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Interactive comment on “Methane dynamics in different boreal lake types” by S. Juutinen et al.

Anonymous Referee #2

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General:

The dataset of CH₄ concentrations from randomly sampled 177 Finnish lakes and 30 additional eutrophic lakes is globally unique and provide a very good basis for reliable estimations of CH₄ effluxes from boreal lakes into the atmosphere. The lakes were grouped according to regional typology for implementation of European Union water framework directive and the aim of the study was to evaluate generalization tools for CH₄ release from northern lakes. The dataset with several environmental variables was analysed with multiple linear regression analysis. The paper address a relevant scientific question of CH₄ fluxes between ecosystems and the atmosphere and is within the scope of BG. I recommend publication of this manuscript, but I suggest that the authors consider the following comments and suggestions. Because the

S1941

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Interactive Discussion

Discussion Paper



referee comments by David Bastviken are already available on the web pages of BG, I do not repeat the topics raised by him.

Why not to extrapolate the results from 207 lakes and present an estimate of total CH₄ efflux from the lakes of the boreal area, or a global estimate for all freshwater lakes by applying a recent estimate of the number and area of lakes on the Earth presented by Downing et al. (2006)? Several authors have presented such estimates with much lower number of lakes studied. The authors conclude that 'In the absence of more accurate data, lake area from remote surveys could be used as an approximation for the CH₄ emissions in boreal and arctic landscapes with similar glacial history' (page 3478, lines 24-26). Thus, I suggest that this conclusion could be applied in this paper. Somebody else is certainly soon keen to do such 'a new global estimate' applying the results of this profound study, please do it by yourself.

Some remarks:

The timing of the sampling was planned to cover the most critical periods (late winter, after thaw in May-June, late summer, October) for water column stratification and mixing, and, thus, for the gas exchange between lake surface and the atmosphere. The extensive sampling during two years and four times per year and lake has certainly, for an unknown extent, contributed to the wide variation observed in the dataset, which cannot in practice be avoided in this kind of study. The timing of turnover periods may vary for several weeks in lakes close to each other; this is influenced by water quality (colour) and morphometry of the lakes and the surrounding catchment. E.g. the observations of the largest methane storage in spring in 6.3% and in autumn 8.7% of the lakes (page 3468, line 11) may be related to sampling before the complete mixing, or that the lakes are meromictic (less probable). Annual variation in weather conditions may also remarkably impact stratification of the water masses as well as variation in precipitation may influence on the availability of the substrates for methanogens producing methane. In some cases rather modest, although significant, relationship found between CH₄ concentration and some other variables found in tables 4-7, may have

S1942

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5, S1941–S1943, 2008

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had influenced by the sampling frequency. Was there more intensively sampled data of CH₄ from any of these lakes? And if there were, could the impact of sampling frequency on the annual flux be compared with that obtained by four times/yr sampled data.

Page 3478, lines 1-3, Table 7: 'Both the regression analyses and lake type comparisons indicate increasing CH₄ emissions with decreasing depth, increasing total phosphorus concentration and decreasing lake area.' However, Table 7 shows that the relationship was negative to totP and positive to area. Table 4 shows that in some lake types CH₄ concentration was positively related to totP, but negatively to O₂, depth and area. How to explain this discrepancy?

Table 4 second last line: parameter value for depth is missing.

Interactive comment on Biogeosciences Discuss., 5, 3457, 2008.

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5, S1941–S1943, 2008

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