

## Review of ‘Heterogenous CO<sub>2</sub> and CH<sub>4</sub> content of glacial meltwater of the Greenland Ice Sheet and implications for subglacial carbon processes’

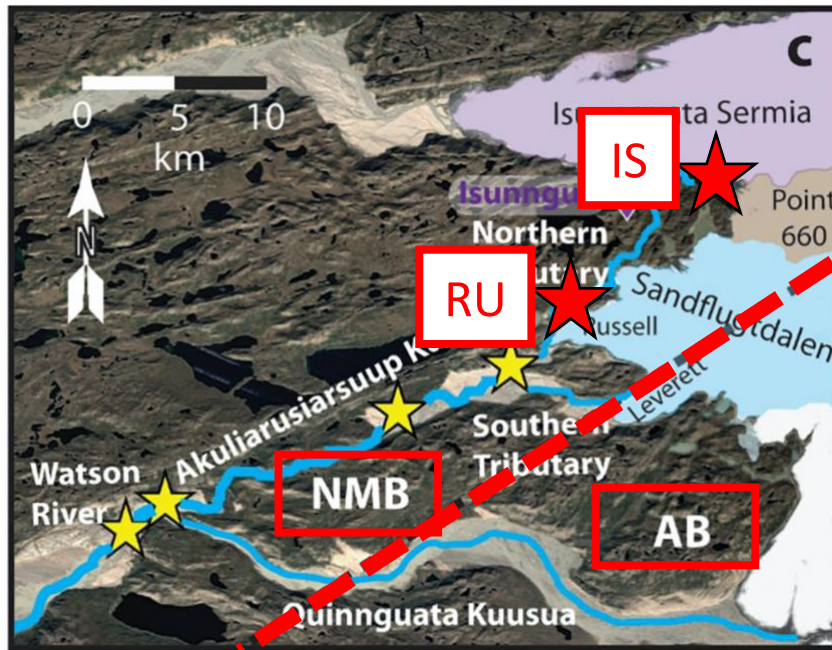
We thank the reviewer for his comments and have addressed them in detail below. We see that the review describes three major issues. One is based on the hydrologic characteristics of our field sampling sites (and field site names as mentioned in other reviews). This issue is linked to a second issue related to atmospheric-stream water exchange between subglacial outlets and the stream water. The final issue is our presentation of effects of sub-glacial processes on production and consumption of methane and CO<sub>2</sub>. We agree that many of these points are important to expand upon in our discussion while simultaneously recognizing and evaluating potential speculation. However, regardless of these unknown factors associated with the field sampling and locations, we think the data presented here still yield important information concerning the heterogeneity of greenhouse gas production and consumption caused by multiple sub-glacial processes, which we discuss, and that the heterogeneity complicates assessments of impacts to atmospheric compositions during glacial retreat, for example since the Last Glacial Maximum and into the future in a rapidly warming Arctic with continued ice sheet retreat.

The authors measured CO<sub>2</sub> and CH<sub>4</sub> concentrations and C stable isotopic signatures in meltwater samples from three marginal catchments of the Greenland ice sheet (GrIS) to evaluate sources and sinks of these important greenhouse gases under the fast melting ice sheet. The study is timely, wellstructured and -written, and uses adequate methods (with exceptions described below). However, interpretation of some of the results (especially the CH<sub>4</sub> part) relies on incorrect assumptions and/or is unsupported by data, and is therefore too speculative at best. The issues listed below need to be carefully addressed before the study can be published.

First, the authors misunderstand and/or misrepresent the regional hydrology of the Kangerlussuaq area. The large outlet Isunnguata Sermia drains into the Isortoq River/Isortup Kuua (N of the Watson River catchment); its catchment is indeed quite large and extends deep into the ice sheet (probably to the ice divide). However, the water samples collected in this study did not originate from this large catchment and using the name Isunnguata Sermia is erroneous. Rather, the authors’ IS site falls into the Point660 subcatchment sensu Lindbäck et al 2015 (as the authors admit at line 88), with an area of ca 30-60 km<sup>2</sup>. This is part of the Russell Glacier catchment system (sometimes treated as part of the Leverett Glacier catchment, eg in Lindbäck et al. 2015), which is complicated, but likely does not reach far into the ice sheet. Calculating the Russell catchment area (for the RU site) as the difference between two individual and independent estimates of the Leverett catchment is incorrect (for example, some authors estimated the Leverett catchment at >1000 km<sup>2</sup>); the catchment feeding the Russell river is likely much smaller than 300 km<sup>2</sup> (see eg van de Wal and Russell 1994). Figure 2b attributes the name Watson River to the river system originating at Point 660 and joining the larger river discharging from Leverett Glacier, which is problematic. Whereas the river terminology in the area is indeed somewhat confused, Watson River usually refers to the last short section of the river formed by the confluence of Akuliarusiarsuup Kuua and Qinnguata Kuussua which then enters Kangerlussuaq where the hydrology data used in the study (from van As et al 2018) were taken.

We agree these suggested changes are important to be consistent with previous geographic naming schemes. However, our assessments do not include flux calculations or specific yield estimates, which would depend on the catchment area of the glaciers studied, and proglacial processes are not in the scope of these manuscript. We will make the changes suggest as outlined below, but the changes in nomenclature will not greatly impact the presentation or interpretation of results or our findings, which show differences in subglacial carbon processes that result in heterogeneous concentrations of CO<sub>2</sub> and CH<sub>4</sub> in subglacial discharge.

