Biogeosciences Discuss., 8, C4601–C4604, 2011
www.biogeosciences-discuss.net/8/C4601/2011/

Biogeosciences Discussions

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 #2

Received and published: 28 November 2011

The paper contains new and highly valuable data of gas bubbles and their chemical composition in winter ice from three different lakes at the same area (Torneträsk Lake) in Northern Sweden. The sampling methodology, bubble characterization and gas analyses are technically sound and address a new and important topic. The release of greenhouse gases methane and carbon dioxide in the boreal and arctic region will represents an important fraction of the GHG emissions, these fluxes are mostly studied during the summer season from wet surfaces. The pulse of gases escaping during ice melt should be quantified more precisely and this study documents a fresh approach to address this problem. The authors sampled four different lakes in the same region. The study therefore allows them to assess the variability of the gas trap in bubbles

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Interactive Discussion

Discussion Paper

in lake ice formed under almost identical meteorological conditions. The paper is in general well written, the discussion is too long and at present not always supported by data and models. I have two general concerns: 1) The modeling 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_2. The section on "depth dependency of gas composition" as well as Figures 6&7 need a serious revision to address the more detailed concerns documented below. 2) 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. Here are more detailed comments: 3) The title is too long, the emphasis is not so much on biogeochemical processes but on the inventory of gases in trapped bubbles. 4) Abstract (line 14): "Our methane emission budget" – there are four different lakes and four budgets. . . It might be worthwhile to mention the CO_2 budget and the other gas analyses too. 5) 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. 6) 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. 7) Results – Gas composition (9644, line 13) . How was the gas composition analyzed continuously and what is the difference in "high-resolution" measurements? 8) 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? 9) 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. 10) Depth dependency (9647, line 12). O2 consumption in most lakes occurs at the sediment surface and not in the water column. 11) (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”. I don’t see how the atmospheric equilibrium model leads to an increase in the concentration with depth in Fig 6. 12) 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. 13) (9648, line 10). As the CO2 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. 14) (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. 15) 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. 16) Atmospheric influence (9653, lines 7 ff). The time-series in Fig.

8 looks interesting and the conclusions have a stronger base than the hydromorphological factors, because they are based on two bubbling events in the same region. 17) Methane budget: (9654, line 10 ff). The extrapolation is carefully done and adequately discussed. It provides a valuable contribution to compare "background" summer fluxes with winter fluxes.

Interactive comment on Biogeosciences Discuss., 8, 9639, 2011.

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