Author response letter for BGD-12-10719-2015 "Effects of permafrost thaw on arctic aquatic ecosystems"

We would like to thank the four referees for their comments. We are pleased to read that the referees find our manuscript timely, critically-needed, and well-written, and that the referees appreciate our structured review of the literature. Below, we answer (black text) to the general and specific comments of the referees (blue text). We have included the suggested changes in a track-changed manuscript as well as a new version of the manuscript without tracked-changes.

Referee #1

This comprehensive manuscript reviews how permafrost thaw influences arctic aquatic ecosystems through their mostly first-order effect on water chemistry and quality. I found this manuscript to be very well put together – it reads well, it is well structured, it is timely and focuses largely on recent research. It ties together a wide variety of literature and ideas, and it offers good suggestions and guidance for future research. It is well suited for the readership of Biogeosciences, particularly given its placement within the special issue. I have relatively minor suggestions.

First, I suggest some attention to be paid to how or whether these processes are included in terrestrial ecosystem models and/or land surface models, or how the many processes described within can guide model prioritization. I recognize this is not the focus of this paper but it would be a laudable goal to at least reference. Using such a focus to wrap up may assist in the ending, which a bit strangely ends on a call to do eddy covariance measurements. These GHG-measuring ideas are fine suggestions but a slightly off-key way to end the manuscript, which would benefit from a slightly stronger wrapping up more focused on the core ideas of the review.

We agree with this suggestion and have now added a brief paragraph ("5.3.5 Inclusion and prioritization in models") at the end of the concluding section. We have also shortened the text on eddy covariance measurements.

p. 10722, line 14 – it is important that the C stock numbers retain the indication that some of this organic carbon is well below 1 m, which is a traditional horizon on which to count these stocks.

We have added "of which about half is deeper than 1m".

p. 10726, line 6 – can you quantify how "widespread" is the disappearance of thermokarst lakes into rivers in Siberia?

We have made this sentence more general, also based on other referee comments. We have also deleted the reference Kravtsova and Tarasenko (2010) but have added another (Smith et al., 2005).

p. 10757, line 6 – including the time scales of change at 30-40 years for this process prompts the question of what are the time scales of all of these processes described within the manuscript? In this context 30-40 years is described as "short" but

elsewhere within 30 years the biological response to P-fertilization was already substantial. As much as possible I encourage the authors to go through the sections as assign likely or characteristic time scales of behavior.
Yes, we see the referee's point here. We want to note that in specific cases (such as Holocene timescales) we had already clarified the timescales in many cases. We have gone through the manuscript and assigned characteristic timescales wherever appropriate, and have replaced terms such as 'short-term' and long-term' with a more specific time scale (e.g., 'years to decades').

Very minor technical corrections:
p. 10733, line 12, remove the comma after "decrease"
p. 10742, line 10, pluralize "distance"
p. 10769, line 20 "ocean-ward" seems slangy – perhaps ocean-bound ?
p. 10772, line 2, "organics" should be organic +noun – matter? Acids? Etc..
p. 10781, line 8, Probably "DOC" was meant for one of the POC's.
Thanks. We have incorporated all the corrections that were suggested.

Referee #2 (Pokrovsky)

The manuscript of Vonk et al (Reviews and Syntheses: Effects of permafrost thaw on arctic aquatic ecosystems.) was examined regarding its suitability for Biogeosciences and was found fully satisfactory from the view point of novelty and scientific quality. The overall added value of this manuscript is extremely high. It clearly represents a reference work in its genre, fully up-to-date, state-of-the-knowledge of aquatic systems in the permafrost-affected regions.
The majority of cited references are dated 2010-2015 showing highly attractive re-search topics discussed in the paper. The summary, feedback and future research needs can be especially appreciated. The organization of the paper is logic, thoughtful and the Discussion is well developed. The Abstract represents fundamental approach of high academic value, thorough and insightful.

The only major shortcoming of this work (which does not necessarily require serious revision) is that "Wetland processes are not specifically discussed in this review" (L 25-26, p.1072). The wetlands represent significant coverage of the permafrost surface and contain huge stock of potentially vulnerable soil carbon. In addition, they contain significant water stock, much larger than that of the slopes.
We recognize this is a shortcoming, but due to the length of the manuscript we have decided to not go into this topic further. We have, however, here added some information on the soil carbon stock of peatlands to emphasize their importance.

In this regard, it would be important to clearly distinguish 1) slopes, 2) river deltas, 3) river valleys, and 4) watershed divides (plateau). In each of these elementary landscapes, the permafrost development is different and often unique. This is highly pertinent to what is stated in L 24-27 of p. 10775. As such, the status of aquatic systems related to thawing permafrost will be different among different landscapes.

A synthetic cartoon of various water landscapes subjected to permafrost thaw would be very welcome but probably too difficult to produce at this stage.
We certainly agree that landscape characteristics are key in determining the degree and type of thermokarst. We are not entirely sure what the referee wants us to change or add here, but we have highlighted the importance of landscape-scale differences in the introduction and in section 5.2 (Climate feedbacks).