The Isunnguata (IS) catchment we refer to is named as such because the watershed lies in between the Isunnguata and Russell (RU) glaciers, as described in delineations by Rennermalm et al. (2013) and the IS site is located at the southern edge of the Isunnguata Glacier. Previous work at the IS and RU sites in this study suggest differences in the subglacial lithology resulting from a geological contact in this area between the Nagssugtoqidian Mobile Belt (NMB) and Archaean Block (AB; Deuerling et al., 2019). These data indicate that our IS site predominantly drains a subglacial environment with a lithology more like the main Isunnguata Sermia catchment, which is believed to be mostly NMB, while our site at the Russell Glacier (as well as Leverett Glacier) drains mostly AB rocks (Fig. R1; Deuerling et al., 2019). We will include in revisions the uncertainties mentioned in the catchment size of the Russell but believe that the Isunnguata catchment in this study is appropriately named. We will also include a more thorough description of the tributaries to the Watson River, with an updated map to indicate the differently named segments of the river.

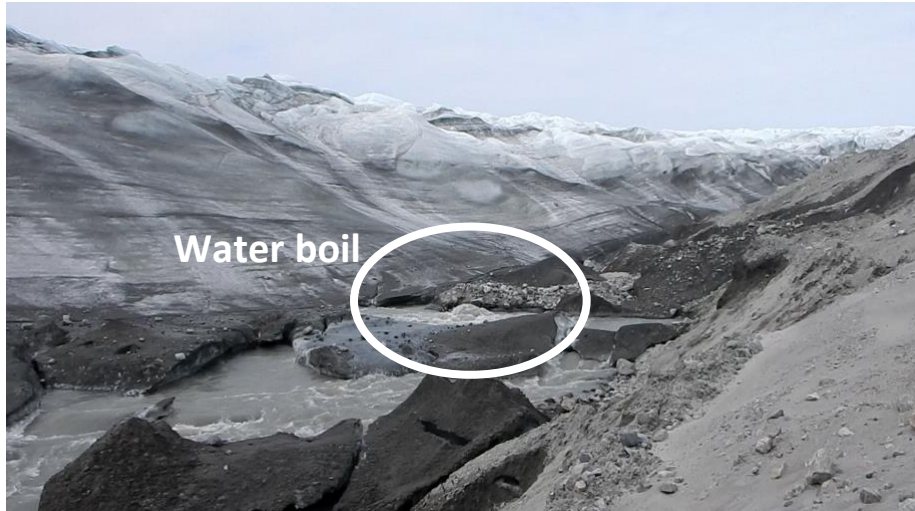


**Figure R1.** Map indicating location of IS and RU sampling points (red stars) and the approximate lithologic boundary between the Nagssugtoqidian Mobile Belt (NMB) and Archaean Block (AB) (dotted red line). Figure modified from Dawes (2009) and Deuerling et al. (2019).

Moreover, it is difficult to compare the study sites with previous works as the authors neither give details of their positions nor show any pictures. I assume the IS site is identical to the site used in Christiansen & Jørgensen (2018). The CH<sub>4</sub> supersaturation referred to at lines 107-108 was measured in the air, with respect to atmospheric concentration, not in the water. The stream itself is very small (discharge at the portal was ca 1 m<sup>3</sup> /s at peak melt in July 2018) and partly/mostly of supraglacial origin (a supraglacial/marginal stream can be found flowing into the ice margin several hundred meters above the portal). In terms of the RU site, it is not clear whether the authors sampled the main stem of the Russell Glacier river or one of the short subglacial tributaries flowing into it from the Russell Glacier margin (as depicted eg in Dieser et al 2014). The Russell Glacier river in this area has already flowed through a number of lakes and the CH<sub>4</sub> signal would be difficult to interpret as purely subglacial. The order of magnitude difference in CH<sub>4</sub> concentrations reported here and in Dieser et al (2014) is not discussed in the text but suggests that indeed samples were collected from the main stem of the river. A better description of the sampling sites is essential for an adequate assessment of the authors' interpretation.

All samples were collected from the respective main river stem as close as possible to the subglacial discharge site. Water did not flow through lakes directly in between glacial outlet sites and sampling sites at Isunnguata and Russell sites, though lakes are present further upstream of the Russell site and a broad and slow-flowing river channel was present between the glacial outlet and sampling point at Kiagtut Sermiat (referred to as a lake in this review). However, in all cases, dissolved CO<sub>2</sub> and/or CH<sub>4</sub> were out of equilibrium with respect to atmospheric concentrations, indicating that subglacial processes altered concentrations and that exchange with the atmosphere between the glacial outlet and sampling site did not erase (but we acknowledge may have altered) the subglacial carbon signal. While exchange would impact gas concentrations and isotopic compositions, there are multiple lines of evidence that suggest the observed chemical signals are predominantly controlled by subglacial processes, which we discuss further throughout this review (e.g. p. 9).

We agree that in addition to the GPS coordinates that were provided in the original manuscript, better descriptions, including the inclusion of pictures of sampling sites, would be valuable to maximize inter-comparisons between this and other nearby studies. We include pictures and more detailed site descriptions below.



