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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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Hello Katja (and colleagues),

I saw the title as I was perusing recent submissions. Obviously, had to read ... and then felt obligated to make some comments.

The amount, distribution and cycling of gas (particularly methane) in the upper few hundred meters of marine sediment presents a wonderfully interesting and challenging problem. Clearly, to fully understand this topic, we need detailed measurements and analyses of pressure cores, which retain the gas. There remain precious few such studies. Ideally, this work can be coupled to the distribution of gas phases within the



sediment.

I like the overall approach in this study, and it's an improvement on most past efforts, where the distribution of gas phases within the pressure core was unconstrained.

In a cursory review, I have three comments, one minor, one intermediate, and one major. I can offer more detailed comments later if you want.

(1 - minor) A paper by Milkov (2005) is cited regarding the global amount and distribution of gas hydrate and problems thereof (p. 4530, 4531). Is this the right reference? I think it might be Milkov (2004) "Global estimates of hydrate-bound gas in marine sediments: how much is really out there?, Earth-Sci. Rev., 66, 183–197."

While tertiary to the present submission, it should be recognized that the global amount and distribution of gas hydrate depend on the potential volume of gas hydrate and the amount held within this volume. That is, the debate lies not only in the vertical dimension, but also in the horizontal dimension. This leads to multiple problems with the analysis by Milkov (2004). If interested, see section 5 of Dickens (Climates of the Past, open for discussion).

(2 – intermediate) Degassing curves should be shown for all cores, not just one. Ideally, these would additionally show the volume-time relationships, so it is clear how they were degassed. These curves should also be in MPa not bar (even though the conversion is straightforward). Lastly, the threshold pressures for all cores should be tabulated. There is such a limited amount of these data available, that it remains important and useful to show. Which brings me to ...

(3 - major) I do not understand the given degassing curve with the present text and the lack of referencing and discussion. A general theory for how pressure cores should degas with dropping pressure and time has been formulated (Dickens et al., Proc. ODP 164, 2000). Pressure should drop to a threshold pressure equivalent to that on the gas hydrate phase boundary (after release of air and free gas). If gas hydrate exists,

pressure should track the gas hydrate phase boundary, as it evolves with changes in water activity (salinity). This concept was evaluated on Leg 164 cores (Dickens et al., 2000), but the degassing was mostly under non-ideal conditions (too fast). It was subsequently tested in a long (1 week) experiment for one core in particular (Milkov et al., EPSL, 2004), which seemed to support the theory.

The degassing curve shown in the present submission appears to fall well away from such theoretical degassing curves. Notably, the threshold pressure is too high, and the curve is not quasi-linear during purported gas hydrate dissociation. So, what is going on? Is the curve plotted correctly? Is the theory flawed? Does the style of degassing depend on the tool used and how gas evolution is measured? Were the cores degassed far out of equilibrium given the total gas concentration? Following number 2 above, it would be great to have a good discussion on this issue.

Jerry

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