Interactive comment on “Heterogenous CO2 and CH4 content of glacial meltwater of the Greenland Ice Sheet and implications for subglacial carbon processes” by Andrea J. Pain et al.

Anonymous Referee #1 Received and published: 29 July 2020

GENERAL COMMENTS:

Pain et al. report carbon dioxide (CO2) and methane (CH4) concentrations, 13C stable isotopes and water chemistry from subglacial meltwaters of three glaciers located in southern regions of Greenland. The authors do a good job of describing these systems and discussing controls on both CO2 and CH4 dynamics in glacial systems. The manuscript is generally well written and easy to follow. In my opinion, this is a worthwhile contribution to the literature on the biogeochemistry of glacial systems. My suggested changes are mostly technical, but I distinguish below between “Specific Comments”, referring to systematic changes in multiple sections of the manuscript or more substantive changes, from “Technical Comments”, which are editorial.

> Thank you for your positive feedback regarding this study and its contribution to the understanding of subglacial biogeochemistry and carbon dynamics. We believe the suggestions provided by this review, namely to indicate with greater clarity the novel aspects of this study, the statistical tests conducted, as well as expand our discussion to include other possibilities besides a two-end member mixing model for CO_2 concentrations at our Isunnguata sampling site, will substantially improve the manuscript. We address specific and technical comments in detail below.

SPECIFIC COMMENTS: Could the novelty of the study be better highlighted? A significant body of work exists on the southwestern glaciers in particular (Innunguata, Russell), as described on lines 103-113, so it’s not entirely clear to the reader what the novel contribution of this study is from the text at the outset.

> Yes, we will make efforts to highlight the novelty of this study, which is to assess the heterogeneity in greenhouse gas (CO_2 and CH_4) compositions of subglacial discharge of the Greenland Ice Sheet. Previous studies have evaluated CO_2 (Ryu and Jacobson, 2012) from the Isunnguata sub-catchment in this study, CH_4 concentrations in atmosphere near the Isunnguata sub-catchment (Christiansen and Jørgensen, 2018), CH_4 microbial cycling at the Russell (Dieser et al., 2014), and CH_4 concentrations at the Leverett Glacier (Lamarche-Gagnon et al., 2019). This is the first study to compare both CO_2 and CH_4 concentrations during the same time periods of Isunnguata and Russell glacier discharge, which demonstrates the regional heterogeneity in subglacial carbon dynamics in glaciers discharging into the Watson River, as well as heterogeneity between these southwest locations and southern Kiattut Sermiat site. 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 hydrologic and microbial processes as well as mineral weathering reactions. The significance of this finding is to point out the potential range of greenhouse gas fluxes in a warming world with retreating ice sheets, as is occurring today, as well as following the Last Glacial Maximum. These results have two important implications: they first provide

a range of potential impacts on atmospheric greenhouse gas compositions during ice sheet collapse after the Last Glacial Maximum. They additionally emphasize the need for caution in upscaling efforts of greenhouse gas fluxes from GrIS melt as polar amplification of global warming increases current rapid melting of the Greenland Ice Sheet.

Are errors throughout reported as standard deviations or standard errors (e.g., see section 3.1)?

Errors are all reported as standard deviations. This will be added to text in a revised version.

The samples were collected over a couple of years, which is perfectly reasonable for Arctic sites given the finances and logistics of working in the region. However, since the samples collected in 2018 were from the summer, and the 2017 samples were from the spring and fall, it would seem inappropriate to display the points with adjoining lines as a time series (i.e., Figures 3,4, 6, 7), because there can be large interannual differences in meltwater dynamics. There is no perfect solution to this, except to remove the lines adjoining the 2017 and 2018 samples, and perhaps discuss differences between the two years given the DMI climate data for both regions and/or the PROMICE discharges for the two southwest glaciers.

We agree that connecting the 2017 and 2018 data points may misrepresent our data set and will therefore redraw the figures to more clearly distinguish different sampling years. The suggestion of describing melting dynamics between 2017 and 2018 data using DMI and PROMICE discharge is excellent and will be very helpful in our discussion of temporal variations in gas concentrations throughout the melt season.

The justification for the measurement and presentation of the NH4+ data aren't obvious (only stated on L430). This should be explicitly indicated in the methods, but the data are not particularly informative and could be excluded (though this is entirely up to the authors).

We will provide justification for the inclusion of this parameter in the methods section. While glacial N cycling is complex (Wadham et al., 2016), NH_4 is produced during organic matter remineralization and may be used as a tracer for heterotrophic metabolism as there are a limited number of abiotic NH_4 sources. We use associations between $CO_{2\text{-total}}$ and NH_4 in Figure 8b as supporting evidence suggesting that organic matter remineralization is an important subglacial CO_2 source.