**Figure R2. IS sampling site (photo taken July 15<sup>th</sup>, 2018)**

Isunnguata samples were collected from a site downstream of Point 660 where water flows from a subglacial discharge site that produced a visible boil at high discharge. No flow occurred further upslope of the boil, which was most clearly observed during the peak melt season (photo taken July 15<sup>th</sup>, 2018), suggesting it is the principal water source to the stream. As indicated by the photo, the water contains high concentrations of suspended sediment and is predominantly subglacial, with only minor observed contributions of supraglacial meltwater. This location is very close to the site discussed in Christiansen and Jørgensen (2018), where high atmospheric CH<sub>4</sub> concentrations were interpreted to reflect CH<sub>4</sub> supersaturation of the subglacial meltwater discharged at this site, which we now document in this manuscript.



**Figure R3. RU sampling site, photo taken on July 22<sup>nd</sup>, 2018**

Russell Glacier water samples were collected just downstream of the above pictured ice wall (Fig. R3) and thus water did not flow through any lakes directly in between the glacial outlet and the sampling location in this segment of the stream. While sampled water is a combination of recently discharged subglacial meltwater from under the Russell Glacier and proglacial discharge from further upstream (contributions from the Isunnguata at point 660 and other subglacial outlet sites as this segment of the river flows long the toe of the Russell glacier), gas concentrations are out of equilibrium with respect to atmospheric concentrations, suggesting the gas signal reflects subglacial processes because sites just upstream of this site are close to equilibrium with respect to atmospheric CO<sub>2</sub> and CH<sub>4</sub>, with distinct increases in CH<sub>4</sub> concentrations downstream of the ice wall, indicating subglacial water contributions. While we did not include sites upstream of the RU glacial discharge site in this paper, gas concentrations at upstream locations can be found at doi:10.18739/A2PC2T94. If appropriate, we will present these data in a revised manuscript or supplemental information.



**Figure R4. KS sampling location (photo taken June 16<sup>th</sup>, 2017)**

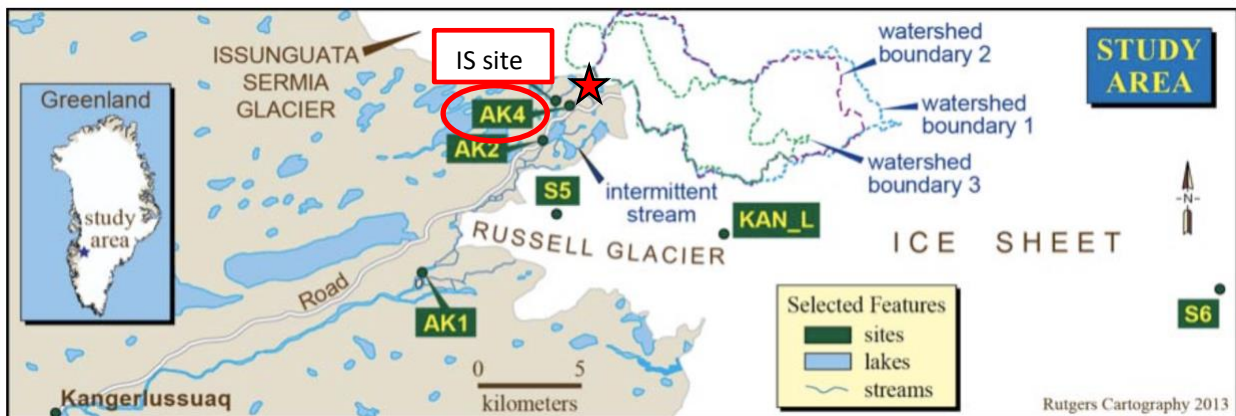
The KS sampling location occurred as close as possible to the glacier outlet, seen in the center background in the picture above. The distance between the glacial outlet and our sampling site is roughly 1 km, however the river flow is slow with no rapids and therefore gas exchange should be minimal compared to that of the Watson River, in which rapids lead to more degassing. We additionally observe constant undersaturation of CO<sub>2</sub> with respect to the atmosphere at this site, indicating that water has not yet equilibrated with respect to atmospheric gas concentrations. Since CO<sub>2</sub> was still out of equilibrium with the atmosphere at our sampling point, CH<sub>4</sub> should also have been out of

equilibrium if it was considerably different from atmospheric concentrations in the subglacial environment.

While the inaccuracies in the description of the regional hydrology are easy to fix, they led the authors to a more problematic aspect of the study: correlating the CH<sub>4</sub> and CO<sub>2</sub> concentrations and isotopic signals from the IS and RU sites with discharge data from the Watson River. While both the Russell Glacier and Watson rivers show clear diurnal variations in discharge, large scale dynamics (including subglacial outbursts) observed in the large rivers is missing in the smaller Russell Glacier river, possibly due to the buffering effect of the lakes along the course of the river, as illustrated in the figure below comparing unpublished discharge data in m<sup>3</sup> /s from LG (Leverett Glacier river), RU (Russell Glacier river), and IS (Isunnguata Sermia river/Isortup Kuua) in the summer 2018. The Watson River discharge dynamics may be further complicated by the unaccounted for Qinnguata Kuussua, which provides more than half of its water. This is in contrary to the authors' assumptions (line 217).

