Referee 1:	**Responses:**
1) The authors miss some important citations in the reference section: Cole (2007) Carpenter(2005) IPCC (2006) Demarty et al submitted 2007	These articles have been added to the reference section.
2) Some references included in the references sections does not appear at the manuscript: Abril et al Carpenter et al 1986 del Giorgio et al, 1999 Kritzberg et al 2006	These articles have been removed from the reference section.
3) Page 4 , line 6 What is the reason for different sampling stations at each field campaign? The authors could explain it?	Corrected Page 4 Line 7: the sampling stations remained the same among field campaigns, but the number of sampled stations depended on weather conditions.
4) Page 4 - Change the concept (hydraulic system) to (watershed area)	Corrected.
5) Page 4 – What is YSI 600?????	Sensor for Temperature and dissolved oxygen saturation - Corrected.
6) Page 4 , line 17 – What is 2007 after Reservoir word?	Syntax error corrected.
7)Page 4 , line 19 What is Estmain-1 and LG-2 – Is bibliographic reference too?	Names of the 2 reservoirs in the articles cited, corrected.
8) Page5 – line 16 – usually we use Flame Ionizating Detector to analyse CH4 in GCs. The authors use Thermal conductivity detectors?	We actually use a FID, error corrected.
9) Page 8, line 7 – The figures 2 E and F does not contain information to conclude that January and March have highest values. Please insert other figure to explain it or modify your statement.	The highest values at Eastmain reservoir and at Mistumis L. were observed in March 2007 and March 2008, and not in January. Corrected.

10) The last phrase of page 8 was not linked with the continued sentence at page 9. I will recommend to the author to improve entirely the section 3.2.	We have modified this section to clarify the understanding of the text.
11) I think that the results contained in the tables are little be investigate by the authors.	The information presented in the tables is used in the article. As it is the use of tables, we have not detailed the data presented in tables but we have described the general trends and let the reader look at the details in the tables.
12) I am recommend better important improvements of the manuscript before published.	We have revised the language, made some modifications according to the reviewer’s comments that have improved the overall quality of this article.

Referee 2

Abstract

L10-11, “CH4 fluxes were of minor importance”: in terms of C flux, or CO2-equivalentflux?	In term of CO2-equivalent flux, corrected in the text.

Introduction

P2940 L16: Cole 2007 is rather a review than a study	Corrected, reference added.
L19. Carbon loss in freshwaters is mainly due to outgassing, burial ranks second (cf. Algesten et al. 2004, GCB).	“buried” replaced by “processed”.
L24. “heterotrophy” is an ambiguous term, use “ecosystem net heterotrophy” instead. Also, heterotrophy is a state, not a process.	Corrected.
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	Corrections are made in the introduction to precise that we focus on diffusive fluxes. The fact that degassing and ebullition are not studied in this article is mentioned in the introduction as well as in the discussion as suggested. The degassing fluxes are the fluxes resulting from turbination of water in the power house. The bubbling is actually the ebullition.

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.	See new Table 1.
P2942 L 22-23. replace “under 10 m” with “< 10 m” and “above 10 m” with “> 10 m”avoid ambiguity.	Modified accordingly.
P2943 L17-18. I 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.	We made several tests in lab to determine the effect of the method. CO_2 and CH_4 measured from samples taken before and after the water exchanger and analyzed by GC gave similar results (Student t-test: $p > 0.05$). We are finalizing the comparative study, which will be published by the end of the year.
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.	We actually use a FID, error corrected.