Minor comments (in the order of their appearance)
p. 10725, L 11: The number of lakes requires definition of the lake size range
We have added this information (0.1 to 50 km2).

p. 10726, L 6: Lake drainage into rivers: rather, into larger lakes and finally to the rivers (hydrological network), see Kirpotin et al (2008) for western Siberia case (One of possible mechanisms of thermokarst lakes drainage in West-Siberian North. International Journal of Environmental Studies, 65, No 5, 631-635. doi: 10.1080/00207230802525208).
We have made this sentence more general, also based on other referee comments.

p. 10728, L 2: Note the possibility of full freezing of shallow thermokarst lakes in winter and pronounced solute concentration process (Manasypov et al., 2015, BG)
We have now added "if lakes are not frozen to the bottom" to this particular sentence.

p. 10728, L 28: diurnal variations are unlikely to be pronounced during May-July, probably only in August
We have now added "during the peak of summer" to this sentence.

p.10731, L1-4: Is it possible to provide some details on this issue?
We have added a bit more detail to this sentence now, and have added a reference to section 2.2.3, where this issue is discussed in depth.

p. 10733, L 9-10: Consequences of “Thermokarst expansion into new soils at the lake margin” for water chemistry are also discussed for western Siberia frozen peat (Shirokova et al., 2013, Biogeochemistry)
We have added this reference.

p. 10733, L 27-29: Permafrost thaw induced by forest fire and the consequences on stream water chemistry are discussed for Central Siberia zone (Parham et al., 2013, Biogeochemistry)
We have added some findings and the citation of this paper to the text.

p. 10734, L 4-14: DOC (and DIC) fluxes in Siberian rivers across the permafrost gradients are discussed in Prokushkin et al (ERL, 2011), doi:10.1088/1748-9326/6/4/045212.
We have added this reference.

p. 10736, L 8-13: Consideration of local conditions for permafrost thaw is very important remark, often neglected in global estimations. As such, GIS-based evaluation of type of landscapes in the permafrost-affected region is highly needed.
Yes, this is a good point, we have added this to the "Remote sensing" heading in 5.3.4.

Section 2.3. Metal contaminants in thermokarst lakes of different stage of development are addressed in a seasonal scale in Manasypov et al (2015, BG), and the average concentrations of metals in thermokarst lake waters across the gradient from discontinuous to continuous permafrost are discussed in Manasypov et al. (2014, The Cryosphere, doi:10.5194/tc-8-1177-2014)
This was added to the manuscript. See the track changes in the word document. One reference was added (Manasypov et al., 2015).

p 10740, L 28: less degraded? Explain, why are they less degraded than deeper soils? Corg may be tightly linked to minerals in deeper horizons and thus very poorly bioavailable
This sentence might not have been too clear. We have now edited this text. Our assumption here is that litter and near-surface soils (generally) contain a higher labile C content than deeper soils.

p. 10741, L 5-6: Kawahigashi et al demonstrated an increase (not decrease) of DOC flux northward, with the increase of the permafrost coverage on the Yenisey watershed, with the explanation as given below in L 7-10.
Both the text and this referee comment is correct, we just address different things; we talk about "*increasing delivery of biodegradable DOC ... in areas of decreasing permafrost extent*" (southwards). The referee here talks about the flux of DOC not about its degradability.

p. 10742, L 5-7: Specify, that this is relevant to mainly mountainous regions, not on flat surfaces.
It can occur both in flat regions where rivers incise the landscape, as well as more hilly landscapes. The studies and data that are cited are from lowland to somewhat hilly regions, but not from mountainous regions.

p. 10743, L 25-28: Why 320 nm? Normally, this is 280 or 245 nm!
Absorption coefficients at 320 nm are strongly, positively correlated with absorption coefficients reported at other wavelengths (for the same sample set). Absorption coefficients at 254 nm or 280 nm are often reported because they have been correlated with aromatic C content after normalization to DOC concentration (Chin et al., 1996; Wieshaar et al., 2003). Studies interested in light-absorbing and photochemical processes involving CDOM often report CDOM absorption coefficients at 310-320 nm (not normalized to DOC concentration) because (1) this range provides information on the depth of UV light penetration in the water column (see text), and (2) this range is relatively less influenced by absorption from non-CDOM particles (iron, etc; Weishaar et al., 2003), and (3) because maximum

photobleaching and other photochemical processes have been reported in this range (Goldstone et al., 2004, for example). We have, however, not included this information in our manuscript, as these are finding from Cory et al. (2014) and explaining their specific techniques goes beyond the scope of our manuscript.

p. 10746, L 23: is it possible to provide a reference?
A reference, Mann et al., 2014, was added here.

End of section 3.3: Potential bioavailability and speciation of metal contaminants changes progressively, in the sequence of thermokarst lakes development, from small depressions and thaw ponds to large thermokarst lakes the concentration of metals decrease whereas the molecular weight also decreases thus increasing metal – OM complexes bioavailability (Pokrovsky et al., 2011, BG; Shirokova et al., 2013, Biogeochemistry)
These concepts were added to the manuscript, and Pokrovsky et al., 2011 was also added.

Section 4.1.2, L22-25: Similarly, both CO2 and CH4 concentrations were found to be higher in smallest thaw depressions at the beginning of permafrost thaw (Shirokova et al., 2013, Biogeochemistry)
We have added this information and the reference to the text.

p. 10765, L 14-17: What is the mechanism, coagulation of DOC?
We have specified 'mineral adsorption' as the mechanism.

p. 10768, L 20 & 21: 2 times "overall", may be re-phrase
We have removed the second "overall".

p. 10769, River export to the ocean. L 19: this sentence is not totally true: PARTNERS (Arctic GRO) provide such measurements, which are fully compatible with previous and on-going measurements by RusHydromet (concerning Ca, Mg, Cl, SO4, Cl, Si, DIC and DOC, see Gordeev et al., 1996 Am J Sci; Pokrovsky et al 2010 Chem Geol).
We agree that these two projects provide critical measurements of biogeochemistry at the mouths of large Arctic rivers. However, the PARTNERS / Arctic GRO project is still in its relative infancy (~10 year data record, which provides challenges for detecting *change* at this point), while there have been some problems with interpretation pointed out for the RusHydromet dataset (see Holmes et al. 2001, Marine Chemistry). Given that this paragraph was about scaling, while the next paragraph discusses results of long-term measurements, we have deleted this partial sentence from its introduction. This comment from this reviewer served to point out the fact that it was mis-placed in this paragraph.

p. 10770, L 16-19: This sentence is too general to be supported by unpublished data. Explain what exactly S. Tank found for the Mackenzie catchment.
We have removed this citation, and reference to it in the text.

p. 10770: The variation of riverine DIC flux across watershed with different permafrost coverage is thoroughly discussed in Pokrovsky et al., 2015 BGD.
We have included this reference.

p.10772, L 6: In much of the Russian Arctic, organic carbon transport from land to ocean. . .” – relative to the Canadian Arctic and the Mackenzie River?
We have rephrased this and added "relative to the North American Arctic".

p. 10773, L 21: higher OC input? rather, OC export?
We have changed "input" to "export" in this sentence and in one other occasion in this paragraph.

