Referee #2 (M. Langer)
Received and published: 9 September 2015
«Some minor issues.»

General Comments:

The study is written very well and understandable. However, a few points concerning the discussion around climate feedback mechanisms require clarification (see specific comments).

We clarified this as suggested (see answers to specific comments below).

Furthermore, the method section should include a description of the used temperature loggers and sensors (accuracy, location, and so on).

We added a sentence about the temperature data loggers in the Methods section.

In addition, the calculated CH4 and CO2 fluxes are based on coarse assumptions (average atmospheric concentrations) and a very basic diffusion model. Therefore, it would be recommend to provide realistic uncertainty estimates. Straight forward error propagation methods (e.g. Monte-Carlo simulations) should be applicable. In particular, the available statistics from multiple concentration measurements should be used and could be further investigated. It should be clarified whether the calculated flux magnitudes and directions are significant.

We ran statistical tests on our CO_2 and CH_4 data (including normal distribution testing and Monte Carlo substitutions), and added the results in a new supplement file (Table S2). We found that trough ponds (IWT type) were significantly different from the other two types of water bodies (polygonal ponds and lakes). We slightly modified the Discussion accordingly.

It might be also interesting to calculate the vertical C-balance of the different water body types in order to clearly label them as sink or source for atmospheric carbon. For numerous polygon ponds the CO2 gradient seems to be on the order of about -5 μ mol to -10 μ mol which for some ponds is partly balanced by the CH4 gradient (Fig. 5). Taking into account some uncertainties in the atmospheric CO2 and CH4 concentrations and additional CH4 fluxes from ebullition, the C-balance of some ponds might shift from negative to neutral. A short uncertainty analysis is highly recommended as outlined above.
Table 3 could be partly translated into a simple (arrow) diagram in order to illustrate the flux differences.

As suggested by the Referee, we produced an arrow diagram showing the net median fluxes of both CO_2 and CH_4 (diffusion and ebullition merged together) for each water body type. See the new Figure 9 for details. This new figure synthesizes the information from Table 3 without repeating it unnecessarily. As mentioned above (previous comment), we can conclude that trough ponds differ from the other water body types, as they can be considered as a significant GHG source (especially CO_2), whereas the other types are either small net sinks (polygonal ponds) or small net sources (lakes).

The result section appears short in comparison to the discussion. I recommend to embed descriptions of Fig.6 - 8 into the result section.

As suggested by the Referee, we added a description of Figures 6-8 in the Results.

Specific Comments:

Title: Maybe "the eastern Canadian Arctic" is a bit too general since all investigations were carried out on Bylot Island which might be not representative for the entire eastern Canadian Arctic.

We added 'Bylot Island, Nunavut' in the title. To our knowledge however, this work is the first to report on GHG age differences across the Eastern Canadian Arctic (apparently from the whole Canadian Arctic), so we preferred to keep this part. The modified title is now '*Modern to millennium-old greenhouse gases emitted from ponds and lakes of the Eastern Canadian Arctic (Bylot Island, Nunavut)*'.

p.11664; l.6: The statement that the strength of climate feedback is determined by the age of the released carbon requires clarification. Why would carbon that is 5000 years old cause a stronger climate feedback than carbon that is only 500 years old? I completely understand that it makes a difference whether old carbon can be processed or not. However, this would not affect the carbon-climate feedback mechanism, but change the size of the carbon pool that can be activated. I agree that this is an important question which is also reflected in number publications which discuss the permafrost carbon pool every year in very important journals.

We modified the sentence (also in response to the other comment below) to clarify that the strength of climate feedback is not related to the age in years per se (5000 years old is not less efficient than 40 000 years old, as long as it is in excess), but rather that if it's modern it does not generate a significant positive feedback, as opposed to if it's old (in excess in the system).

Permafrost on Bylot is syngenetic, meaning that carbon has been progressively sequestered (or 'locked') in the frozen ground since the beginning of permafrost inception (i.e. since the mid-Holocene deglaciation). Thermokarst processes are now doing the opposite, i.e. releasing excess carbon (century to millennium-aged) in the system.

p.11664; l.9-12: I recommend to use the terms "glaciated" or "covered by ice sheets" instead of "ice covered". This might be picky, but it reminds the reader on the extend of the ice cover.

We changed for 'ice sheets' (2nd sentence) as recommended, but we kept 'ice-covered' (1st sentence) because we mention the last glaciation just after, in the same sentence.

p.11664; l.15: I would say "contribute to positive climate feedback if released as GHGs".

