308/2015/bgd-12-C8308-2015-supplement.pdf

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