Ref. Kirpotin et al. 2008, not easily available to readers, can be replaced by one of those Kirpotin et al. (2009) West Siberian wetlands as indicator and regulator of climate change on the global scale. International Journal of Environmental Studies, 66, N 4, 409-421, DOI: 10.1080/00207230902753056.
Kirpotin et al. (2011) West Siberian palsa peatlands: distribution, typology, hydrology, cyclic development, present-day climate-driven changes and impact on CO2 cycle. International Journal of Environmental Studies, 68(5), 603-623, doi: 10.1080/00207233.2011.593901.
The finding that thermokarst lakes are increasing in mid-latitudes but decreasing in Southern Siberia is not reported in the two suggested references, so we decided to keep the current Kirpotin et al., 2008 citation.

Referee #3 (Boike)

Overall, I enjoyed reading this review and synthesis paper very much; it is well written and includes an amazing review of literature. The text is about 60 pages long and thus it is sometimes hard for the reader to keep the focus. Thus, the authors might consider re organizing, for example merging and distilling the content of some of the sections. For example, section 4 seems to be a mixture of literature review and synthesis and is partly repetitive to section 1-3.
Overall, I recommend strengthening the synthesis part; what are the new insights and challenges? Furthermore, this paper should also synthesize some of the new results that are provided by the various papers in this unique THAW special issue.
We are aware of the impressive length of the manuscript, but have tried our best to decrease text where possible. Section 5.3 presents a detailed list of future research needs including the challenges we face. We agree that the THAW special issue is a unique opportunity to add additional insights but nearly all of these papers are still in their discussion phase (i.e. not peer-reviewed yet). We therefore have cited THAW special issue papers on individual occasions but want to refrain from making grand synthesis statements.

Some specific recommendations are provided below.

Specific comments, with focus on sections 1& 2

Abstract

-The Arctic is a water-rich region, with freshwater systems covering 16 % of the northern permafrost landscape.

Where does this number come from? Later in the paper, several numbers are given.

Unfortunately, we cannot add citations to the abstract. Later in the introduction, the 16% is mentioned again (now with reference, it is based on the GLobal Lakes and Wetlands Database from Lehner and Döll, 2004). The other numbers that are given are related to the 16% but are just reported per permafrost zonation.

-... lentic and lotic systems

I suggest supplying short explanations for these words (in brackets).

We have added a brief explanation now, reading "lentic (still) and lotic (moving) systems."

Introduction

-The Arctic is extremely rich in water. Lakes, reservoirs, rivers, and various types of wetlands, floodplains, bogs, fens and mires, on average occupy 16 % of the landscape underlain by permafrost (Fig. 1; Global Lakes and Wetlands Database; Lehner and Döll, 2004).

These numbers refer to lake sizes larger than 105 m2. It is important to note that (i) smaller water bodies are not considered and (ii) that these water bodies are especially hot spots for greenhouse gas emissions (Abnizova et al. 2012; Repo et al., 2007&2009).

We have now added the lake size to this statement refers to (waterbodies greater than 0.1 km^2, as specified in Lehner and Döll, 2004). This early on in the introduction we do not want to introduce references that highlight specific findings, but we have added the Abnizova et al 2012 and Repo et al 2007 references (Repo et al 2009 concerns N_2O) and their findings later in 3.5.1.

-In this review we provide an overview of the effects of permafrost thaw on aquatic ecosystems and their potential feedbacks to climate, with a consideration of all aquatic ecosystems located within the permafrost zones defined by Brown et al. (1998; Fig. 1).

I suggest to focus more on the synthesis. What are the new insights that have come out by recent literature, including the recent THAW special issue papers? Did new challenges arise or have some even been “resolved”?

As we mentioned before, we unfortunately cannot include too many overarching conclusions based on the THAW special issue. However, we do present a lot of new insights (the majority of our cited papers are between 2010 and 2015). Albeit brief, section 5.1 ("Summary") is a synthesis of the manuscript and the findings it presents, while our recommendations for future research act to synthesize current gaps in research understanding.

-Thermokarst lake processes , which include lake formation, expansion and drainage, are the most abundant (10–50 % of permafrost-impacted landscapes; Jorgenson et al., 2006; Kokelj and Jorgenson, 2013)... and are particularly emphasized in this review..

Please give an explanation why. Other permafrost related features (for example, ponds) have been identified as hot spots of greenhouse gas emissions.
Here we use "lakes" as a general description of any water body. As we agree that small water bodies are also important, we have here rephrased to "lake and pond formation" and have added references on the importance of ponds further in the text (see earlier answer to comment).

Figure 4: The figure needs revisions. Why are palsas included, but not other permafrost related ground features? Why is there solar radiation in the winter time? Spring solar radiation can warm thermokarst lakes under the ice during spring.
Furthermore, summer thermal stratification occurs in shallow ponds/lakes (< 2 m) and in deeper (> 4 m) lakes (THAW special issue; Boike et al. 2015).
We have adjusted figure 4 to now include "ice-covered" and "ice-free" instead of "winter" and "summer" and have removed the term 'palsa'. The thermal stratification information from Boike et al. 2015 (special issue) is now included in section 2.1.

