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Biogeosciences Discussions

Interactive comment on "Quantifying in-situ gas hydrates at active seep sites in the eastern Black Sea using pressure coring technique" *by* K. Heeschen et al.

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Review of: Quantifying in-situ gas hydrates at active seep sites in the eastern Black Sea using pressure coring technique

By: Heeschen et al.

Reviewed by: John W Pohlman, US Geological Survey

Methane and chloride data were obtained from seeps (Batumi and Pechori Mound) in the eastern Black Sea using samples collected with a pressurized sediment core. Methane volumes obtained during degassing experiments were used to calculate gas

hydrate saturation. Gas hydrate saturation was also calculated using low chloride anomalies, but values using this technique were lower, presumably because of recent gas hydrate formation. High chloride brines offset the low-chloride values used to calculate gas hydrate saturation. Using a steady state model, it is not possible to discern the offset caused by brine formation. Thus, the authors conclude pressure cores provide more accurate hydrate saturation estimates. To my knowledge, this is the first study to directly compare hydrate saturations calculated by degassing and chloride anomalies that show significant differences between the two. For that reason alone this manuscript merits publication, with some revisions.

A great deal of effort and space were expended to obtain the in situ chloride profile. I do not question the approach of the model or its output, but Haeckel et al have essentially described this model in their 2004 manuscript. I do recognize they have added terms for AOM and sulfate, but I don't think it is necessary to go through the entire model description again since it is already published. Describe the differences and then take the figures from the appendix (particularly A1) and put it in the paper. Panel A is really interesting and should not be stuffed into an appendix most readers will not look into.

Section 4.3 is sort of interesting –at least the changing ratios of the gases during the experiment, but those data are not a serious point of the discussion. Most of the discussion in that section goes toward the gas source characterization, which is based entirely on Bernard Ratios (not Factor!). While I do not disagree with the conclusion, without isotopes any source interpretation is sketchy. Plus, Reitz et al have already sourced the gas at this site (Reitz et al., GCA 2011). Haeckel is a co-author on that one, so he knows about this. Do something interesting with the degassing ratios. Do not characterize a characterized gas source.

Bascially, I really like the hydrate saturation comparison part of the paper and think showing the pressure cores give more accurate results than chloride anomalies is a valuable contribution. How the in situ profile was modeled is also interesting, but the redundancy of the model description from Haeckel et al (2004) is not necessary. De-

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scribe the modifications and bring the figures from the appendix into the body. Also, the gas source characterization has to go since it has already been published. While these changes will change the structure of the manuscript they will not change its value. Perhaps it will make it more poignant.

Specific Comments: I have made numerous comments and editorial suggestions on the pdf I attached to this review.

Thank you for allowing me to review this interesting manuscript. I am happy to review the revision or I look forward to seeing the final published form.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/8/C2259/2011/bgd-8-C2259-2011-supplement.pdf

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Interactive comment on Biogeosciences Discuss., 8, 4529, 2011.