Biogeosciences Discuss., 10, C8974–C8976, 2014
www.biogeosciences-discuss.net/10/C8974/2014/

Biogeosciences Discussions
Open Access

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

Anonymous Referee #3

Received and published: 25 February 2014

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 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.

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

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. . .)

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 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?

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

BGD
10, C8974–C8976, 2014

compare your results to those previously published. 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 artifact 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. 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 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.

4) 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.

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