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Interactive comment on "Heterogenous CO_2 and CH_4 content of glacial meltwater of the Greenland Ice Sheet and implications for subglacial carbon processes" *by* Andrea J. Pain et al.

Anonymous Referee #1

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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.

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Discussion paper

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.

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

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.

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).

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 statistics 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.

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).

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).

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?

L103-113: See also Dubnick et al. (2017; https://doi.org/10.1002/2016JG003685), which includes calculated CO_2 undersaturation at Kiattut Sermiat.

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.

L240: What was the 13C-CO_2 value for summer across all sites?

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

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.

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

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 CO_2total provided in the text (L266-267). As is, it's confusing because it looks like CO_2 concentrations are negative, which in principle is impossible, though I understand what the figure shows.

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.

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

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