We modified the sentence as suggested.

p.11664; l.15-17: Is it possible that water bodies act as carbon sinks under current climate conditions and change to carbon sources or become neutral under warming? Anyways, the atmospheric GHG budget would be affected and, thus, a climate feedback would exist even though relatively modern carbon is processed. However, I agree that the size of the carbon pool that could become available due to the thaw of permafrost is important. In general, I suggest to reduce the argumentation on feedback mechanisms which are not explicitly in the focus of this study. The

identification of carbon pools and their pathways and magnitudes of release already make up very good justifications.

We agree that feedback mechanisms are not the focus of this work. We toned down the argumentation as recommended. We think it is nevertheless important to mention this factor in the Introduction and discuss it, as we aimed at identifying aquatic carbon sources of different ages, which is of central importance in the general 'endeavor' of upscaling and modeling GHG emissions at larger spatial scales. As mentioned above, we clarified our climate feedback argumentation, focusing on old (excess) *vs*. modern carbon, and not on a linear age relationship.

p.11666; l.22-23: The information about birds might be not necessary. In general, this section could be condensed a bit. The study site description appears long compared to the result section.

We removed the information about birds, and shortened the Study area section.

p.11672; l 22-23: It should be clearly indicated that the measured temperature and oxygen profiles are not representative for entire July. The measured profiles depict a specific situation. Shallow water bodies such as polygon ponds can change their stratification within a few hours according to wind speed. Furthermore, it is very interesting that the temperature profiles c and d in Fig. 3 show bottom temperatures well below 4°C. Is there any explanation for this?

I guess the Referee refers to p. 11671 (l 22-23), instead of p. 11672? If so, the measured temperature and oxygen profiles are indeed representative of the entire month of July, in fact of almost the entire ice-free season. Figure 6 shows that BYL27 (trough pond) is well stratified during the summer, and we have unpublished data (to be included in a forthcoming paper) showing that polygonal ponds are well mixed (and lakes partly stratified) through most of the ice-free season.

Regarding the temperature profiles (c-d) in Fig. 3 obtained in two ice-wedge trough ponds (BYL24 and BYL27), the bottom temperature was indeed near 0°C because this layer of water is lying just above the melting ice wedge (as part of the "active layer"), does not mix with surface waters, and is cooled down through sensible heat transfer. This is now briefly underlined in the Discussion.

p.11674; l.18: I suggest to be more precise with the term feedback mechanism. Landscape features or elements such as a pond or a lake are not mechanisms per se. These landscape features can introduce processes which are relevant for climate feedback mechanisms. I think, a climate feedback mechanism is e.g. increased CH4 emission of an ecosystem due to warming. This could be caused by the formation or extension of lakes due to permafrost degradation and/or by general changes in the biogeochemical processes due to warmer conditions.

We changed the end of the sentence to avoid any confusion, and did so throughout the manuscript. However, as stated above we define the climate feedback effect based on the difference between 'old' carbon (i.e. in excess in the system, regardless of its absolute age) and 'modern' carbon that is constantly used and recycled through short-term biogeochemical processes.

p.11678, l.22-26: This statement is only true under the assumption that only the number of lakes increases with climate warming while all ecosystem processes remain the same. The presented data give no evidence that CO2 emission or uptake of Arctic water bodies will not change with climate warming. This study investigates the current state of an ecosystem from which the response to climate warming is difficult to derive. Nevertheless, it is an important finding that under current conditions the investigated ponds are sources of CH4 but sinks of CO2. However, this state might or might not change under climate warming.

We agree that our work is valid for the current state of these aquatic systems; we also acknowledge at the beginning of the sentence that we are only considering 'ice-free season ebullition'. We added 'under current climate conditions' at the end of the sentence to clarify this aspect.

p.11681; l.19: Does thaw bulb mean talik?

Yes. We modified that as suggested.

Fig. 4: What indicates the separation line? CH4 and CO2 are already distinguished by filled and unfilled circles.

We added the vertical line to clearly separate CH_4 and CO_2 data, but also to insist on the substantial difference in partial pressure (horizontal axis) for each gas, with at least one order of magnitude difference.

Fig. 5: I suggest to be consistent with units. Does [M] indicate mole? The text uses [m mol] frequently.

The unit [M] ('molar') refers to a concentration (= mol L^{-1}), whereas we used the unit 'mmol' (as in mmol m^{-2} d^{-1}) when referring to fluxes. We think both units are relevant, standard, and commonly used in the literature (International System).