P2944. I do not think it is necessary to write out equations (3) and (5), a reference toWeiss (1974) is enough.	Corrected.
L2945 P3. It should be mentioned that the k was estimated from wind speed by usingCole and Caraco 1998.	Corrected.
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.	We reorganized the results according to these comments.
All results should be described in past tense, please correct where necessary.	Corrected.
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	The high value observed in March is due to one sampling station located above a flooded peat. That is why the statistical tests show no significant accumulation between January and March.

measurement is huge (287 _ 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?	March and April data have been considered as one group. Corrected.
P2947 L11-14. Unclear sentence, please rephrase.	Corrected.
P2947 L18-19. “was observed in the field to last around one month, from 15 May to15 Jun”. According to Table 1, no field sampling was done during this period, so where does this data or conclusion come from?	Accordingly to the beginning of the ice break-up (registered by the ice survey program over the last 30 years), the degassing period in the studied areas was supposed to last around one month, from May 15th to June 15th.
P2948 L2. Replace “considered” with “calculated”.	Corrected.
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.	Corrected.

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?	Language mistakes corrected.
P2949 L13-16. This sentence indicates that there are 30 years of emission data, but this can hardly be the case. Please clarify.	See correction made at P2947 L18-19.
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.	The absence of primary production leads to observation of the bacterial respiration which is usually hidden (CO_2 produced by respiration being consumed). Specified.
P2950 L3. Do you mean “highest pCO2”, or rather “highest pCO2 increase”?	Highest $p\mathrm{CO}_2$ values
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.	The high value observed in March is due to one sampling station located above a flooded peat. That is why the statistical tests show no significant accumulation between January and March.

References

Please give page numbers for Tremblay et al. 2005	Done.

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.	Standard deviation representing the spatial variability are given in Table 2.
Was the spatial variability accounted for when calculating the annual GHG emission?	Propagation formula were used to calculate the error of the under-ice period daily rate of increase (Table 3), but no for subsequent calculations because errors increased at each steps of the estimation calculation leading to high uncertainties, which were though to be unrealistic.
Also, was each station sampled once during each campaign, or several times?	The sampling stations were visited only once per campaign.
Table 2. What are the errors?	Standard deviations, corrected.
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 “underice”, 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)	Done.

Table 6. These numbers can be (and are) reported in the text, so I suggest to remove this table.	Done.
Figures	
Fig.1. “Celguard” needs to be properly described (gas exchanger, degasifier).	Done.
Fig. 2 is very small in my copy. What do the error bars show?	Bars represent standard errors.
Fig. 3. The term “baseline” should be defined in the legend.	Done.

Reviewer #3

Review of Demarty et al. This MS presents CO2 and CH4 concentrations measured during one year in two artificial reservoirs and in two lakes in Canada, together with ancillary data (temperature and oxygen). CO2 and CH4 fluxes to the atmosphere are then calculated. The dataset represents quiet a lot of field work. The MS itself is however relatively poor, interpretation of data is superficial and the most important literature is ignored. The authors minimize in their discussion the role of boreal lakes and reservoirs as CH4 sources. Finally, the CH4 concentration data might be affected by problems due to the sampling methodology. In the present version of the paper, it is not possible to check the quality of the CH4 data due to a lack of detailed information in the material and method section (reference to another submitted paper). In its present form this MS is quiet far from the quality standards of Biogeosciences. Compare for instance with the recent paper by Juutinen et al. BG 6, 209–223, 2009, on the same subject, and not even cited here.

Methodological problem:
CH4 has a low solubility. The water sampled after the gas extraction system has lost most of its methane to the gas phase and contains much less CH4 than the lake water. Because of the low solubility of CH4, it might take hours for complete equilibration of the gas phase in the extraction system and for the water to recover the in situ CH4 concentration.

We made $p\mathrm{CH}_4$ measurements before and after the gas exchanger (used to measure $p\mathrm{CO}_2$) and this device did not affect the measurements. We are finalizing the comparative study, which will be published by the end of the year.

