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

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Review of: Methane and sulfate dynamics in sediments from mangrove-dominated tropical coastal lagoons, Yucatan, Mexico

By: P-C Chaung et al.

General Comments: The authors present sulfate, methane and chloride data from sediment cores collected from two coastal mangrove systems in the Yucatan Peninsula. The authors group the cores into 5 sets that generalize the sulfate and methane profile behavior. Because the analytical data are limited to concentration profiles of 3

constituents, they apply the Wallman et al. 2006 transport-reaction model to explain potential processes affecting the pore water geochemistry. An unusual and interesting observation is that methane and sulfate often coexist in the porewater, suggesting a non-competitive substrate (i.e., one used only by methanogens) allows methanogens to be active in the presence of sulfate reducers. A series of incubations that includes a treatment with the non-competitive substrates TMA and methanol demonstrates the microbial machinery and other factors required to produce methane from these substrates is present in the sediments from the investigated sites. The suggested implication is that mangrove ecosystems may be large methane emitters, provided the observations and model results accurately represent mangrove systems at large.

Although the diversity of data is limited and in some cases the conclusions are speculative (see below), the authors do a commendable job of testing the hypothesis that non-competitive substrates accounted for the accumulation of methane in the sulfate reduction zone. The study does not provide definitive evidence that the process is active, as the only data directly supporting its activity are from ex situ incubation experiments. The study should be used as motivation for tackling this specific question in greater detail in a mangrove ecosystem. It would appear others have observed the same effect in mangroves, but this appears to be the first to suggest a mechanism for the repeated observation. This is an important and interesting contribution. With moderate revisions, this reviewer recommends publication of this manuscript in Biogeosciences.

Specific Comments:

1. The grouping of the profiles helps to consolidate the data in a way that makes the application of the model more systematic. However, the authors have a tendency to overstate the certainty of their findings. For example, the model does not "illustrate" that methane is produced from DOM. . .it suggests production from these unmeasured carbon sources is possible. Also, shallow methane production does not necessarily promote high methane fluxes to the water column and atmosphere as the authors

state. Although benign in intent, these statements being expressed definitely in the abstract may be misleading because they imply the conclusions are based on data. Be clear that the conclusion are based on modeling results and that no measurements regarding fluxes were obtained.

This reviewer recommends the authors provide a figure with generalized sulfate profiles (and methane, if applicable) for each group in Fig 2. Such a model (and a description of each group in the Fig 2 headings) would give the reader a better intuitive sense for the groupings.

2. Why would mangroves have such a high abundance of non-competitive substrates in comparison to other brackish systems?

3. Using the near surface methane gradients and modeled results, the authors should quantify the differing methane flux potentials for each environment rather than speculating about the importance of this methane source.

4. The site description should include a description of where and why anhydrite might contribute excess sulfate. An alternate possibility not discussed is oxidation of sulfides. Total sulfides were not measured, so their potential contribution cannot be discussed. Perry and others have written much about why anhydrites and gypsum are found on the Yucatan platform. More details would make this argument more convincing.

5. Were the sediments dried and prepared for TOC analysis as part of this study, or Gonneea et al., 2004? The methods do not include the analysis. The results do not specify the origin of the data. Please clarify.

6. Increasing OM content with depth? How is this? Suggestion of a changed depositional pattern not discussed.

7. Why would negative sulfate depletion be observed at the surface and not at depth if the source of the excess sulfate is from depth? See Core 7CH-Oct01. Also, in cases when the sulfate/chloride ratio is high is there reason to expect that fluids coming from

an evaporitic deposit would have a higher overall salinity? The evidence for contributions from anhydrite are not especially compelling. Basically, the authors state that there is anhydrite in the area, so that explains the excess sulfate. From looking at one of the Perry references, it is not clear that one would expect a groundwater contribution in the Chelem lagoon (inside the Chicxulub impact zone). More details would be helpful. Sr data would be even better, but that is not likely to be available and is not required.

Technical Corrections:

17921, line 21: ‘porewater’

17923, line 7: delete ‘that’

17923, line 12: ‘sites’

17927, line 5: ‘inhibited’

17930, line 3: ‘atmosphere’

17920, line 20: ‘chloride’

Figures:

1. Put letters on the figure panels

2. Some units on figures indecipherable (CH4 conc)

Interactive comment on Biogeosciences Discuss., 12, 17913, 2015.