

## ***Interactive comment on “Annual follow-up of carbon dioxide and methane diffusive emissions from two boreal reservoirs and nearby lakes in Québec, Canada” by M. Demarty et al.***

**Anonymous Referee #2**

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General comments:

This study provides a meso-scale estimate for the greenhouse gas fluxes from the reservoirs and the lakes situated in the same area. The study presents interesting results about the greenhouse gas balances in the reservoirs and lakes based on several sampling occasions and sampling depths. I think this manuscript is worth publishing, but it will require revisions before publishing. I find the manuscript in the present form somewhat incoherent and confusing, it requires a lot of patience and effort from the reader to be able to form a general view of what have been done and how and why. I do realize it is difficult to present a large dataset and a lot of results in a very simple

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way, but the manuscript would be greatly improved with a more focused approach and a more accurate use of terms and references.

My specific comments are: p. 5433, l. 20 There is nothing here about the sampling at Robert-Bourassa reservoir. Please revise. p. 5437, l. 7 Do you mean spatial variability? Please clarify. p. 5439, l. 20 “As explained above” Where? In the previous paragraph, there are CO<sub>2</sub> results. Please clarify. p. 5440, l. 7 On what basis do you assume that increase of pCO<sub>2</sub> under the ice is linear? Drawing a regression line between two sampling occasions (two flocks of points) will give a reasonably good regression coefficient but no signal if the line is linear or curved. The method you present for estimating springtime emissions might be useful, but you will have to convince the reader by discussing about the possible sources of error. P. 5441, l. 13 A comment on the discussion as a whole: One of the objectives of the study is to present a follow-up from 2006 to 2008 of GHG concentrations and fluxes, but in the discussion, there is nothing about the differences between years nor how the previous year affects the next year’s concentrations and fluxes. Please check that your objectives and findings are in line with each other. p. 5442, l. 5 I don’t understand how CO<sub>2</sub> accumulation between January and March tells something about the formation of ice before January. Please clarify. p. 5442, l. 17 You refer to Demarty et al. (2009) telling that the continuous measurements have been performed in the reservoirs earlier. This leaves me wondering why to estimate CO<sub>2</sub> spring emission by a few sampling occasions and extrapolation if CO<sub>2</sub> has already been measured continuously? You also justify the linear extrapolation of the CO<sub>2</sub> increase under ice by these continuous measurements. Again, the question is why to use regression between two sampling occasions, if you have measurements that are more frequent? If the idea of this whole sampling frame was to provide a tool to accurately estimate annual CO<sub>2</sub> flux by only 3 to 4 sampling occasions per year, and to use continuous measurements to confirm this method, I think you should tell a bit more about the methods and the results of these continuous measurements. Referring to the published paper is not quite enough in this case.

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p. 5442, l. 22 In several places in this manuscript, you refer to Demarty et al. (2009) by stating “We observed” or “we showed”. I understand it is quite correct, because you are referring to yourself. However, starting the sentence by “we observed” leaves the reader to think that you are talking about this study, not the parallel study. Please refer to the parallel study by “Demarty et al. (2009) observed. . .” or “Demarty et al. (2009) showed. . .” to avoid confusion. p. 5442, l. 27 I think this kind of the information belongs to methods or results, definitely not to discussion. p. 5444, l. 12 You don't tell what is this conclusion suggested by Duchmin et al (2006) that you find contradicting to your results.

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