Biogeosciences Discuss., 10, C8019–C8023, 2014
www.biogeosciences-discuss.net/10/C8019/2014/

Biogeosciences
Discussions
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Interactive comment on "Pumping methane out of aquatic sediments – forcing mechanisms that affect the temporal dynamics of ebullition" *by* A. Maeck et al.

Anonymous Referee #2

Received and published: 17 January 2014

General comments:

This paper addresses the temporal character and triggers of ebullition from sediments in an impounded river, relevant questions within the scope of BG. The authors collected methane ebullition data using 3 automated bubble traps with frequent sampling and over 5 months in regions of known high sediment accumulation. The paper provides novel quantitative evidence for the role of self-inhibition in ebullition, perhaps due to depletion of the trapped bubble store in the mud. In addition, the authors propose a novel framework for comparing the temporal character of ebullition from different settings. The analysis of how uncertainty in flux estimates grows with shorter measurement periods is clear and illuminating, but the extrapolation to global emission estimates is

unwarranted due to the small sample and its bias. The writing is clear and fluent, but some of the text was inconsistent with the data presented in Table 2. The references are appropriate, and the abstract is concise and complete.

Specific comments:

Title: "Aquatic sediments" should be replaced with something more specific to the study site.

Pg 18694, Ln 4: Have you done anything to estimate loss rates from dissolution of gas trapped in the ABTs? Since you claim to be able to measure ebullition rates within 10%, you should check that dissolution does not significantly increase that number. It would then be appropriate to note how this uncertainty propagates to your flux estimates and conclusions.

Pg 18694, Ln 20: The gas disturbed from the bottom could have a different chemical composition from bubbles released naturally.

Pg 18696, Ln 14: The choice to do logical regression seems interesting. The rationalization that the volume of gas released is not a linear function of the forcing mechanism suggests that you performed a regression analysis on the gas flux data themselves. What were those results? It would be interesting to see which forcings correlate well with the actual fluxes, taking into account their widely-varying magnitudes. For example, what if high-frequency hydrostatic oscillations allow low-level ebullition (still higher than the mean over the measurement period), while low-pass hydrostatic pressure drops trigger large venting events?

Pg 18699, Ln 19: Analysis of synchronized 5-min ebullition events assumes that the forcings also trigger the ebullition within the same 5-minute period. This may be true, but some experiments in mud-analog systems have shown that mobile bubbles may take a few minutes to rise to the surface (Boudreau et al. 2005). What happens to the percentages and regression coefficients when you allow for time lags of 5, 10, or 15 minutes?

Pg 18700, Ln 3-5: The logistic regression can identify impacts on the presence or absence of significant ebullition, but that's not as general as explaining the "observed variability of ebullition rates."

Pg 18700, Ln 5-6: The text suggests that high-frequency hydrostatic pressure variations decrease the probability of high ebullition rates. However, Table 2 shows that 2 of 3 traps gave positive regression coefficients with high frequency pressure fluctuations. The remainder of the quoted paragraph similarly supports a positive correlation between high-frequency fluctuations and higher ebullition rates, though later text contradicts this (see comment on pg 18702 below).

Pg 18700, Ln 19-21: You observed that the site with fastest sedimentation had the highest mean ebullition rate. What were the mean ebullition rates for the 3 traps, and how did they compare with the exponential relationship found in Maeck et al. (2013)?

Pg 18702, Ln 17: This discussion again references negative regression coefficients with high frequency pressure fluctuations and provides a plausible explanation for such results, but the text contradicts Table 2.

Pg 18703, Ln 4-25: What were the water depths where the 3 ABTs were deployed? If shallow sites can generally release bubbles in response to ship wakes, but ABTs 2 and 3 did not vent anything following ship passages, it's important to your explanation to know if 2 and 3 were deeper than 1. I couldn't find those numbers in section 2.2 or Fig 1.

Pg 18705, Ln 1: Varadharajan and Hemond (2012) showed that ebullition was triggered by drops in hydrostatic pressure, not just atmospheric.

Pg 18705, Line 9-29 and Fig 9: The proposed framework to compare and understand regimes of episodicity in ebullition is a valuable contribution of this study, but I wonder if it could be clarified and simplified. The essential distinction supported by the data

is between regimes of external triggering vs. fast internal methanogenesis that makes bubbles large enough to be mobile faster than the trigger can release them. If you were to scale the time axis in the subfigures by the inverse forcing frequency, might not subfigures (1) and (4) have a similar temporal character? Both seem to be driven by external triggers, rather than an internal methane source. In the case of data from this study (1), the production rate and forcing frequency are both higher than in (4), but in both, one would need to sample over a time period much greater than the period between significant trigger events. These two cases seem similar to each other but distinct from (2), which you suggest is driven by its internal production rate because the forcing does not act frequently enough to relieve the buildup of gas bubble volume and buoyancy. While the conceptual example drawn in subfigure (3) seems plausible, it does not constitute evidence supporting this framework. Even if it were correct, I hypothesize that if one scaled the time axis by the inverse forcing frequency and magnified the y-axis to see temporal variability, the temporal character would be similar to that in (1) and (4). This is because such a case would also be externally-triggered, with the frequency of ebullition determined by the forcing frequency and the magnitude scaled with the methane production rate. To distinguish between cases (1,3,4) and (2), the externally-triggered vs. internally-driven regimes, one could simply consider the ratio of timescales of production vs. forcing. When the ratio of (production rate / forcing frequency) is low, one might expect an externally-triggered system, while a high value of that ratio would imply an internally-driven system.

Pg 18707, Ln 6: Given that your heavily-impacted study site is "not representative for all aquatic systems," it is unreasonable to extrapolate your results to the global scale.

Table 2: Are the signs on the coefficients for High frequency pressure fluctuations correct? They do not match the text (see notes above).

Fig 2: How does the uncertainty analysis shown in Fig. 2 impact the general results and conclusions? The asymptotic 4% relative error seems well-constrained, but how does that uncertainty propagate into flux estimates? One interpretation is that the uncertainty associated with short sampling intervals is more important than that from the instrument, but I don't see this stated in the manuscript. If that's the case, do you need a figure devoted to instrumental uncertainty?

Fig. 9: Are the y-axes in the subplots scaled the same, by the methane generation rate, or something else? See note above.

Technical corrections:

Fig 4: Units in subfig (a) should be mmolˆ2 mˆ-4 dˆ-2 Hzˆ-1 = mmolˆ2 mˆ-4 dˆ-2 s, and in (b), Paˆ2 Hzˆ-1 = Paˆ2 s

Fig. 8 could use more distinct line styles to distinguish between the ebullition from different traps (maybe consistent with fig. 5?)

Interactive comment on Biogeosciences Discuss., 10, 18687, 2013.