Biogeosciences Discuss., 6, C130–C135, 2009
www.biogeosciences-discuss.net/6/C130/2009/

Biogeosciences Discussions

Interactive comment on "Carbon dioxide and methane annual emissions from two boreal reservoirs and nearby lakes in Quebec, Canada" *by* M. Demarty et al.

Anonymous Referee #2

Received and published: 20 April 2009

General comments This paper addresses a timely and significant issue, namely the greenhouse gas emissions from boreal reservoirs. A multiyear-dataset is used to estimate greenhouse gas (CO2 and CH4) emissions after ice-out; these emissions are usually the highest during the year due to the accumulation of gases under the ice cover. Thereby, the authors can calculate the annual CO2 and CH4 emission from two reservoirs and three natural lakes. This is interesting and relevant information, as CO2 and CH4 emissions from hydroelectric reservoirs is a subject of intensive current debate.

The authors have an impressive dataset to work with, but I find that data analysis

and interpretation could be expanded to greater detail. Also the presentation of the results is not always very clear. Further, main focus is given the CO2 data, but also the CH4 data deserve some further analysis and discussion. Also, CH4 emission via gas bubbles rising from the sediments (ebullition), which is a major CH4 emission pathway in many lakes (see Bastviken et al. 2004), is dismissed without convincing data or references. There also are a few details in the method used for sampling and measuring CH4, which may have affected the quality of the CH4 data. Further, the similarities or differences in GHG emission between reservoirs and natural lakes are neither clearly reported nor discussed, which I find surprising given the title and design of the study.

Due to the very nice datset on fairly remote and large systems, covering both the annual cycle and several years, this study has the potential to represent a significant step ahead in our understanding of GHG emissions from boreal aquatic systems, in particular of reservoirs. I therefore think that this study deserves to be published in Biogeosciences. However, the authors will have to make a thorough revision in order to increase the overall quality of the paper.

Specific comments

Abstract

L10-11, "CH4 fluxes were of minor importance": in terms of C flux, or CO2-equivalent-flux?

Introduction

P2940 L16: Cole 2007 is rather a review than a study

L19. Carbon loss in freshwaters is mainly due to outgassing, burial ranks second (cf. Algesten et al. 2004, GCB).

L24. "heterotrophy" is an ambiguous term, use "ecosystem net heterotrophy" instead. Also, heterotrophy is a state, not a process.

P 2941 L29. I guess “degassing and bubble flux” means CH4 emisison via ebullition; this needs to be clarified. If yes, it is a pretty strong statement that ebullition fluxes are very small in boreal reservoirs, given that CH4 ebullition is a major, or even the dominant, emission pathway in lakes (see Bastviken et al. 2004). Further, of the three references supporting this statement, one does not mention ebullition at all (Roehm & Tremblay 2006), one refers to other studies and mentions that ebullition may occur in shallow reservoirs (Tremblay et al. 2005), and the third (Bastien & Tremblay) is not published yet. Hence, it seems to me that unless convincing data on CH4 ebullition from these reservoirs can be presented, one should be careful with such general statements. It is better to mention that ebullition may occur, and to discuss to what degree it may contribute to total CH4 emission, depending on the depth of the reservoir (see McGinnis et al. 2006, JGR).

Material and methods

Study sites – it would be helpful to also give the surface area and the maximum and average depth of the reservoirs and lakes.

P2942 L 22-23. replace “under 10 m” with “< 10 m” and “above 10 m” with “> 10 m” avoid ambiguity.

P2943 L17-18. It am a bit puzzled why CH4 samples were taken after the water has passed the gas exchanger. The gas exchanger strips all dissolved gases from the water, and should thus strongly affect the CH4 concentration in the water leaving it. It would have been far better to measure CH4 in samples taken before the gas exchanger. It is possible that this methodology worked anyway, in case pCH4 in the gas loop reached equilibrium with pCH4 in the water passing the gas exchanger, but this should be better supported by data. As this is a critical point that could affect the quality of the CH4 data, I really think that the authors should show the data of the methods test, to document the reliability of the CH4 data.