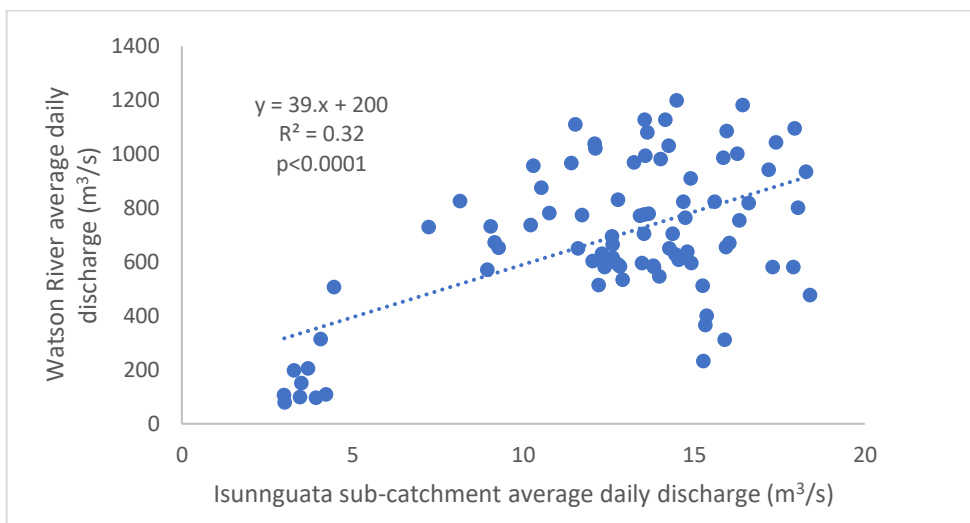
To my knowledge, this dataset has been made available to the authors, and it might be beneficial for the authors to use it for their correlations. Maybe it was not used because there were just 2 and 3 samples collected at RU and IS, respectively, in 2018 (as shown in Figures 3, 4, 6, 7, 9)?

The reason we do not use the dataset that is referred to in this review is because it has not published yet and would only be applicable for 2018. However, we agree that the varying scales of water contributions from the Isunnguata and Russell glaciers compared to that of the Watson River could result in non-proportional discharge between individual glacier discharge sites and the Watson River. These differences could impact the correlations we observe between gas concentrations and discharge. We address this issue by comparing Watson River daily discharge collected by PROMICE (As et al., 2018) to data collected by Rennermalm et al., (2013), which measured discharge just downstream of our Isunnguata sampling location (Fig. R5). We compare average daily discharge between both sites for the first year that complete melt season datasets were available from both sites (2009).



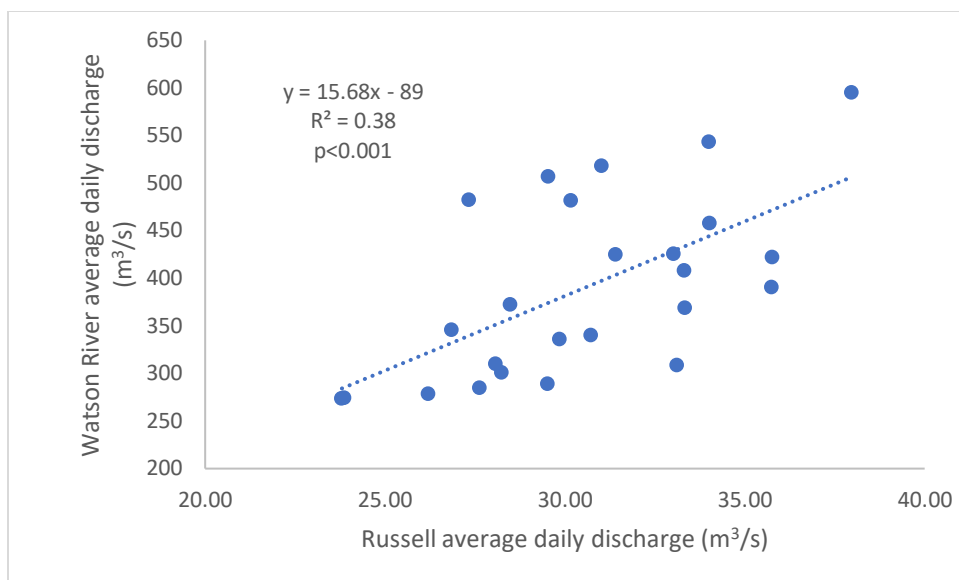
**Figure R5. Isunnguata (IS) site from this study, indicated with red star, compared to location of gauging station (AK4) in Rennermalm et al. (2013). We will use 2017 and 2018 data from this gauging station to address concentration-discharge relationships in the IS catchment. The catchment of this watershed lies between the**

Isunnguata and Russell glaciers as indicated above and its area is estimated between 36-64 km<sup>2</sup> (figure modified from Rennermalm et al., 2013).