CH4 concern in boreal lakes and reservoirs: P2950 The authors write “Our results clearly show the diffusive CH4 emissions are not of concern in the studied systems”.Such strong statement must be supported by more discussion. Highlighting low CH4 diffusion in given lakes is possible only in comparison with the potential high CH4 ebullition, the high diffusion in the littoral zone and during lake overturn in these lakes and in other boreal lakes and reservoirs (see literature below). From this literature, it is clear that CH4 emission from the pelagic part of deep boreal lakes is minor (except during overturn). The data presented here must be interpreted in the light of this literature and one conclusion will probably be that either these data miss most of the CH4 flux, or the studied systems are different for a given reason. In addition the authors have some data that show the importance of overturn (L13P2949) but do not present nor discuss them.	We agree with the referee and actually did not put enough emphasis on the fact that our study do not take into account ebullition. Measurements have been made by another team to estimate this process at Eastmain 1 Reservoir during summer 2008; data are being processed and preliminary results show very weak ebullition in this reservoir. We are aware of the literature proposed by the referee. However it seems difficult for us to integrate these references in our discussion. Huttunen *et al.* (2004) appeared more adequate for our purpose. As mentioned in the discussion, shallow areas of lakes and reservoirs (in Quebec) are not suitable habitats for CH4 production as they are generally rocky shore with little or no organic matter in place with well oxygenated waters.
P2946 The authors write “no difference in surface pCH4: : : ANOVA and Tukey test,p>0,05). However, in Table 2, the high CH4 concentration and heterogeneity in Eastmain1 reservoir in March 2008 (287+/-982 ppmv) is not discussed at all, why? Dissolved CH4 concentration reaching 1000ppmv in lake oxic waters is something special that deserves discussion.	Outliers are included and explain why the statistical tests are not significant.
The section from L25P2950 to L5 P2951 is pure speculation and refers to only one paper.	We disagree with the reviewer’s point of view. The observed trend is not pure speculation, it is based on field data. However to clarify this, we have rewrite this paragraph and added references.

In the M&M section it is stated that the studied reservoirs have flooded peats. Are the authors aware of the following study: Scott et al 1999. The importance of floating peat to methane fluxes from flooded peatlands. Biogeochemistry 47, 187-202 ?	Some sampling stations are above these flooded peat and the spatial variation is represented by the standard errors presented in Table 3. Moreover, floating peat are not an issue in Quebec reservoir's as there is only a very small fraction of the flooded peat (<15% of the flooded land) that are becoming floating peat and these are broken down rapidly (within 1-3 year) by the wave and ice action.
Details and presentation	
A map of the lakes and reservoir, showing sampling stations Surface area, depth, etc: : : Residence time of water in the reservoirs Figure 2 difficult to read	We have added a new table (Table1) describing the sampling station; Maps with sampling stations are available but would need a large place.
Section 3.3 very difficult to understand, CO2 accumulation is observed but no CH4 accumulation? What is a “baseline pCO2”? referring to submitted data make the section not understandable	Section reorganized accordingly (as suggested by the two other referees).
P2950L10 Define “the return to natural aquatic ecosystem value”	Values in the same range than nearby lakes.
Literature to refer to (see also references therein): Bastviken, D., J. Cole, M. Pace, and L. Tranvik (2004), Methane emissions from lakes: dependence of lake characteristics, two regional assessments, and a global estimate, Global Biogeochem. Cycles, doi:10.1029/2004GB002238.	
Søvik A K; Augustin J; Heikkinen K; Huttunen J T; Necki J M; Karjalainen S M; Kløve B; Liikanen A; Mander U; Puustinen M; Teiter S; Wachniew P. Emission of the greenhouse gases nitrous oxide and methane from constructed wetlands in europe. Journal	

of environmental quality
2006;35(6):2360-73.

Huttunen Jari T; Alm Jukka; Liikanen Anu; Juutinen Sari; Larmola Tuula; Hammar Taina; Silvola Jouko; Martikainen Pertti J Fluxes of methane, carbon dioxide and nitrous oxide in boreal lakes and potential anthropogenic effects on the aquatic greenhouse gas emissions. Chemosphere 2003;52(3):609-21.

JUUTINEN Sari ; ALM Jukka ; LARMOLA Tuula ; HUTTUNEN Jari T. ; MORERO Micaela ; MARTIKAINEN Pertti J.; SILVOLA Jouko ; Major implication of the littoral zone for methane release from boreal lakes Global biogeochemical cycles 2003, vol.17, no4, pp. 28.1-28.11