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Interactive comment on “Identification and analysis of low molecular weight dissolved organic carbon in subglacial basal ice ecosystems by ion chromatography” by E. C. Lawson et al.

Anonymous Referee #1

Received and published: 23 September 2015

This paper by Lawson and coworkers reports very interesting results with regard to low molecular weight DOC (LMW-DOC) speciation and abundance in basal ice from glaciers that have different organic and lithological substrates and thermal regimes. The case is made that these LMW-DOC compounds may support and/or be a product of biogeochemical activity beneath glaciers with consequence to regional aquatic ecology and global biogeochemical cycles.

This paper fulfills the basic criteria of Biogeosciences and its topic is suitable for publication in this journal. However, for me, there are several important obstacles that remain before this manuscript is ready for publication.

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One of the main strengths of this study is the use of ion chromatography for the quantification of an array of LMW-DOC compounds including free amino acids (FAA), extractable carbohydrates (FCHO), and carboxylic acids (FCA) at exceedingly low concentrations. However, given the novelty of this approach (in glaciology anyway) and the lability (volatility?) of many of these compounds, a more rigorous evaluation of the technique with respect to the basal ice samples is required. For example, did you explore if LMW-DOCs changed over the course of the analysis? You state that the FCA analysis of a single sample took 30 minutes (sec 3.3.3). I'm assuming that your samples were analyzed as a batch (not explained in the paper) and that you had 28 samples (Table 1)? Does this mean that the last sample to be analyzed sat in the instrument tray for 14 hours at room temperature? Might there have been any changes in LMW-DOC abundance or composition over the course of the batch analysis due to organic or inorganic processes? For example, is there a trend in acetate increase or decrease over this time in replicates? This would have a significant impact on your interpretation and warrants consideration and/or an explanation.

Another issue is that role of basal ice, and its constituent compounds, is confusing and potentially overstated. For example, one could argue that any DOC that is incorporated into basal ice is decoupled from the subglacial hydrologic system and does not get exported to proglacial aquatic ecosystems. Even when subglacial meltwater is exported (polythermal and warm-based glaciers), unless basal ice melt occurs across the entire bed and the subglacial drainage system drains meltwater from across the entire bed (which they don't), then the magnitude of basal ice contribution to the subglacial meltwater is unknown. It becomes negligible when you consider the subglacial routing of supraglacial meltwater during the melt system and the seasonal evolution of the subglacial drainage system from being distributed to being a more channelized "quick flow" system as the supraglacial meltwater flux increases. My understanding of basal ice formation (granted that the authors are by far more authoritative on this point than I am), is that its composition reflects subglacial conditions at the time that the material accreted onto the base of the glacier and subsequent biogeochemical modifications to

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12, C5679–C5682, 2015

[Interactive
Comment](#)

[Full Screen / Esc](#)

[Printer-friendly Version](#)

[Interactive Discussion](#)

[Discussion Paper](#)



it since accretion. In the case of the polythermal and warm-based glaciers (Russell, Finsterwalderbreen, Engabreen), subglacially routed supraglacial meltwater would be expected to contribute to the subglacial pool and glacially-overridden material may not be the only source of DOC, as this paper seems to assume.

Finally, I found that the relationship between microbial cell abundance and LMW-DOC and DOC was an interesting result, yet not adequately addressed. If these compounds are biogeochemically significant, either as a substrate or product of in situ activity, wouldn't you expect a correlation between microbial abundance and LMW-DOC concentration?

More specific suggested corrections are as follows:

Abstract (line 13): FAA is used but never defined

Abstract (line 25): Why “current” subglacial environments? Could you delete “current”?

I think that the term “allochthonous” is misused throughout. The overrun OC hasn't been derived from somewhere other than its present location, as the term suggests. Allochthonous has been used in studies to describe DOC brought in to a system, be it a river or ocean (etc. . . .) from somewhere else. This isn't the case here, and so a different term should be used, perhaps using “microbial” vs. “terrestrial” to make the distinction?

Section 3.1 (line 24): What does “BI” and “PR” mean?

In several locations (e.g. section 4.2, line 13; section 5.3, line 18) the observations that you make have been reported in the literature and you might consider citing them.

Section 4.2 (line 18): Are these emission or excitation wavelengths?

Section 4.3 (line 21-23): should it be $p < 0.05$ rather than $p = 0.05$?

Page 14157 (line 11) : “...sources have extensive contact...” This would be highly site specific, wouldn't it? If the water source is part of the well-developed quick-flow

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Discussion Paper



component of a channelized drainage system, subglacial contact would be minimal, wouldn't it? Particularly if it was confined to a scoured bedrock channel (N-channel)?

Section 5.3 (line 12): Here, and elsewhere, the assumption is made that the Joyce OM is “very labile”. While I agree that it probably is, you never test the source OM for lability, nor do you cite corroborating evidence to support that lacustrine OM is labile.

Conclusion (line 23): there's an extra “also” in the sentence.

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