Section 2.1
-..thermokarst lakes
Please give a definition of thermokarst lakes. It might be important to note that water body maps are based on (optical) surface classification (such as Lehner and Döll, 2004) and they do not provide information on the type and/or genesis of the lake (for example thermokarst or glacial lake).
We have added a brief definition of thermokarst lakes (and ponds) in section 2.1. Indeed, surface classification maps do not distinguish between types of lakes, but in this section we just present the amount of lakes in permafrost regions to illustrate the significant importance of water cover in permafrost areas. It is also worth noting that the scope of our manuscript covers the effects of within-catchment permafrost degradation on lakes, in general.

-They encompass a wide range of physical characteristics, which in turn contribute to the large variations in their biogeochemical properties. In this section, we focus on the effects of thermokarst on the physical and optical limnology of ponds and lakes (Fig. 4).
The biogeochemical properties of lakes and ponds are very different in their characteristics. An important example for an important difference in the physical limnology, shallower ponds and their sediments freeze completely during the winter, while deeper lakes remain unfrozen. Summer stratification is depending on lake depth. Figure 4 thus does not give a complete overview of the physical processes in (deeper) thermokarst lakes and (shallower) ponds.
We certainly acknowledge that depth is an important feature in determining lake characteristics due to depth of winter freezing and have added a sentence on this (section 2.1, 3rd paragraph). Figure 4 is not meant to be a complete overview of all processes in lakes/ponds, but we have added a minor note to the caption to highlight that this schematic does not include shallow ponds and lakes that freeze to the bottom.

-Thermokarst lakes vary greatly in surface area, from ponds that are only a few meters across (Bouchard et al., 2011; Breton et al., 2009) to lakes that are several kilometers in their maximum dimension (Arp et al., 2011; Pelletier, 2005; Pokrovsky et al., 2011). In some thermokarst-impacted landscapes, ponds and lakes cover up to 30% of land surface area (Hinkel et al., 2005; Côté and Burn, 2002)...

While thermokarst lakes dominate in total water surface area, Muster et al. (2013) show for three Arctic sites (Canadian High Arctic, Northern Russia and Alaska) that ponds represented over 95% of the total water body number. This might be an important point considering them as "hotspots" in the permafrost landscapes (Repo et al. 2007&2009, Abnizova et al. 2012; Laurion et al. 2010).

Overall, the numbers on ponds on lake coverage are likely to be (much) higher. For example, the inclusion of subpixel-scale water bodies increases the water surface area of the total land area in the Lena Delta from 13 to 20% (Muster et al. 2012).

This is an interesting addition, and we have added the Muster et al. 2013 reference to section 2.1, 3rd paragraph.

-In summary, thermokarst lakes and ponds occur in wide varieties (Fig. 2) depending on, for example, their formation process, surface area, and depth; from here on we collectively refer to all of these waterbodies as thermokarst lakes.

I strongly suggest differentiating between thermokarst lakes and ponds, due to distinct differences in their physical and biological properties. In the following text, the term "ponds" is still used using varying definitions: page 10752: "thermokarst ponds" are smaller in size and freezing to bottom in winter; page 10756 " trough ponds" are very shallow systems (< 1 m); page 10752: "thermokarst ponds" are smaller in size and freeze to bottom in winter. Lakes and ponds should be defined clearly and used throughout the text.

We have gone back and forth on the level of distinction between lakes and ponds. We certainly agree that pond and lake properties could be very different, but there also are similarities in response to inflow of thawing permafrost OM. The few cases in the text that the referee highlights where we still use the term "ponds" are really pond-specific (e.g. where the shallow depth is a key parameter). We have re-read the manuscript and split lakes and ponds at numerous occasions. A clear definition of the distinction of lakes and ponds is not generally agreed upon, but we have added (section 2.1, 3rd paragraph): ".. *from here on we generally distinguish between ponds and lakes based on their depth (ponds freeze to the bottom in winter, lakes do not) but we also follow the terminology used in the studies we cite*."

-Depending on their CDOM and particle content, the surface waters of thermokarst lakes may strongly absorb solar radiation, and this gives rise to pronounced surface warming of the more colored lakes. In combination with the cooling of their bottom waters by the permafrost beneath, this means that thermokarst lakes can have pronounced vertical thermal and density gradients and may be strongly stratified in the summer despite their shallow depths (Fig. 4; Sepulveda-Jáuregui et al., 2015). Strong thermal stratification during summer has been reported in many thermokarst lakes, for example even in waters less than 2m deep in northern Québec (Laurion et al.,

2010; Deshpande et al., 2015). Similarly, in a north–south transect in Alaska, the lakes on yedoma-like permafrost were typically stratified in summer despite their shallow depths, with less than 0.1mgO2 L1 at the bottom of the water column. On the other hand, lakes in non-yedoma permafrost or non-permafrost catchments tended to be less stratified and had well-oxygenated bottom waters (Sepulveda-Jáuregui et al., 2015). In other thermokarst lakes, little or no stratification has been observed during summer (Burn, 2003; Hinkel et al., 2012; Pokrovsky et al., 2013), possibly as a result of greater wind exposure, increased convective mixing or less near-surface heating...

In general, the entire section "Physical and optical limnology" would benefit from further organization and structure. One important point here is that the thermal regime and stratification is mostly determined by lake morphometry (depth), and the discussion could be organized along this theme.

We do not believe that there is adequate information in the literature to make any generalisations according to depth. Other factors that are likely to affect stratification are latitude (affecting water temperature – warm waters will stratify more easily because of the non-linear relationship between water density and temperature), fetch (lakes with longer horizontal dimensions will be more subject to wind mixing), wind (shear stress increases as the square of wind speed) and water transparency (CDOM-rich waters will strongly absorb radiation, giving rise to stratification). We have now revised this section to draw attention to these other factors, and have rephrased sections to avoid generalisations from the Québec subarctic or other specific regions.

The paper by Sepulveda-Jáuregui et al., (2015) shows stratification only for deeper lakes (> 6 m) which is in agreement with Boike et al. (2015; THAW special issue). However, shallower thermokarst lakes (4-2 m depth) experience strong stratified events only during short periods. In contrast, shallow lakes/ponds (< 2 m) experience strong stratification (Langer et al. 2011). Shallower lakes may be strongly stratified in the summer not despite (as cited in the text), but because of their shallow depths.

