Biogeosciences Discuss., 10, C9384–C9388, 2014
www.biogeosciences-discuss.net/10/C9384/2014/

Biogeosciences Discussions
Open Access

Interactive comment on "Greenland Ice Sheet exports labile organic carbon to the Arctic oceans" *by* E. C. Lawson et al.

E. C. Lawson et al.

j.l.wadham@bris.ac.uk

Received and published: 31 March 2014

Reviewer #3

1. Overall, this paper presents some interesting results and makes a valuable contribution to our understanding of organic matter (OM) export from glaciers, specifically Leverett Glacier, an outlet glacier flowing from the Greenland Ice Sheet. Of particular value is the quantification of the particulate organic carbon (POC) flux and the identification of specific chemical compounds in the OM pool (free carbohydrate, etc. . .) as it relates to potential lability in downstream environments. This is novel data, not only in a geographical context but analytically as well, and represents a leap forward from studies that have used other techniques to more broadly characterize the dissolved organic carbon (DOC) pool. Also interesting is the finding that the OM flux was decoupled from

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

the meltwater flux. The microbial incubation results are interesting but may be better suited to a separate publication (below). These findings are important and advance our understating of glacier biogeochemistry and OM export from glacier systems.

I have several concerns that would preclude the paper from being published in its current form: 1) There's a heavy reliance on data presented in the "supplemental information". I can understand putting descriptions of analytical techniques as supplemental info, but there are numerous cases where results are presented and subsequently discussed, but appear as supplementary information rather than being in the main body of the paper. Not only is it frustrating for the reader to have to search for the figures and tables that are not in the main body, but if the data's not important enough to be included in the main body of the manuscript, why refer to it as often as you do? I'd recommend either narrowing the focus of the paper or moving some of the figures that you refer to from the supplementary material section into the body of the manuscript (e.g. dissolved analyte vs Q; Ca+Mg:Na+K; SS POC, etc. . .)

We thank the Reviewer for their advice on restructuring the methodology section in the manuscript and Supplementary Information; this was a common comment from all Reviewers. We have revised both methods sections and have included all essential information that should be in the manuscript in order for easy reading and continuity in the main methods section. We have limited the Supplementary methods to technical information and additional details.

2. Consider narrowing the focus of the manuscript. The way that I read this, there are 2 main stories: 1) OM flux, 2) what happens upon export (supports heterotrophy). The manuscript in its current form is mainly focused on the first while providing a relatively scant discussion of the second. I feel that the OM flux is complicated enough, and requires sufficient explanation that it stands alone as the topic of the paper. Along the line of narrowing the focus of the paper, there is information presented that doesn't really contribute to the overall discussion (e.g. cryoconites). While I can see how this info could be important, it's not discussed in any meaningful way within the context

BGD
10, C9384–C9388, 2014

of the OM flux, so why include it? For example, unless you're proposing cryoconite derived OM as a source for what you're observing in the subglacial outflow, why bother including it?

Rather than narrowing the focus of the manuscript, we have tried to better link the two concepts of export and bioavailability in the discussion, as the latter were commented upon favourably by the two other Reviewers.

The OM flux is the major focus of the manuscript and we intended to use the determinations of bioavailability and the increase in bacterial cell counts in the incubation experiments to stress the potential importance of glacial export to downstream ecosystems. It was beyond the scope of this manuscript to focus in detail on what happens after glacially exported OC reaches the ocean, only to show evidence that it could increase bacterial productivity as the export DOC is utilised. This is why discussion on the latter is limited.

We use the cryoconite hole data, together with data from the subglacial environment, to identify the sources for LMW DOC in bulk runoff. In this case, it is plausible that the cryoconite LMW-DOC is a major source of the LMW-DOC observed in runoff and may contribute to the latter being bioavailable to marine heterotrophs.

3. The fluorescence data needs to be carefully considered here. First of all, you need to include the offset between excitation and emission wavelengths on your graphs and Tables. You may mention it in the text, but it needs to be on the graphs too to avoid confusion. For example, Table 2 presents peak wavelengths for the various fluorescing moieties in the OM. So, for example, 336 nm, this is emission, so you excited at 318 (336 – 18 nm offset)? This needs to be explicitly stated if the reader is going to compare your results to those previously published.

The offset between excitation and emission wavelengths was also mentioned by Reviewer 1 and has been added to Figure 5 and Table 2. The peak wavelengths in Table 2 are excitation, not emission as the Reviewer suggests. We have added a further

BGD
10, C9384–C9388, 2014

column to Table 2 for the emission wavelengths that correspond to the dominant fluorophores.

4. Also, where's your synchronous scan relative to the Rayleigh scatter? The double peak in the snow samples in Fig. 5 could indicate either an algal exudase or an artefact due to detecting the shoulder of some Rayleigh scatter. You'll want to be sure of this as your interpretation hinges on if you're measuring OM or scatter. A simple way to do this would be to run an EEM on a snow sample to see where your synch scan plots relative to the scatter.

The HORIBA Jobin Yvon Fluorolog-3 enables both the correction of Rayleigh and Raman scattering which we did after each samples was analysed. To correct for Ramen scatter, we ran a deionized water sample at the start of each day and subtracted this from the glacial sample. To correct for Rayleigh scatter, we used the Fluorolog-3 software, which also accounted for inner-filter effects. Hence, we are confident that the fluorescence peaks represent DOM rather than some Rayleigh scatter. However, we used the software to do a generic correction for the Rayleigh scatter which may not have been appropriate for the different types of samples with differing DOM concentrations, e.g. cryoconite hole water and more dilute supraglacial meltwater. However, we followed the approach of previous work using spectrofluorescence to analyse DOC compounds in glacial samples, e.g. (Barker et al., 2006).

We did not state specifically in the methods how we corrected for Raman and Rayleigh scattering. This has been added to the edited version of the manuscript.

5. Further, your presentation of fluorescent organic moieties as a % of total fluorescence is misleading. In using the synchronous scan method, you're measuring fluorescence along a narrow band. You're not necessarily capturing all of the fluorescence peaks that are present in the sample, and of those that you are detecting, there's no reason to think that you're measuring the peak maximum rather than a peak shoulder. It's OK to say that cryoconite waters exhibit strong fluorescence in the protein-like

BGD
10, C9384–C9388, 2014

range, but to quantify it as a % of the total fluorescence is misleading because you're not scanning for the total fluorescence in the OM. You'd need to do a total fluorescence scan (EEM) to determine this.

We have now corrected the text accordingly and have removed the "Protein-like fluorescence (%)" column in Table 1 and removed mention of this data in the main manuscript and Supplementary Information.

6. You make a strong and effective case for the relationships between DOC and free carbohydrates using attribute agreement analysis. Can you use the same technique to look at DOC concentration ([DOC]) vs discharge (Q), or free carbohydrate vs. Q, of Q vs. Si, etc. . .? Would this help to resolve the mobilization of discrete subglacial OM pools, or reconcile source contribution to the net flux? As I wrote earlier, it's a very interesting paper and a great dataset, but the manuscript requires some significant work prior to publication.

During the preliminary data analysis we did look at using attribute agreement analysis to assess the degree of synchronicity between different geochemical and physical parameters as suggested by the Reviewer. However, it would be beneficial to return to this preliminary work and see if further statistics analysis would help resolve where different analytes originated and whether this could help resolve the mobilization of discrete subglacial OM pools.

Further attribute analysis has been undertaken during the revision stage of the manuscript and has shown that there is little synchronicity between different geochemical and physical parameters. This is reported in full in the revised manuscript.

Interactive comment on Biogeosciences Discuss., 10, 19311, 2013.