**Figure R6. Comparison of discharge data between the Watson River (PROMICE) and the Isunnguata/Point 660 catchment (Rennermalm et al., 2013). Average daily discharge is compared between June 9 and September 1, 2009**

While the discharge values are significantly positively correlated (Fig. R6;  $p < 0.0001$ ), suggesting that assuming a rough proportional relationship between the smaller Isunnguata sub-catchment and the much larger Watson River is reasonable, the correlation is only of moderate strength. We therefore agree that concentration-discharge relationships would be better represented by discharge data from the Isunnguata sub-catchment. We are therefore collaborating with Asa Rennermalm, who will be a co-author on the revised manuscript, and who has collected discharge data from the Isunnguata sub-catchment over 2017 and 2018 that will soon be available. While we do not have such information for the Russell Glacier, Watson River discharge increases between the Isunnguata catchment and the Russell Glacier, making Watson River discharge a more appropriate approximation than for the Isunnguata. We test this assumption by comparing Russell discharge data (from unpublished source indicated in this review) from the record indicated with Watson River discharge in 2018. While only a few weeks of Russell glacier discharge is available, Russell average daily discharge is significantly positively correlated to Watson River average daily discharge ( $p < 0.001$ ), suggesting that assuming a proportional relationship is appropriate (Fig. R7). Additionally, because our analyses are not intended to be quantitative (e.g. no flux estimates are reported), and simply to address the relationships between gas concentrations and discharge, we believe that this approximation will still yield valuable insight into subglacial CH<sub>4</sub> and CO<sub>2</sub> dynamics in this region.



**Figure R7. Watson River compared to Russell Glacier average daily discharge between June 20 and July 15, 2018 (cite source), during which interval both records are available. Watson River discharge information is from (van As et al., 2018) and Russell Glacier discharge is unpublished (personal communication).**

The low number of samples and the fact they come from two different years, 2017 and 2018, is another weakness of the study. Figures 3, 4, 6, 7, 9 appear as though they show time series (suggested by the lines connecting the dots), which is not the case. I suggest the authors redraw the figure so it's clear the data come from two independent seasons. This should also be acknowledged in the text, and the possible differences in hydrology and the potential impact on the export of gases discussed more in detail. For example, Hawkings et al (2015) showed large interannual variations in total discharge and solute and particulate fluxes from the Leverett catchment between 2009-2012. Also, no statistical analysis of the results was done and it's unclear whether the differences in water chemistry were significant between the streams – was this due to the low number of samples?

Because our intention was to capture changes in water chemistry between disparate locations and throughout the melt season, with a primary goal of identifying differences between glacial discharge sites, logistical constraints did not allow us to sample any individual subglacial discharge site throughout the melt season. While not included in our original manuscript, we conducted statistical analysis through one-way ANOVA, which indicated that both CH<sub>4</sub> and CO<sub>2</sub> concentrations differ between sites (p<0.0001), supporting the principal findings stated in this manuscript. Dissolved gas isotopic compositions, however, do not significantly differ. We will include these statistical tests in the results and discussion of a revised manuscript. We will additionally redraw the figures to clarify that samples were collected over multiple years, which also addresses a comment by Reviewer 1.

This should also be acknowledged and/or explained. Analysis of stable isotopic signatures of C in CH<sub>4</sub> and CO<sub>2</sub> is a powerful tool for determining the origin of the respective gases; however, caution must be exercised when interpreting the results for glacial meltwater samples. Glacial



meltwater is a very dynamic mixture consisting of several components with different origins: the water mostly comes from the surface and so has been in direct contact with the atmosphere (and its CO<sub>2</sub>); the sediment is predominantly of subglacial origin. Dissolved CH<sub>4</sub> also likely originates from the subglacial environment, while CO<sub>2</sub> has multiple sources -- as the authors show and discuss throughout the ms. The isotopic separation factor  $\epsilon_C$  (ie  $\delta^{13}C_{CO_2} - \delta^{13}C_{CH_4}$ ), used in this study to assess the sources and sinks of CH<sub>4</sub>, is suitable for closed systems (as defined in Whiticar 1999), but caution must be exercised when using it for glacial meltwater and the limitations should be acknowledged and discussed in the text. The authors also calculate the fraction of oxidised methane ( $f_{ox}$ ) using a number of assumptions, some of which might not be substantiated. For example, Michaud et al (2017) modelled the kinetic isotopic fractionation factor  $\alpha_{ox}$  beneath the West Antarctic Ice Sheet, an environment likely to be more representative of the bed of the GrIS, at 1.004. The authors use a value of 1.049, which may lead to an underestimation of microbial oxidation of CH<sub>4</sub> in the GrIS subglacial system. More importantly, outgassing, as a major process affecting meltwater gas concentrations, should not be ignored. In the turbulent flow of glacial rivers, most CH<sub>4</sub> will likely outgas very quickly: for example, in the Leverett Glacier river, virtually all CH<sub>4</sub> is gone after ca 2 km (Lamarche-Gagnon et al 2019). Moreover, outgassing affects not only the concentrations, but also the isotopic composition of gases due to fractionation (see eg Banks et al 2017), driving the remaining dissolved gas to more positive (heavier) values. This may result in an overestimation of CH<sub>4</sub> oxidation. While outgassing was probably less significant at IS (as the authors sampled only 10 m from the subglacial outlet), it may have affected gas concentrations at RU (100 m) and definitely would have at KS (>1 km and a proglacial lake; see below). Much more attention should be paid to the possible effects of this process in the discussion. Moreover, it should be pointed out that outgassing is likely enhanced in glacial systems by considering the large pressure differentials between the subglacial environment where the CH<sub>4</sub> is produced and the atmosphere, and the rapid depressurisation that results from pressurised subglacial waters exiting the ice. Such depressurisation effect is likely to also influence the isotopic signature of the sampled gases in runoff (Banks et al 2017). While accounting for outgassing/depressurisation and their effects on isotopic fractionation might be difficult, if not impossible, the authors should at least discuss the limitations and biases of not doing so, and whether or not the assumptions from their oxidation model would still hold true.