The text "despite their shallow depths" has been removed from this sentence.

Is the discussion between the importance of Yedoma and non Yedoma lakes simply caused by differences in their respective lake depths?

No, the oxygenation and stratification differences between Yedoma and non-yedoma lakes is likely caused by a combination of OM input and depth.

-The combination of high rates of bacterial metabolism, small lake volumes and prolonged ice cover means that permafrost thaw lakes can experience full water column anoxia for much of the year,...

This statement is only valid for deeper thermokarst lakes.

We have here added *"(that do not freeze to the bottom in winter)"*.

-..in striking contrast to deeper, less productive lakes in the Arctic such as Toolik Lake, Alaska, and Char Lake, Canada (Deshpande et al., 2015).

These lakes are not thermokarst lakes. Can the comparison still hold?

The aim was to compare with two lakes that are very well known in the Arctic limnological literature, and sometimes used (mistakenly) to generalize about all Arctic lakes. It has now been specified that these are not thermokarst lakes, for readers who are less familiar with high-latitude limnology, and that their chemical and physical properties are very different.

-Presently, there are major gaps in our understanding of the physical and hydrological dynamics of thermokarst lakes, including measurements of heat transfer from the..
After the THAW issue papers, which gaps still remain and which have been resolved? Any new challenges?
We have identified the remaining gaps and challenges in section 5.3. We have included some of the findings of the THAW special issue papers, but cannot use these papers as yet (still in discussion phase) to include any conclusions on whether issues have been resolved.

Section 2.2 ..transfer of material to land
In general, what is the role of the specific hydrology on carbon fluxes, for example differences between low and high gradient watersheds, importance of water balance components (snow and/or rain dominated), varying active layer depths and subsequent groundwater flow? For example, at polygonal tundra sites at the site of Abnizova et al. (2012) in North Siberia, the runoff of the low gradient polygonal tundra site (comprised of lakes and ponds) is very small, only about 10% of the total water budget. This is several orders of magnitude smaller when compared to larger rivers (for example, the Lena River with an annual discharge of about 16 800 m3 s1). Consequently the export of DOC via runoff is low in this watershed, but the CO2 emission from these ponds is very high (Abnizova et al. 2012).
The importance of the "hydrologic connectivity" as another physical determining parameter determining DOC is mentioned in the text and has also been introduced for a high Arctic Canadian site (Abnizova et al., 2014).
This is a good point, and a useful addition to our text. We have added two sentences to the end of the first paragraph of 2.2.1 to address this point.

References (in addition to the papers from the THAW special issue)
Abnizova, A., Young, K. L. and Lafrenière, M. J. (2014), Pond hydrology and dissolved carbon dynamics at Polar Bear Pass wetland, Bathurst Island, Nunavut, Canada. Ecohydrol., 7: 73–90.
Abnizova, A., Siemens, J., Langer, M., and Boike, J.: Small ponds with major impact: The relevance of ponds and lakes in permafrost landscapes to carbon dioxide emissions, Global Biogeochem. Cycles, 26, GB2041, 10.1029/2011gb004237, 2012.
Langer, M., Westermann, S., Muster, S., Piel, K., and Boike, J.: The surface energy balance of a polygonal tundra site in northern Siberia – Part 2: Winter, The Cryosphere, 5, 509-524, doi:10.5194/tc-5-509-2011, 2011.
Muster, S., Langer, M., Heim, B., Westermann, S., and Boike, J.: Subpixel heterogeneity of ice-wedge polygonal tundra: a multi-scale analysis of land cover and evapotranspiration in the Lena River Delta, Siberia, Tellus B, 64, 2012.

Muster, S., Heim, B., Abnizova, A., and Boike, J.: Water Body Distributions Across Scales: A Remote Sensing Based Comparison of Three Arctic Tundra Wetlands, Remote Sensing, 5, 1498-1523, 2013.
Repo, M. E., J. T. Huttunen, A. V. Naumov, A. V. Chichulin, E. D. Lapshina, W. Bleuten, and P. J. Martikainen (2007), Release of CO2 and CH4 from small wetland lakes in western Siberia, Tellus, Ser. B, 59, 788–796, doi:10.1111/j.1600-0889.2007.00301.x
Repo, M. E., Susiluoto, S., Lind, S. E., Jokinen, S., Elsakov, V., Biasi, C., T., V., and Martikainen, P. J.: Large N_2O emissions from cryoturbated peat soil in tundra, Nature Geoscience, 2, 189-192, 2009.

Referee #4

This is a critically needed review of thermokarst systems in northern regions. The authors have provided a comprehensive background on how permafrost thaw influences aquatic ecosystems and provided several notes on what is lacking in current literature on this subject. While the manuscript is generally well written, I find several areas that are a major concern.

First, and foremost, I believe the authors do an injustice to the importance and the undoubtedly major effort that was required to put this together by presenting a manuscript of 50+ pages. Indeed, the manuscript reads more like a textbook than a paper with the authors having to refer to various sections in text (e.g., "See Sect. 2.2"). While I understand that this is a large subject, there are several means to reduce the size of this manuscript and better present the information. For example, I would strongly suggest splitting lotic and lentic systems into two separate papers. These two subjects are often squished together interrupting the general flow of the manuscript.

We certainly recognize that the manuscript is somewhat lengthy, but have tried to organize it as well as possible to guide the readers. We purposefully wanted to combine all aquatic systems (lentic and lotic systems) to target both the stream and lake "scientific communities" that tend to be rather separated. Organizing the manuscript in this way also allowed us to identify similarities across these two system types, such as for the input of thaw-induced permafrost OM into streams and lakes. However, because there are also clear differences between streams and lakes we have chosen to devote specific sections to each of these systems, as appropriate.

