

## **Reply to reviewer #2:**

We thank the reviewer for the relevant comments and remarks which considerably improved the quality of the paper. We answered all comments below and we prepared an adapted version of the manuscript in a supplement file where all modifications (suppressions and additions are in red).

1. **Referee:** The paper is in general well written, the discussion is too long and at present not always supported by data and models.

**Response:** *We concede that the discussion is long and we decided to rewrite one part of this section to make the methods and explanations on the model clearer. This unfortunately implies difficulties in shortening the manuscript.*

2. **Referee:** The modelling part is not presented with sufficient detail and is based on questionable assumptions. Specifically, the reconstruction of the dissolved gases in the water column is erroneous because it neglects important factors such as the convection of the water column during ice formation and the chemical equilibria of CO<sub>2</sub>.

**Response:** *A new version of the modelling part, more explicit, has now been written in the revised manuscript. We believe convection is limited in such lakes during winter since the water column would be thermally stratified (e.g. Casper 2000 for reference). And the model has been considerably improved in two way, along both referees comments. First, it uses the available local values of CO<sub>2</sub> concentration in the water (see Jonsson et al., 2007) and second, it takes into account the carbonate system (using the CO2Calc software from the USGS).*

3. **Referee:** The section on "depth dependency of gas composition" as well as Figures 6 & 7 needs a serious revision to address the more detailed concerns documented below.

**Response:** *This whole section has been rewritten, the method supported with conceptual diagram, caution taken to dissociate "concentration" and "mixing ratio" concepts.*

4. The relations between lake characteristics and bubble formation are not conclusive because they are based on one lake per category only. The study is valid in assessing the variability of gas loads in lake ice and the influence of atmospheric conditions but the statistical power is lacking to generalize along hydrological or lake morphology factors.

**Response:** *We agree. However, it is nowhere said in the paper that this pilot data set could serve as a way to quantify and upscale the geometry/hydrological regime vs. gas release relationships...we simply draw the attention of the reader on its potential importance!. We added a sentence in the conclusions to clarify this.*

5. **Referee:** The title is too long, the emphasis is not so much on biogeochemical processes but on the inventory of gases in trapped bubbles.

**Response:** *New title: "Gas properties of winter lake ice in Northern Sweden: implications for carbon gas release"*

6. **Referee:** Abstract (line 14): “Our methane emission budget” – there are four different lakes and four budgets... It might be worthwhile to mention the CO<sub>2</sub> budget and the other gas analyses too.

**Response:** *Abstract modified in accordance in the revised manuscript.*

7. **Referee:** Introduction (9641 line 5ff). Here the previous studies on lake ice bubbles and their gas composition should be mentioned. As mentioned above, the “Interactions between the water column and the ice cover...” cannot be conclusively assessed because each lake type is represented only once.

**Response:** *Sentence added at the end of the introduction.*

8. **Referee:** Methods (line 15 ff). The section should be expanded: What kind of calibration was done for the gas analysis? What is a “dry extraction technique”? What is a Toepler pump? Where are the Kovacs enterprises located? Add more references and expand on the methods in such a way that they could be repeated without contacting the authors.

**Response:** *The methods section has been improved. See the revised manuscript for details.*

9. **Referee:** Results – Gas composition (9644, line 13). How was the gas composition analyzed continuously and what is the difference in “high-resolution” measurements?

**Response:** *The manuscript has been changed for clarity.*

10. **Referee:** Total gas content (9645, line 15) What do you mean by “should be taken with care”? What is the precision of the measurements? If the values represent a minimum estimate – how large could the real values be?

**Response:** *The new methods section has been extended and made more explicit.*

11. **Referee:** Discussion – classification (9646, line 13 ff). The variability of the data is just too high to really justify a link between morphology and content. The classification along morphologies is convincing but based on Fig. 5 one can argue that types 4, 5 and 6 have a very similar range of methane concentrations.

**Response:** *Sentence added at the end of section 5.2.*

12. **Referee:** Depth dependency (9647, line 12). O<sub>2</sub> consumption in most lakes occurs at the sediment surface and not in the water column.

**Response:** *The text has been changed.*

13. (9647, line 18). Here the authors should clearly state that the assumption of atmospheric equilibrium represents just a theoretical reference scenario. Normally lake water will be over-saturated with methane, carbon dioxide, but under saturated with oxygen close to the sediment. “Grey triangles down” is an awkward expression. It is sufficient to call the symbols “triangles”.

**Response:** *This part is clearer in the revised manuscript and the model has been improved. It now takes into account the observed water surface CO<sub>2</sub> concentration. The term “Grey triangles down” has been changed.*

**14. Referee:** I don't see how the atmospheric equilibrium model leads to an increase in the concentration with depth in Fig 6.

**Response:** *Because ice formation in lake (where the ice results from the progress of a freezing front in the water reservoir) rejects a large part of the gases into the liquid water, with limited access to the atmosphere (moat). This results in an increase of the gases concentration as the volume of liquid decreases. This has also been typically observed during controlled experimental closed system freezing in sea ice tanks (INTERICE experiments, HSVA, Hamburg, Germany)*

**15. Referee:** 9648 line 11. The "simple conservative mass balance" on which part of Fig. 6 is based should be given as an equation. It is not clear to me how they were calculated.

**Response:** *This is now synthesized in a conceptual diagram.*

**16. Referee:** (9648, line 10). As the CO<sub>2</sub> profiles in water (Fig 7) are simply wrong. Either a more complete model should be applied which takes alkalinity and acid-base equilibria into account or this part of the discussion should be deleted. It is worthwhile to publish and discuss the gas composition in the bubbles as a function of depth. The speculation of the distribution of dissolved gases in the water column is erroneous because of convective mixing of the water and of progressive growth of ice from top to the bottom. This part of the discussion should be omitted or based on real observations of the water column during the winter months.

**Response:** *The model has been adapted in two ways in the revised manuscript, first, it uses the available local values of CO<sub>2</sub> concentration in water and second, it takes into account the carbonate system (using the CO2Calc software from the USGS). We believe convection is limited during the winter in our lakes because they should be thermally stratified.*

**17. Referee:** (9652 line 4). It is hard to see how methane could be oxidized in the ice. A chemical mechanism involving reactive oxygen species would have to be involved. This requires photochemical activation by substantial sunlight, which is in short supply in winter at 68° N.

**Response:** *We agree and we suppressed "in the ice".*

**18. Referee:** Controls on bubble distribution. (9652, line 15) Other explanations for bubble-rich ice cores could hold: If the sediments are rich in organics and characterized by a large sedimentation rate then the methane flux from the sediments could be larger than in the neighboring environments. Isolating hydrological and morphological influence factors would require a large range of field sites and a more careful characterization of the sedimentary regime.

**Response:** *Agreed. Sentence added in section 5.4.*