P2943 L24. While CH4 can be measured on a TCD detector, a FID detector has higher

sensitivity. The authors should report the analytical precision of their measurements.

P2944. I do not think it is necessary to write out equations (3) and (5), a reference to Weiss (1974) is enough.

L2945 P3. It should be mentioned that the k was estimated from wind speed by using Cole and Caraco 1998.

Results

I miss a comparison of GHG fluxes between the reservoirs and the natural lakes – from the title, and given the study design, this seems to be a major purpose of the paper. A clear results section, and a discussion, is necessary and relevant. For example, just taking a quick look at the means (Table 1), it seems to me that pCH4 was higher in the reservoirs than in the natural lakes. Is this the case? And if yes, how could that be? Also, the highest pCO2 was in Eastmain 1 reservoir (p2946 L24) – this should be explicitly discussed in the context of a comparison between reservoirs and natural lakes.

All results should be described in past tense, please correct where necessary.

There are many comparisons, between lakes, gases, seasons, and years. This makes parts of the text difficult to follow (e.g. 3.2, first paragraph). The authors should try to improve the structure of the text. Maybe sub-headings could be useful?

P2946 L19. Looking at Table 2, there seems to have been a very strong CH4 accumulation in Eastmain 1 under the ice (pCH4 rise from 40 in Jan to 287 in Mar)! However, the error for the March measurement is huge (287 $\pm$ 982). Is the obvious CH4 increase caused by accumulation under the ice, outliers, or rather due to bad data? This issue needs to explicitly addressed, and analyzed statistically, as it might have strong implications for the annual CH4 emission from the reservoir.

P2947 L2. Exactly how was the grouping done?

P2947 L11-14. Unclear sentence, please rephrase.

P2947 L18-19. “was observed in the field to last around one month, from 15 May to 15 Jun”. According to Table 1, no field sampling was done during this period, so where does this data or conclusion come from?

P2948 L2. Replace “considered” with “calculated”.

P2948 L12-19. This section is a bit unclear. I think what the authors mean is that in most studies, ice-out emission is calculated as the difference between accumulated amount gas minus amount gas at atmospheric equilibrium, while this study subtracts the gas amount corresponding to the mean open-water pCO2 and pCH4. This should be clarified.

P2949. The discussion of the ice thickness is quite unclear. For example, the conclusion “thus suggesting that ice formation. . .” in L2 is not very easy to understand. Also, what does “ice follow up” (L4) mean?

P2949 L13-16. This sentence indicates that there are 30 years of emission data, but this can hardly be the case. Please clarify.

P2949 L18. I would argue that it is not primarily the presence of bacterial activity that causes CO2 accumulation under the ice, but rather the complete absence of primary production.

P2950 L3. Do you mean “highest pCO2”, or rather “highest pCO2 increase”?

P2950 L18-19. This study reports under-ice accumulation of CH4 during 4 winters (2 winters in Eastmain, 1 winter in Mistumis, and 1 winter in Clarkie). Of these 4 data on CH4 accumulation, 1 shows strong accumulation (see comment above), and the other three do not. With these data, it seems that the conclusion “no clear CH4 accumulation was observed under the ice” is not well supported. The observed patterns in under-ice CH4 accumulation should be clearly reported and discussed, not dismissed.

References

Please give page numbers for Tremblay et al. 2005

Tables

Table 1. There is an impressive amount of sampling stations. How was the spatial variability between the stations? The spatial variability should be reported in the text. Was the spatial variability accounted for when calculating the annual GHG emission? Also, was each station sampled once during each campaign, or several times?

Table 2. What are the errors?

Table 3. “Springtime” for most people means flowers and singing birds, while you refer to pCO2 increase under the ice. I would suggest to rename “springtime” to “under-ice”, also in the text if applicable. This will also more clearly mark the difference to “springtime CO2 emission”, which is used for emission after ice-out (e.g. in Table 4)

Table 6. These numbers can be (and are) reported in the text, so I suggest to remove this table.

Figures

Fig.1. “Celguard” needs to be properly described (gas exchanger, degasifier).

Fig. 2 is very small in my copy. What do the error bars show?

Fig. 3. The term “baseline” should be defined in the legend.

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