Second, the authors generalize large statements without the support of literature or evidence. There is a difference between ecosystems in subarctic Quebec and those of Siberia, Alaska, and the Arctic. Yet, I found several instances of simply incorrect or conflicting statements being made with the use of very regional examples that the authors are themselves familiar with. It is not appropriate to generalize the entire circumpolar north with examples from subarctic Quebec. Indeed, I noted several places in which literature did exist to support some of the statements being made, but were not referenced. The authors are experts in their field, but a wider literature review beyond their own research locales is necessary for a manuscript such as this.

We agree that the Arctic is a large and diverse region, and we are aware that we cannot generalize too much with examples of specific regions only. Instead, our intention was to highlight specific examples of various regions to illustrate local processes, and to highlight differences between regions. Thanks to the multiple reviewer comments, we have now broadened our reference list. We have also revisited our text to be more broad with examples from different regions, to generalize less when possible. We have also (i) added examples from the eastern Canadian Arctic, and (ii) specified when a particular reference is from a certain region (instead of presenting it as a general statement).

As with any collaborative multi-authored paper I found many differences in spelling, grammar, and style that will need to be harmonized. The oxford comma is sometimes used, sometimes not. English spelling is sometimes used, sometimes not. Referencing order should be oldest to newest or newest to oldest, but it seems to switch around in various sections (I have attempted to note all of these, but my ability to do this over 50+ pages waned in the later sections). It would be helpful if a single person could go through and harmonize the writing style of each section.
We have re-read the manuscript and have now (i) been consistent with English (British) spelling and (ii) have inserted oxford commas throughout, and have (iii) listed references from oldest to newest.

Several sections had a very poor writing style in regards to referencing specific examples. Instead of referencing the specific examples presented, words like 'A study showed', 'A synthesis of', and 'Studies found' were used. This is an inappropriate way to discuss a single study. Reference the specific study being discussed so the reader knows what and who you are talking about, especially if it is only one study being discussed. Vonk et al. (2015) found that... instead of "A synthesis study of... (Vonk et al. 2015).
We wanted to avoid starting many sentences with a reference, and therefore we often chose to present the finding and cite the reference it originated from in the end. One can consider this a matter of style. We have, however, changed this at numerous locations throughout the text, to minimize this effect.

Specific Comments:
Abstract: 10721 Line 2: "The thawing of this permafrost" is an awkward way to start the sentence. Do you mean permaforst thaw?
We have adjusted this.

Line 13: replace 'modifying variables' with 'modifying factors'
We have adjusted this.

Lines 14-20 are awkwardly written. Causes a break in the flow of the abstract
This first sentence is indeed rather long and we have now added (i), (ii) and (iii) to help guide the reader.

Introduction

10723 lines 3-5 and 13-15 are repetitive

Line 13-15 talks about pulse versus press thaw types, and are not a repetition of the more general statement in line 3-5. We have slightly rephrased the second sentences, though, to avoid the sense of overlap.

10724 line 28 needs a comma after "optical and chemical limnology," Indeed, here and elsewhere oxford comma's are required in a string to be consistent with other portions of your paper. Sometimes the oxford comma is used, sometimes it is not. Likely a factor of multiple authors.

We have inserted oxford commas throughout the manuscript.

10725 Lines 10-15 are repetitive

These lines targets the abundance of lakes specifically, whereas the percentages that are discussed in the introduction refer to aquatic systems in general. Therefore, we have chosen to keep this text in the manuscript.

10726 lines 5-6 say that 'thermokarst lake disappearance by drainage into rivers is a widespread scenario in Siberia' – well yes, and also the NWT, YT, and Alaska. Many references could be used here..

We have made this sentence more general, also based on other referee comments. We have also deleted the reference Kravtsova and Tarasenko (2010) but have added another (Smith et al., 2005).

Line 14: Description is northern Quebec, but elsewhere in the manuscript it is listed as 'eastern Canadian Arctic'. Actually, after going back through the manuscript, many of the examples are from references in northern Quebec but generalized as the entire Arctic or the eastern Canadian Arctic. I find that many of the examples are from northern Quebec and very few are from the eastern Canadian Arctic.

As is also highlighted above, we have carefully re-read the manuscript and have (i) added examples from the eastern Canadian Arctic, and (ii) specified when a particular reference is from a certain region (instead of presenting it as a general statement). This included specifically stating when references are from northern Québec.

10727 Line 28: I find the generalization that thermokarst lakes can have profound vertical thermal density gradients and strongly stratified unsupported by literature. Many thermokarst lakes have been shown not to stratify in northern environments (Chris Burn published on this in the NWT). This is due to large differences in CDOM as well as regional climate. Darker lakes in warmer regions will stratify, ultra oligotrophic lakes in colder lakes won't necessarily. Here and elsewhere the authors conflate thermokarst systems in ultra-oligotrophic Arctic regions with subarctic regions.

This paragraph and the previous have now been modified to better differentiate between thermokarst lakes in subarctic regions, and Arctic thermokarst lakes in tundra landscapes which demonstrate much less stratification. We now also note the multiple factors that affect stratification.

10728 Line 3: I find the sentence “strong thermal stratification during summer has been reported in many thermokarst lakes” not to be supported by any evidence other than in lakes in northern Quebec that have a very unique limnology. The references provided for this statement are only from subarctic Quebec.
This paragraph has been modified.

Line 6-8 is not supported by a reference
This reference has now been included.

Line 10-13: Here the authors acknowledge that some thermokarst systems do not stratify.. however, there are more references and examples of these systems not stratifying than support for those that do. I find little evidence to support the generalizations the authors are making in this section.
This paragraph has been modified to avoid generalization.

Line 20: no reference provided for this statement
References have now been included.

Line 28: “Additionally, there may be strong diurnal variation in stratification and mixing”, there is no support for this. Reference the study.
References have now been included.

