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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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Dear Jerry,

thank you very much for your interesting comments on our paper and I am very sorry for our late reply.

Comment 1: We'll change/add the Milkov (2004) citation. Thank you for pointing it out.

Regarding the side discussion on overall gas hydrate budgets we agree with you. As mentioned by you there are a number of issues concerning the calculations of gas hydrates from any kind of method, may it be due to the local or areal resolutions or the

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small scale changes in composition, volume or mass of gas hydrates, the occurrence of free gas. You name it. In terms of a publication this discussion would be one of its own, I would think. Out of curiosity: Which method would you choose?

Comment 2 and 3: Thank you very much for your interest in the data and for encouraging us to show even more data. The focus during gas sampling was on the determination of the total gas volumes, whereas to follow the theory you published in 2000 (Dickens et al., Proc. ODP 194, 2000) would have meant to work under stable temperature conditions and most of all in equilibrium. As you already guessed from the data and from your own experience at sea we were not able to work under - or even near - equilibrium given the fact that there is very limited time to degass the cores at sea. We had one night for each of the cores and up to 220 l of gas. The DAPC instrument is more than 2.5 m long and will not fit any lab given the fact that it has to stand upright for degassing. And this leads to the second issue: a stable temperature. Once the device is back on board ($\sim$20°C) a good part of it can be cooled using ice, however, the very top part can not. In addition, the device has only been at the seafloor, i.e. at in situ temperatures (8 – 9°C), for a very short time and the heat capacity of the device is high. To get to the point: we do not exactly know the temperature during the degassing plus it likely changes over time and certainly it does change over the length of the core.

This leads to two conclusions: Equilibrium was not given at any time and thus the degassing characteristics (volume-pressure-time plots) do not hold any information on the distribution between free gas and hydrate bound methane, unfortunately. Showing them might only confuse the reader that has your theory in mind. However, I do like your comment and think it worthwhile to add an explanation into the supplements to why we do not plot all data.

Regarding the threshold pressures, the information we get is very limited as well. Again this is due to the variation in temperatures. At in situ temperature (8-9°C) the threshold pressure, which is a function of pressure, temperature and compositions, should be around 65 bar. Looking at figure S2 it is rather 72 – 75 bar. Given that all factors

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but the temperature are stable, a change of 1-2°C can cause a change in threshold pressure of up to 10 bar. In the Black Sea gas hydrates occur at very shallow depth and thus when sampled they are also present at the top of the core. This area we can not cool properly while the DAPC is on board and with the high heat capacity of the large device it is unlikely that the top of the device will ever reach in situ temperature but rather be slightly warmer. Gas hydrates in this area might decompose at higher pressure compared to the rest of the core and to in situ conditions. Thus, the error in threshold pressures can be rather high and adding them to the paper might only lead to false interpretations.

Best wishes

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