Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2019-317-RC1, 2019

Biogeosciences
Discussions

Interactive comment on "Basal thermal regime affects the biogeochemistry of subglacial systems" *by* Ashley Dubnick et al.

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Received and published: 18 September 2019

The paper "Basal thermal regime affects the biogeochemistry of subglacial systems" by Ashley Dubnick and co-authors tests the hypothesis that glacier thermal regime controls subglacial biogeochemical processes through bed(rock) material mobilisation. The authors collected samples of basal and meteoric ice from one cold-based and three polythermal glaciers of the Devon Ice Cap in the Canadian Arctic and compared their solute and nutrient contents and microbial communities. They found that while basal ice was enriched in solutes, nutrients, and microbes compared to the respective overlying ice in polythermal glaciers, this was not the case in the cold-based glacier investigated. Moreover, location seemed to play a more important role for microbial community structure than ice type.

To my knowledge this is the first study explicitly addressing the role of thermal regime on subglacial biogeochemistry and as such it is a very valuable contribution to glacial ecosystem research. The sites were carefully selected and sampled, adequate methods were used to analyse their solute and nutrient contents and microbial community structure, and the conclusions are well-supported by the results. The ms is logically structured and well written.

Overall, this is an interesting study suitable for BG. I have a few comments/criticisms I would like the authors to address before the ms is published though.

First, the results of microbiological analysis should be showed and discussed in more detail (e.g. how many raw sequences were obtained and how did that change after rarefaction; how many OTUs were identified and were the dominant OTUs similar to those of other glacial environments; was microbial abundance in the samples quantified in any way?).

Second, it is a bit unfortunate that the only cold-based glacier sampled had a different bedrock type than the three polythermal glaciers, as it makes the differences between the sites more difficult to explain (bedrock vs. thermal regime effect). This should be acknowledged in the relevant sections of the discussion.

Last, there is a discrepancy between DOC (both warm and cold basal ice contained more DOC, including proteinaceous material, compared with meteoric ice) and microbial communities (warm basal ice vs. cold basal ice and meteoric ice). This is in my opinion not sufficiently explained in the ms. Is it because solutes are entrained even by cold-based glaciers but particulates are not? Or may it be an effect of bedrock (see above)?

Minor comments

260-264 As you didn't specifically look at any microbial functions, the discussion of potential N2 fixation feels a bit out of place here and could be deleted.

266-289 Emily O'Donnell (Lawson)'s 2016 in BG was the first detailed study on DOM in basal ice and showed e.g. the importance of bedrock and leaching of DOM in wet conditions at the glacier bed. I think it would be a useful reference for this section.

305 Here, the 2012 Global Change Biology paper would be a more appropriate reference, as the experimental data in Wadham et al. come from it (as the first author of this ms surely remembers. . .).

323 There already exist spatially explicit studies of microbial communities in glacial environments, mostly the surface – e.g. Cameron et al. 2016 FEMS, Darcy et al. 2017 FEMS. We also found spatial differences in Disko Island glacier stream assemblages (Zarsky et al. 2018 FEMS). These studies might be worth mentioning here.

Figure 3 seems to show data already shown in Table 1. If this is the case it may be redundant.

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