10729: Line 16: Oddly using examples of toolik lake, and char lake to contrast northern quebec examples. Why these two lakes?
As noted above, the aim was to compare with two lakes that are very well known in the Arctic limnological literature, and sometimes used (mistakenly) to generalize about all northern high latitude lakes. It has now been specified that these are not thermokarst lakes, for readers who are less familiar with high-latitude limnology, and that their chemical and physical properties are very different.

- This section is using ’favor’ while other sections use the english spelling for several common words, e.g., favour.
We have corrected the language throughout the manuscript for US English spelling, and have instead followed British English spelling as Biogeosciences is a European journal.

10730 line 20, needs an oxford comma after ’play out for nutrients, DOC,’
OK.

10731 line 10: I know TEF was introduced earlier, but the paper is so large that it may help to redefine it again..
OK, we have done this.

line 24: requires reference

This sentence is an overview statement of the paragraph it is followed with, and appropriate references can be find further on in that paragraph.

10737 line 1: remove the word 'remote'
OK.

- Capitalize 'Arctic', it is a noun.
We have reformatted the manuscript to follow the Arctic Institute of North America style guide (http://arctic.ucalgary.ca/guide-authors), and now capitalize 'Arctic' when it is a noun or refers to the geographic region.

Lines 13-17: I actually expected the order of the section to follow the major consequences presented here.. but it seems as if it is an overly small section and then contaminants are discussed elsewhere in the manuscript. Seems like it was split up and redistributed.
We had decided to treat contaminants as one of the fluxes that are released from thawing permafrost (just as e.g. DOC, ions, etc.) and have therefore chosen to divide contaminants over the different sections in the manuscript (2.2 Transfer of material from land to water; 3 Degradation). We agree that section 2.3 is a bit separated and have decided to change 2.3 into 2.2.6 to include it in the section that targets transfer of material from land to water.

10738: Line 10: "Few studies have yet", which few?
We changed this to "Studies of inorganic contaminants…" and the exact studies used are listed further down in this paragraph (Manasypov, 2014; Klaminder et al., 2008; Rydberg et al., 2010; Deison et al., 2012; MacMillan et al., 2015).

Line 18: "One study found" this is a very poor way of writing this. Be specific, if you are discussing a specific study, be specific about it. Deison et al. (2012) found that lakes..
We have changed this.

Line 22-26: Run-on sentence, please reword
We have reworded this, and split it into two sentences.

10739 lines 1-5: Run-on sentence, please reword.
We have rephrased this.

Line 15: need an oxford comma after the word solubility
Thanks, we have added this.

10740 line 5: This sentence needs comma's... "Biological processing of DOC occurs prior to, and upon, hydrologic..."
We have added commas.

Line 24: Another example of poor writing style. If you are discussing a specific study, be specific about it. "Yonk et al. (2015) found that BDOC in Arctic soils and surface waters..."
We have rephrased this.

10741 line 12: requires comma after 'sources, rates, and'
Comma added.

10742 line 5-7: Unpublished data is not appropriate to use here to support this statement.
We have deleted the references to unpublished datasets. In support of our statement, we retain one published study that has directly compared DOC and POC exports from thermokarst features. We have also added studies from two other regions where data exist for *suspended sediment* and DOC export from thermokarst features, and remind the reader of the typical 1-2% conversion between suspended sediment and POC in aquatic systems.

Line 26: reference ordering is not consistent. Oldest reference to newest reference. Move Laurion and Mladenov, 2013 before Hong et al. 2014.
We have corrected this here, and at other places throughout the manuscript.

10743 lines 3-5: Run-on sentence
We have reworded this sentence.

Line 12: reference ordering is incorrect, oldest to newest.
Line 18: reference ordering is incorrect, oldest to newest.
We have corrected both.

Line 22-23 "The many shallow lakes across the Arctic often contain high concentrations of light absorbing CDOM" This is a very sweeping statement and its not true as written. Remove.
All the references cited and discussed in this paragraph support the statement that shallow lakes in the Arctic contain high concentrations of light-absorbing CDOM (Hobbie et al. 1980, Gareis et al. 2010, Watanabe et al. 2011, Cory et al. 2014), consistent with the high concentrations of light-absorbing DOC draining from the soils these lakes (e.g., as studied in Judd et al. 2002, Merck et al. 2012). As discussed in this paragraph, some lakes have relatively low CDOM values (e.g., lower than the mean reported from a dataset of > 1000 lake samples from 380 lakes across the 3000 km^2 Kuparuk River Basin in the Alaskan Arctic), but the average and range reported across these studies is consistently high.

10744: lines 14: need commas! "though CDOM concentrations, and thus light attenuation, are often..."
We have changed this.

10746 line 12: "Some trace metals, but as a source for others" –> such as?

This was changed to a more general statement, and the specific examples of metals are given later in the text.

Line 13: comma required after hypolimnia
Comma added.

Line 19: "Studies have shown" such as? As written it seems that this is just a statement pulled out of Tseng et al. 2014 rather than actually outlining the studies that are being referred to. This is not appropriate in scientific writing...
We have corrected this.

Line 22-24: What about turbidity?
True, we have added this.

10747 line 5-7 amend to read "Deepening of the active layer increases microbial diversity and allows microbes to speed up the degradation and transformation of some contaminants" (and include a reference to support this statement).
This sentence was combined with the previous sentence, which included the appropriate reference for this statement.

Line 7-8 is repetitive. Integrate with previous.
We disagree as the second sentence also talks about temperature and nutrient effects.

Line 10-12: where can this occur?
In the eastern Canadian sub-Arctic, which we have added.

Line 17: "Studies in thermokarst lakes in Siberia (Manasypov et al., 2014) have shown" Again, you are referring to ONE study. Use the singular reference. Manasypov et al. (2014) have shown. This is the appropriate way of writing about a singular study.
OK, we have corrected this.

Line 25-26: Repetitive from last page
The statement on the first page was removed.

