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Discussions

Interactive comment on "Dissolved and particulate organic carbon in Icelandic proglacial streams" *by* Peter Chifflard et al.

Anonymous Referee #1

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General comments: This study measures the concentration and flux of dissolved and particulate organic carbon (OC) over a week long period from proglacial streams in Iceland. This study further uses spectroscopic characterization of DOM to assess how its composition varies across the study streams. The authors found that Icelandic glaciers are releasing large quantities of DOC and especially POC compared to previous global estimates of organic carbon release from glaciers. Overall, this is an interesting study that could potentially fill a key information gap in our understanding of how organic carbon release from glaciers varies across the globe. However, as written, there are far too many methodological uncertainties/limitations for consideration of publication. For instance, a great deal more information is needed for sample processing and analysis, OC flux estimates, and assessment of analytical error. It appears that one sample was

collected one time from each stream, with no assessment of analytical error. Moreover, my largest concern lies with the POC analyses. As the authors state in the conclusions (should be stated in the methods), POC concentrations are not measured directly but rather by loss on ignition. First off, this is not a very commonly used technique for POC analysis, especially in stream biogeochemistry. Second, the authors clearly acknowledge that hydrated clay minerals are likely contributing to the high POC concentrations. Third, it is clear that the POC concentrations are highly variable and greatly overestimated. How can concentrations increase from <1 to 56 mg/L over a distance of 3km in river length? Without any replicates or lab studies comparing direct POC vs. LOI analyses, I don't know how these data can be compared to other glacier OC studies with direct POC analysis.

What follows are more specific comments for the authors to consider to help improve clarity and interpretation of findings.

More specific comments: Page 2, lines 20-22: This is confusing. Icelandic glaciers are included in the global estimates of DOC and POC release from glaciers. However, as the authors point out, concentration estimates of OC in Icelandic glaciers as well as many other regions worldwide are not. Please rephrase to improve clarity.

Page 4, line 10: I think the authors have already made it clear that there are no other studies, to their knowledge, of OC release from Iceland glaciers.

Page 4, line 14: This is a very short sampling window. How long does the melt season last? When is the peak of the melt season? How many times was each stream sampled?

Page 4, lines 26-29: Given the extremely low DOC concentration reported here, more information is needed on sample handling and processing. For instance, were samples field filtered or acidified in the field? Were replicate samples collected? How long were sampled stored in the field before transport? How long until analysis for DOC occurred?

Page 5, lines 1-6: More information is needed about the DOC and POC analysis, such were replicated analyses performed? What is the lower detection for DOC? Are there any error estimates on the OC concentrations? DOC concentrations of 0.1 mg/L are quite low for detection on a Shimadzu TOC analyzer. Why were POC concentrations not measured directly rather than determined by loss on ignition? How large were PON concentrations? The filters were not acidified it appears so what about inorganic carbon?

Page 5, line 7: Where samples filtered through a smaller pore size filter than just a GF/F before optical analysis? In my experience, a 0.7 um filter does not remove enough of the background turbidity in low DOM, glacier water samples and therefore greatly interferes with the optical analysis. How many EEMs were included in the PARAFAC model? Given only 2 components were identified, I question the value of even including a PARAFAC model, especially given the uncertainties in sample processing and filtration.

Page 5, lines 21-22: Please provide some reporting/discussion of the physical data presented in Table 2 at some point?

Page 5, lines 24-29: If DOC concentrations in Iceland glaciers are comparable to other regions, do the authors have any idea of why POC concentrations are so different?

Page 5, lines 26-26: What were the % carbon concentrations on the filters and the TSS values? With such high POC concentrations, it would be helpful to see these data in a summary table or in Table 2.

Page 6, lines 11-12: It would be helpful to provide some more mechanistic information about how DOC and POC cycling is impacted by proglacial lakes. In other words, are biologic or physical processes impacting OC cycling?

Page 7, lines 1-2: If there is a strong anthropogenic influence at this site, not sure how much one can glean about OC dynamics and longitudinal changes in concentration

and speciation in proglacial streams? I think it is fine if the authors include this sample site in the results but remove this sample point when discussing longitudinal changes in proglacial streams.

Page 7, lines 3-5: I suggest the authors remove this sample point because of its saltwater influence. How can any conclusions be drawn about longitudinal changes in OC concentrations when a data point is influenced by saltwater rather than simply the fluvial network?

Page 7, line 23: Closer sampling points to what? The glacier terminus? Please clarify.

Page 7, lines 29-31: It is not clear how the authors make the link between fluorescence characteristics and ancient OC?

Page 8, line 5: This could be generally true but I am not convinced without some sort of regression or trendline with DOC/POC concentration versus distance from the glacier. There is a lot of longitudinal variability in OC concentration along the river. Moreover, some of these changes are likely driven by anthropogenic inputs and the influence of saltwater. So the trends (if any?) are not as simple and clear as stated here. I just plotted the DOC/POC data vs. distance from glacier terminus and found that DOC concentrations increased downstream. However, I found no trend what so ever for POC, especially once the last two data points (one with saltwater influence and the other with anthropogenic influence) were removed. The longitudinal approach needs to be revisited.

Page 8, lines 31-32: More detailed methods on the OC flux estimates are needed. What is the total runoff from the glaciers for the entire melt season? A mean runoff is not sufficient for estimating total annual OC fluxes from all of Iceland.

Page 9, lines 4-6: This is a very important methodological issue that should be addressed before this paper is considered for publication (see above). The POC concentrations presented here are not measured directly, highly variable and there are no

replicates (at least according the presented methods).

Page 9, lines 6-8: According to the Hood et al. (2015) paper, Icelandic glaciers are included in the estimates of global OC storage and release from glaciers and icefields?

Table 2: The pH and water temperature data do not seem realistic. I have never seen a pH value anywhere close to 13 in natural waters, even when water originates from limestone springs? Also, how can there be a stream temperature of 14C in a proglacial stream? A stream temperature of 5.6C 1km downstream from a glacier terminus? Are these sites receiving geothermal inputs of groundwater?

Figure 6: A regression plot with DOC/POC concentration vs. distance from glacier terminus would be more helpful. How were these "distance groupings" determined?