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***Interactive comment on* “Gas properties of winter lake ice in Northern Sweden: biogeochemical processes and implication for carbon gas release” by T. Boereboom et al.**

**Anonymous Referee #1**

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Review of the manuscript Biogeosciences Discuss., 8, 9639–9669, 2011, www.biogeosciences-discuss.net/8/9639/2011/, doi:10.5194/bgd-8-9639-2011, entitled Gas properties of winter lake ice in Northern Sweden: biogeochemical processes and implication for carbon gas release, by T. Boereboom, M. Depoorter, S. Coppens, and J.-L. Tison.

**General comments**

I find this a timely study of actual gas content in ice in the context of greenhouse gas emissions from lakes at high latitudes. It also contains an interesting introduction of a new bubble classification that is related to the process behind the bubble formation.

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This classification indicates that the gas in some types of bubbles found previously was dissolved in the surface water (eg formed from exsolution) and was not derived from ebullition in contrast to previous assumptions. This leads to different concentrations of CH<sub>4</sub> (and other gases) in different types of bubbles and such knowledge is needed for attempts to quantify CH<sub>4</sub> emissions during winter.

#### Specific comments

Fig 4. Assuming that the figure show represent average gas composition in the bubbles being present in the ice, this graph is very interesting because it show a CH<sub>4</sub> contribution of at the most 10 % of the bubble content (actually it would be interesting to sum all gases because most of the important gases seem to have been measured). In summertime ebullition CH<sub>4</sub> levels are generally much higher so just figure 4 in itself indicate (1) that most gases come from exsolution and (2) that in bubbles caused by ebullition there is substantial exchange with dissolved gases and probably substantial methane oxidation. This should be discussed more in my opinion.

P 9640 L 17-18: “Most available studies on boreal lakes have focused on quantifying GHG emissions from sediment by means of various systems collecting gases at the lake surface. . .”: Gas collection systems at the lake surface cannot alone say anything about the sediment release because of water column processes. I think it would be more correct to write “Most available studies on boreal lakes have focused on quantifying GHG emissions from water by means of various systems collecting gases at the lake surface. . .”

P9642 L5-6: Is it correct that no CH<sub>4</sub> fluxes are included in the climate models so far or do you mean that lake CH<sub>4</sub> emissions are not included?

P9643 Methods section: I find the methods section too brief. Even if there are references for used techniques I think short descriptions of all steps as performed are necessary. The quality of the data crucially depends on very practical work regarding processing of the cores and gas extractions taking pressure, potential leakage and

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other things into account. It would be good if the readers (also those not familiar with the techniques and equipment used) can evaluate if all details were considered and successfully addressed without having to read a large number of cited papers. So, I think more details are needed.

P9646 L15-20: It is stated that gas concentrations of methane are underestimated because of the sampling procedures. Please provide more method details and explain why and how the samples become underestimated.

P9649 – 9652: I am not sure I understand the modeling of water concentrations in this discussion and in Figure 6 and 7. First, I guess the model is driven by how the exsolution affects concentrations in the water under the ice, and that water concentration ppm values represent ppmv of gas in equilibrium with dissolved gas (not completely clear in the text or figures)?

Second, some assumptions are clearly wrong because it is well known that at least CO<sub>2</sub> and CH<sub>4</sub> surface water concentrations are typically highly supersaturated in systems like those studied and I would not surprise me if such information is available from the studied lakes given the proximity to Stordalen where many GHG studies have been performed throughout the years. Hence, even very high concentrations in ice bubbles could indicate exsolution and the actual concentration will be a result of how large water volume that exchange gas with the bubble in the ice (larger water volume means larger concentration) and the water volume will depend on the water mixing under the ice which is difficult to predict because it may vary spatially and over time.

Third, there are many processes in or under the ice that can affect concentrations of many of the studied gases even after the system was covered by ice. For example, algal growth under clear ice is well known and should affect CO<sub>2</sub> and O<sub>2</sub> concentrations. Similarly, respiration under dark or snow covered ice could affect O<sub>2</sub> and CO<sub>2</sub> as well. Further, CH<sub>4</sub> oxidation can affect CH<sub>4</sub> concentrations, and possibly to a small extent O<sub>2</sub> and CO<sub>2</sub> concentrations, in the water and in bubbles underneath the ice.

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Some of these processes are discussed in relation to the figures, - eg P9651 L14-18 “In our case, the similarity of the reconstructed CH<sub>4</sub> and CO<sub>2</sub> concentration profiles in water in most lakes (Fig. 6, middle and right panels) suggest that acetate fermentation might be the dominant extra CO<sub>2</sub> source at work, as opposed to methane oxidation. This also puts forward acetate fermentation as the source for methane.” - but, even if some statements appear likely given fundamental previous findings in biogeochemistry, I cannot see how the available data in figure 6 or 7 support statements about dominating processes and I can see many alternative explanations. In the example above the similarity between CO<sub>2</sub> and CH<sub>4</sub> profiles can simply be related to physics like exsolution and water movements along with ice development. In addition it is not clear why N<sub>2</sub> decrease and O<sub>2</sub> increase with depth in the models (Fig 6 and 7) and such a behavior seems not to be supported by data in Figure 4.

In general I think too much is read into the theoretical models and Figure 6 and 7 in terms of processes, and especially given flaws in initial assumptions. I think a more robust alternative would be to look at literature data or other studies of the lakes in focus to get a likely range of surface water concentrations and to calculate realistic concentration ranges in bubbles taking not only water concentrations but also volume ratios of water to bubbles into account (gas from what water volume exchange with what gas volume?), and to not do this with depth because the uncertainty of what happens over time when the ice grow is so high.

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