10748 Lines 1-3 So no studies exist that show this?
This was expanded into another paragraph with references.

Lines 4-13: who is doing this? Nothing is published? Line 13-15: requires reference
We have added references here.

Line 22: "A subsequent molecular study combined" WHO? McCalley et al., 2014? Then be specific and state that. "McCalley et al. (2014) showed that the abundance..."
We have changed this.

10749 Line 24: What does the 'Fall' mean in the Arctic? Actually, again the authors are applying a limited example of a very unique region in northern Quebec as support for processes that occur pan-Arctic? Fall in Alaska is very different from fall in Quebec..

We have rephrased this sentence so that it is more specific. Also, we have gone through the manuscript and have clarified the use of examples from specific geographic locations, instead of generalizing.

10750 Line 13: The use of unpublished data is not appropriate as support for this section.

This paper is now in discussion in Biogeosciences, and we have added it to the reference list.

10750 - Why are we transitioning to rivers all of a sudden?

Rivers receive permafrost thaw waters throughout their catchment and can be used to monitor large-scale patterns. We realize the transition was a bit sudden and have tried to introduce it slightly better.

10751 line 17: is it necessary to say that methane is a potent greenhouse gas in a scientific publication with an audience such as this?

Maybe not, we have rephrased.

10752 line 6 requires references

We have added the reference Laurion et al. (2010) here.

10756 line 12-14: I do not understand what the authors are trying to say here... reword

We have removed these sentences.

10759: I find this entire section to be a bit of a scope-creep and could be removed or heavily shortened.

We have shortened this section but want to keep the main messages; the release of old permafrost C into aquatic systems, and the techniques that are available to trace it.

10765 line 6: Why?

This is the first topical sentence. We have slightly edited this sentence for clarity but there is no room to explain all details here. The details are instead outlined in the rest of the section.

Line 12-13: Convoluted sentence, clashes with previous sentence.

We have slightly revised these sentences.

Line 15: reference order is incorrect, oldest to newest.

We have corrected this.

Line 16: Oxford comma required after 'lower DOC concentrations, and' Line 24-30 is only true if lakes remain clear

We have added the comma. We do not agree with the comment about lake clarity. A major driver for these changes in basal production and resource use by higher consumers input of terrestrial DOM is the associated decrease in light conditions (Ask et al. 2009 L&O, Solomon et al. 2011 L&O, Karlsson et al. 2009 Nature, Karlsson et al. 2015 Ecology, Seekell et al. 2015 L&O).

10766 Line 13-15 There is no evidence to support the statement "Increased rates of carbon import and decreased O2 concentrations with further permaforst degradation may particularly affect zooplankton and macroinvertebrate communities in the future".

We agree that there is no evidence of effect of O_2 on higher consumers with permafrost degradation and therefore we had expressed this as that it 'may' affect the communities. We have now rewritten this part to make it more clear and also added references to studies suggesting negative effects of low oxygen on higher consumer abundance.

- Actually, In this review I find very little discussion of biotic communities at all. Most of the focus is biogeochemical and microbial with very very little discussion of heterotrophic community structure of these systems.

We agree that the discussion of heterotrophic communities and their structure in northern aquatic systems subject to permafrost thaw is fairly limited, but this is also due to a lack of available literature, we have therefore identified this as key research directions for the future (in section 5.3.1 under the heading "Trophic structure and food web processes" we highlight "*Autotrophic and heterotrophic communities, and their interaction*", as well as in the heading "Resiliency of stream ecosystems to direct thermokarst impacts").

10768 line 13: "These communities are generally patchy" - not sure what the authors are talking about here...

We have rewritten this sentence to clarify.

Line 18: Why are the authors using southern references to discuss benthic macroinvertebrates. To further my previous point, the biotic discussion in this review is very limited and has almost no northern references to speak of.

We have removed the southern references and elaborated on the benthic macroinvertebrate discussion. We have also acknowledged the fact that there are no available studies on the impacts of thermo-erosional events on the benthic community structure of arctic stream ecosystems.

Line 19-20: This statement is simply not true in all northern regions.

This statement refers to the study by Larouche et al. 2015, and does not need to be applicable to the circum-arctic.

10769 line 4-6: There are many references that could support these statements, please review the literature and cite some.
We have cited a number of references here.

10770 line 11-12: unpublished data should not be used to support this statement.
We have removed the reference to unpublished data.

10771 line 29: The reference is L.G. Anderson? This should be Anderson et al. 2009a.
This appears to be a mistake made during typesetting, as it was not in the original manuscript we submitted. We have corrected this.

10771 line 18: reference order is incorrect
We have corrected this.

line 24-30 run-on sentence, reword.
We have re-phrased as two sentences.

10774 line 2 comma required after availability, temperature, and prior...
Corrected.

10777 line 10: title says lake ecosystems in it, but I find nothing about lakes in this little paragraph.
We have removed "lake" from the title.

Line 17-30: The structure and functioning of Arctic aquatic systems is not poorly studied. There are many examples of studies that examine this in the literature... Indeed, wasn't that a point of this review in the first place? This statement has me a bit in disbelief after reading 50 pages.
We agree that this text, as written, came across as being a bit broad. Our intention here was to highlight the fact that we still know little about how permafrost thaw affects food webs and trophic structure in aquatic systems. As pointed out by this reviewer, this is an area where available literature is particularly weak! We have changed this section to shorten the text, and clarify (narrow) the scope. We have removed the broad text referring to "ecosystem structure and function" and replace this with "autotrophic and heterotrophic communities, and their interaction".

10779 line 2: should probably reference edwardson et al., 2003 when discussing northern hyporhetic zones [Edwardson, K. J., Bowden, W. B., Dahm, C., & Morrice, J. (2003). The hydraulic characteristics and geochemistry of hyporheic and parafluvial zones in Arctic tundra streams, north slope, Alaska. Advances in Water Resources,26(9), 907-923.]
We have added this reference.