L271-275, Fig. 8: The statistics presented in the text vs. Fig. 8 are a bit confusing to follow. The text states that it's a correlation (i.e., independence of x and y), but a linear regression (i.e. dependence between x and y) is shown. Correlation statistics (i.e., r instead of r2) should be shown, or the text should be changed to reflect a linear regression (which is otherwise used throughout the text). Further, from Fig. 8a, the statistics appear to apply to the entire dataset as there is no indication from the caption or figure otherwise; however, the text states that the relationship was only observed for the Isunnguata samples, suggesting that the statisC2 tics presented only refer a subset of the data presented. I also wonder about the validity of removing the outlier... It's possible that this relationship is not linear, but rather parabolic, with lower 13C values at lower CO2 concentrations indicative of rapid weathering, which can mimic the 13C-DIC signature associated with OM remineralization. There are not enough data to test this, but it would be something to keep in mind as the deviation of the “outlier” is not large enough to be indicative of analytical issues, but perhaps a true pattern. For this reason, I’m a little hesitant about the inference of a simple two end-member mixing model, especially since it does not seem to hold for the other sites.

> We will clarify the text regarding the statistics used. All statistics presented are linear regressions rather than correlations, and in all cases the regressions are site-specific and data from multiple sites are not combined.
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> The possibility of a parabolic relationship between $\delta^{13}C$-CO_2 and $CO_{2\text{-total}}$ could explain our data and would imply that no data points are outliers. We will include discussion on other possibilities besides a 2-end member mixing model for Isunnguata and our other sampling locations. However, the otherwise strong relationship between $\delta^{13}C$-CO_2 and $CO_{2\text{-total}}$, as well as significant relationship between $CO_{2\text{-total}}$ and NH_4 for Isunnguata samples suggest that organic matter remineralization is the principal source of CO_2 in the subglacial environment. We acknowledge that the low CO_2 end member (or end members) in our mixing model is poorly constrained, and more data may be necessary to determine whether a 2-end member mixing model is valid.

TECHNICAL COMMENTS:

L26-27: Add reference to sentence starting with “Variations. . .” But also see Tranter et al. 2002, that has discounted a substantial role for glacial weathering on atmospheric CO2 concentrations over geological time scales (https://doi.org/10.1016/S0009- 2541(02)00109-2).

> Thank you for this suggestion, we will include this reference in a revised version.

L32-35: It might be useful to explain here the possible sources of CO2. As is, it seems somewhat disjointed from the preceding sentence, which describes CO2 budget. Both CO2 and CH4 will contribute to the carbon budgets of the system. The sources of CO2 are discussed in the following paragraph, but perhaps just a rejigging of this text would read more fluidly. One option would be to move L32-39 after the following paragraph and slightly expand upon the CH4 introduction before introducing the purpose of the paper. For example, of additional relevance to CH4 in subglacial environments is the formation of the necessary precursor H2 by rock comminution in Telling et al. 2015 (https://doi.org /10.1038/ngeo2533).

> Thank you for this helpful feedback, we will rework this section to improve the clarity of presentation.

Section 2.1: What is the seasonality of these systems? How much of the annual discharge occurs during the period where these were sampled? Is there winter flow? Glacial outburst floods?

> We will address seasonality of discharge in the revised manuscript as suggested in this review, which will provide information about discharge in 2017 and 2018 as well as

comparatively been these two melt years. Winter flow at these sites is low, with minor flow at the main outlet of the Isunngua River (Isortoq River), and no flow in the Watson River (Pitcher et al., 2020).

L103-113: See also Dubnick et al. (2017; https://doi.org/10.1002/2016JG003685), which includes calculated CO2 undersaturation at Kiattut Sermiat.

Thank you, we will include this reference for comparative purposes

L115: What were the specific sampling dates? If this is too much detail to have as text, then at least the number of sampling campaigns at each site each year would be useful information here.

We will include in a revised manuscript (or supplementary material) the number of samples per site per campaign and the date of each sample collection.

L240: What was the 13C-CO2 value for summer across all sites?

The $\delta^{13}C$-CO_2 values we collected at peak melt season (considered here to be during the month of July) were -14.1‰ and -12.1‰ for the Isunnguata, -12.4‰ for the Russell site, and -14.7‰ for the Kiattut Sermiat site.

Figure 1: Subscripts indicating carbonic or sulphuric acid (CA/SA) should be defined in the caption or on the figure.

This correction will be made.

Figure 2b: I'm wondering if there would be a way to trace the Watson River. It is difficult to see how the two study glaciers feed into the river.

We will provide an updated map figure in our revised version that will more clearly identify the sampling sites and names of tributaries and river segments that feed into the Watson River based on the feedback in this and another reviewer's comment.

Figure 2c: Kiattut Sermiat is spelled differently (Kiagtut) in panel c than elsewhere in the manuscript.

This correction will be made.

Figure 7: This caption should be more informative so that the figure can stand alone without the text. The y-axis is not intuitive without the definition for CO2total provided in the text (L266-267). As is, it's confusing because it looks like CO2 concentrations are negative, which in principle is impossible, though I understand what the figure shows.

Thank you for this feedback. This figure will be revised to more clearly indicate the meaning without relying on the caption text.

Figure 7: Could colour blocks instead of circle symbols be used for the legend? It's a small detail, but otherwise only technically refers to the Isunngata panel.

Yes, this revision will be made.

Figure 9: Symbology of the regression lines should be different from for the separation between years in which samples were collected.

We will change the format of the regression line to make this distinction.

References

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