We agree that outgassing could occur between the subglacial outlet portals and our sampling locations. However, our discussion of subglacial methanogenesis sites focuses on the Isunnguata and Russell sites, where CH<sub>4</sub> supersaturation was observed, and from which samples were collected in close proximity to the glacial discharge site. While some outgassing certainly occurs during water transit from the subglacial environment to the sampling location, the extent of outgassing should be relatively constant over the melt season, and thus would not explain temporal trends in  $f_{ox}$  or  $\epsilon_c$ . Additionally, if  $f_{ox}$  and  $\epsilon_c$  were predominantly reflective of outgassing processes, the isotopic compositions of CH<sub>4</sub> from the Russell would lead to consistently higher estimates and  $f_{ox}$  and lower  $\epsilon_c$ , however both variables are nearly identical between Russell and Isunnguata sites (manuscript Fig. 4), suggesting that isotopic compositions reflect similar subglacial processes between the two proximal sites. Nevertheless, the reviewer makes an important point here, which we agree with, and we will include additional discussion of how

possible outgassing may affect our interpretations and highlight the expected impacts on isotopic compositions.

Our calculations of CH<sub>4</sub> oxidation were intended to provide a minimum estimate of the amount of subglacial CH<sub>4</sub> oxidation that would be implicated by the observed CH<sub>4</sub> isotopic compositions. To do this, we used the upper limit of CH<sub>4</sub> oxidation fractionation factors in the literature. Since we do not have enough information to constrain the actual fractionation factor, we believe that presenting a minimum value, even if it is likely to be an underestimate, provides a valuable constraint on the inferred role of subglacial CH<sub>4</sub> oxidation. While other fractionation factors, such as that presented in the cited Antarctic study, may be closer to the value of the true fractionation factor under the Greenland Ice Sheet, using this value introduces more assumptions than our current approach.

Assuming similar subglacial environments between Antarctica and Greenland may additionally not be appropriate because subglacial conditions differ between these settings. For instance, while supraglacial meltwater flows from the surface to the base of the Greenland ice sheet and provides biologically relevant material such as labile organic matter, oxygen, and nutrients in dust debris or imparted through atmospheric deposition, no such supraglacial meltwater transfer has been observed in Antarctic settings and surface water production in Greenland is currently much higher than in Antarctica (Bell et al., 2018). While subglacial liquid water may be present, in Antarctica, the glacial history is significantly different from Greenland and the time since atmospheric contact in Antarctic subglacial lakes can be thousands to millions of years.

Despite the uncertainty in CH<sub>4</sub> oxidation fractionation factors, revisions could include a range of CH<sub>4</sub> oxidation estimates using the range spanned in the literature, if appropriate.

The KS site is additionally problematic as there is a large proglacial lake right by the portal, with an estimated water residence time in the order of 24 hours at peak discharge (Hatton et al 2019). This may significantly change the concentrations and isotopic signatures of the dissolved gases exported further downstream via outgassing (and possibly also microbial processes in the lake bottom sediment), and may be the reason why the CH<sub>4</sub> concentrations at KS are near atmospheric equilibrium. This should also be mentioned when discussing the results from KS.

We agree with this assessment. However, flow at KS through the glacial lake is slow with very little turbulence, which should minimize the extent of outgassing. Additionally, since CO<sub>2</sub> is out of equilibrium with respect to atmospheric concentrations, we believe that the lack of CH<sub>4</sub> supersaturation is not due to outgassing. However, because outgassing is a possible contributing factor, we will discuss this in the text more clearly.

In the discussion, the authors interpret the observed orders of magnitude variability in dissolved gas concentrations in the meltwater samples as differences in the sources and sinks of the gases (lines 290-291). However, some of the explanations of CH<sub>4</sub> variability are unnecessarily speculative and unsupported by data, and some rest on incorrect assumptions. First, the variability in subglacial OM substrates is invoked (312). This is certainly a factor affecting subglacial C cycling and export rates, but no supporting OC data are presented. Permafrost reservoirs, suggested based on the study by Ruskeeniemi et al (2018), are unlikely to be of

importance (and were not alluded to in Lamarche-Gagnon et al 2019, as suggested at line 314), as they extend only a few km into the ice sheet bed. Moreover, Ruskeeniemi et al (2018) only focused on the thermal state of the sediments/soils, rather than the nature of OC. I agree the Holocene ice margin fluctuations were probably important in providing fresh OC substrate that could have been metabolised into CH<sub>4</sub> that is currently being exported. Older (Eemian) OC sources are however also present and exported in the meltwater (Kohler et al 2017) and may have been used as methanogenesis substrates.

We do not include DOC or POC data in this manuscript because delineating OC sources (as subglacial versus supraglacial, for instance), which may have very different concentrations and reactivities between sites and over time, is outside the scope of this manuscript and data set. We do not believe that DOC data would substantially impact the results presented in this manuscript, as many other indicators of OC remineralization (CO<sub>2</sub>, CH<sub>4</sub>, δ<sup>13</sup>C-CO<sub>2</sub>, and NH<sub>4</sub>) all indicate that OC remineralization occurs under the ice sheet but to varying extents, and DOC concentration information is not needed to make this inference.

We will modify the text regarding the formation and nature of the subglacial OC deposits that may be implicated by our results. We acknowledge that we have no data regarding these deposits, which are as of now poorly constrained in both their nature and distribution. However, the lack of information regarding subglacial OC deposits does not impact the interpretation of our results, which all indicate that varying OC remineralization (and mineral weathering) occurs and contributes to heterogeneous greenhouse gas content of subglacial discharge.

Reservoirs of old CH<sub>4</sub> are not considered in the study. Second, a direct effect of oxygen supply to the ice sheet bed by surface meltwater on methane production/oxidation is proposed, based on the negative correlation of CH<sub>4</sub> concentrations at RU and Watson River discharge (319-327). As explained above, linking gas concentrations and isotopic signatures at IS and RU to discharge data from the Watson River is misleading. In addition, the authors only consider live methanogenesis and ignore potential old CH<sub>4</sub> storage/leakage (see above). Dilution by meltwater is only acknowledged at lines 328-329 as an alternative explanation, although it plays a significant role. The local subglacial sources of CH<sub>4</sub> are probably limited to microbial activity (Lamarche-Gagnon et al 2019), which takes place in anoxic sediments buried under the ice. Whether it's recent activity or reservoirs of ancient CH<sub>4</sub>, its export is dependent on meltwater tapping and flushing pockets of produced gas. As a result, CH<sub>4</sub> concentrations in the meltwater are necessarily discharge-dependent. This is indeed complicated by outburst events; however, these are limited to large outlets (lakes form at much higher altitudes further into the ice sheet than those to which this subcatchment extends), and explaining the lack of discharge-CH<sub>4</sub> concentration relationship at IS by outbursts (330-332) is therefore not justified. Last, CH<sub>4</sub> oxidation, discussed at lines 350-361, is certainly an important process controlling the amount of CH<sub>4</sub> that will be exported from under the ice to the atmosphere. However, in addition to the uncertainty in determining the degree of CH<sub>4</sub> oxidation, the authors' interpretation of the data again relies on correlating the CH<sub>4</sub> concentrations at IS and RU with Watson River discharge and on treating the 2017 and 2018 data as a time series, both of which are flawed (see above).

We acknowledge that our interpretations assume that most CH<sub>4</sub> is produced actively, though leakage of “old” CH<sub>4</sub> may occur, and will therefore include this as a possibility. However, particularly in the peak melt season when subglacial residence time should be shortest, our measured  $\delta^{13}\text{C}\text{-CH}_4$  values closely match those measured by live methanogenic communities by Dieser et al. (2014), supporting (but not confirming) active methanogenesis under these portions of the Greenland Ice Sheet.

We disagree that dilution by meltwater is not presented as a key mechanism explaining the discharge-concentration relationship of CH<sub>4</sub> as it is one of only two mechanisms presented for the Russell Glacier that are given approximately equivalent amounts of discussion (Lines 325-327: higher methanogenesis during low flow resulting from greater subglacial residence time. Lines 327-328: dilution of methanogenic subglacial drainage by supraglacial meltwater). These mechanisms are additionally not mutually exclusive. Our modified manuscript using discharge data from the Isunnguata sub-catchment (called Isunnguata in this study) will likely somewhat change the discharge-concentration relationship at the Isunnguata. In our revised manuscript, we will more clearly describe the lack of relationships between discharge and concentration that invoke heterogeneous CH<sub>4</sub> distributions in distributed portions of the subglacial drainage network, which we will distinguish more clearly from potential impacts from outburst events.

In summary, I recommend the authors revisit their local hydrology description and interpretation, rename their sampling sites accordingly, avoid correlating their small stream data with the Watson River discharge record, and properly acknowledge the limitations and uncertainties of the used geochemical calculations for interpretation of the subglacial gas sinks and sources, especially for CH<sub>4</sub>

This review has provided valuable feedback regarding information needed for better clarity of our sampling sites and their hydrological settings, which we will heavily incorporate in manuscript revisions. Many of the criticisms regarding hydrology and use of Watson River discharge to assess concentration-discharge relationships will be addressed by new discharge information for our study site (Isunnguata sub-catchment, referred to as Isunnguata in this study) provided by Asa Rennermalm. Our interpretations may shift somewhat following these modifications although we think based on the small watershed correlation with Watson River discharge, our primary findings will be robust. Specifically, CO<sub>2</sub> and CH<sub>4</sub> concentrations exhibit a high degree of heterogeneity between glacial discharge sites of the Greenland Ice Sheet, and that variations likely occur due to disparate levels of subglacial organic carbon remineralization. We think it is important to demonstrate not only that this heterogeneity exists, but also that it represents a large range of greenhouse gas fluxes from subglacial systems that are controlled by various processes, including weathering reactions and hydrologic and microbial processes. The significance of this finding is to point out the potential range of greenhouse gas fluxes in a warming world with retreating ice sheets, such is occurring now, as well as occurred since the Last Glacial Maximum. These results could significantly impact upscaling efforts of greenhouse gas fluxes from GrIS melt, which will be an increasingly important carbon flux in the coming decades.

## Specific comments

53: please specify if Graly et al 2017a or b

This refers to Graly 2017b and will be modified in the revised manuscript.

58-60: relevant work should be cited here, eg the recent review by Wadham et al (2019)

This citation will be added.

66: Musilova et al (2017) did not study subglacial microbial activity; this reference is irrelevant here

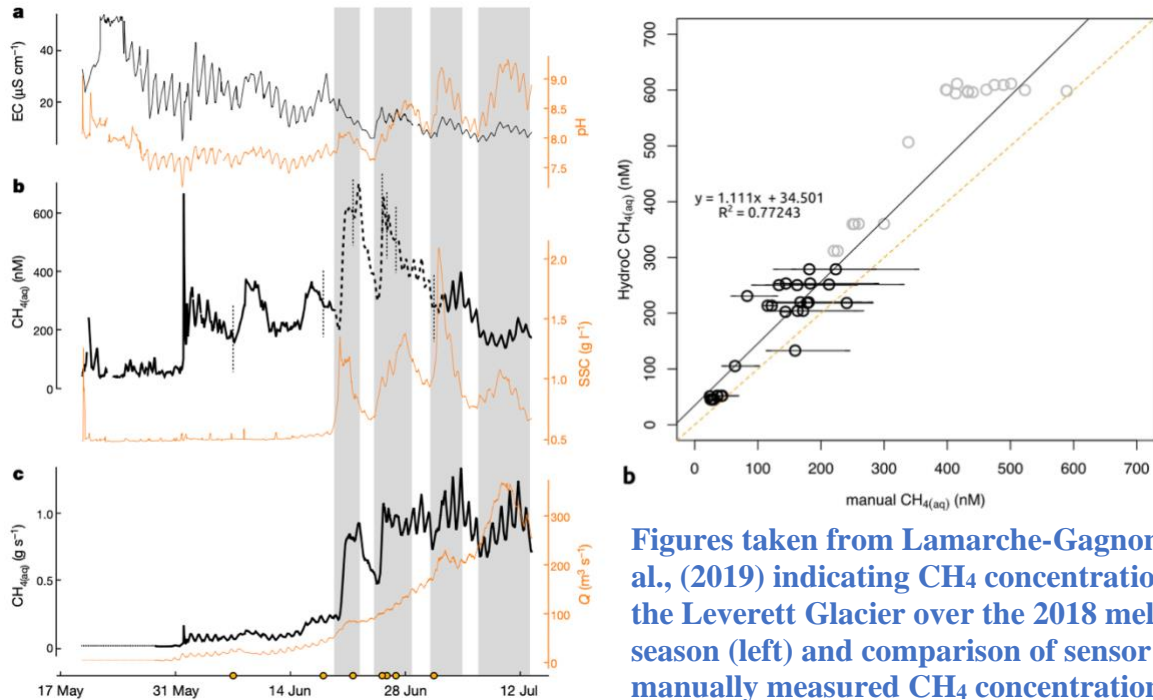
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107-110: methanogens have also been identified in Russell Glacier basal ice (Stibal et al 2012) and Leverett Glacier river suspended sediment (Lamarche-Gagnon 2019); CH<sub>4</sub> supersaturation in meltwater was also measured by Dieser et al (2014) but not by Christiansen & Jørgensen (2019)

Thank you for these corrections, we will revise our statements to reflect this.

306: Lamarche-Gagnon et al (2019) measured higher CH<sub>4</sub> concentrations than 600 nM (up to 4000 nM during early season)

We used the maximum value as indicated in the continuous monitoring record and supplemental information (below). If a maximum of 4000 nM has been published elsewhere, we will modify our statement.



Figures taken from Lamarche-Gagnon et al., (2019) indicating CH<sub>4</sub> concentrations at the Leverett Glacier over the 2018 melt season (left) and comparison of sensor and manually measured CH<sub>4</sub> concentrations (above).

405: how do the results compare to the recent paper by Andrews et al (2018) focused on dissolved C dynamics in Russell Glacier meltwater, including the sources of subglacial CO<sub>2</sub>?

Andrews et al. (2018) estimated up to 30% subglacial DIC production by microbial activity in discharge of the Russell glacier, consistent with our study indicating significant contributions of subglacial organic matter remineralization. We will include discussion of these similarities in our revised manuscript.

446: please explain ‘chemostatic behavior’

Chemostatic behavior indicates that concentration is constant over a range of discharge and indicates that concentrations are not controlled by dilution and are not transport-limited.

695: Figure 1 is a weird combination of 2D and 3D which makes it difficult to interpret. Also, could the authors provide references for CO<sub>2</sub> and CH<sub>4</sub> evasion through crevasses?

The exchange through crevasses is drawn to indicate the semi-closed nature of the subglacial system, whereby gas exchange may occur. While no studies have directly measured this exchange to our knowledge, atmospheric gas exchange in the subglacial environment has been implicated (Graly et al., 2017) and contact between the atmosphere and the subglacial environment invokes exchange in moulins or fractures that allow exchange of atmospheric gases to depth.

700: Figure 2 needs redrawing to correct the river network names and to better indicate the sampling sites; please also use the newer transcription ‘Kiattut’, to be consistent with the text.

This correction will be made.

740: the regression line in Figure 8b doesn’t look right – were some points omitted?

No points were omitted, however there is one point in the CO<sub>2-atm</sub> series that falls below the other points but is partially obscured by a point in CO<sub>2-OM</sub>, which may have resulted in confusion. The regression includes all points in the data series.

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