Biogeosciences Discuss.,
doi:10.5194/bg-2016-471-RC2, 2016

Biogeosciences Discussions

Interactive comment on "Quantification of basal ice microbial cell delivery to the glacier margin" *by* Mario Toubes-Rodrigo et al.

Anonymous Referee #2

Received and published: 28 November 2016

This paper presents total cell counts from six basal ice samples collected at the front of Svinafellsjökull, Iceland. The cell counts are then combined with previously published debris flux estimates to quantify the microbial cell flux from the basal ice layer. Compared to the supraglacial environment, there is still limited knowledge on subglacial microbial ecosystems and processes, and this paper adds new knowledge to this topic. However, in my opinion there are several issues that need to be addressed before the paper is ready for publication. I agree with Anonymous Referee #1 with regards to viable cell counts, so I will not use time to repeat this here.

General comments:

1. The first thing that attracts my attention is that the paper is unnecessarily short for a research paper in Biogeosciences. It gives the impression that the paper was intended

for another journal but somehow ended up as a submission to Biogeosciences. As the paper addresses a very specific topic within subglacial microbiology, namely total cell counts and cell flux in basal ice, I strongly recommend that the paper is expanded to provide the readers with an up-to-date overview of the current knowledge of cell abundance in basal ice and place the new cell counts and fluxes in this context. This will undoubtedly increase the impact of the paper.

2. The rationale for the study is that "basal ice melt-out could deliver viable microbiota to the ice margin that serve as inoculum, potentially accelerating pedogenesis as glaciers recede" (1,24-25). This may be true for some glaciers, but when I look at the location map (Figure 1a) it seems that the entire front of Svinafellsjökull is in contact with either a glacial river or ice-marginal lakes. Hence, the cells that melt out at the six sampling sites will most likely be washed into the glacial river and transported to a downstream sandur or into the sea. It is unlikely that they will accelerate pedogenesis as Svinafellsjökull recedes. As this is a case study, the scientific rationale should reflect what is relevant for the environment at Svinafellsjökull. I suggest that the authors put more emphasis into presenting the ice-marginal environment and a rationale that addresses the conditions at Svinafellsjökull. This will also make the paper more interesting for potential future studies on the microbial community structures in the supraglacial environment, the basal ice, the proglacial river/lakes, and the proglacial foreland at Svinafellsjökull.

3. The Introduction section is basically written as "there is a lot of knowledge about this, but little knowledge about that". This form is not very interesting to read and it seems a bit dubious at times. For instance, the authors write that "few studies have quantified sediment discharge from basal ice . . . (Wainwright et al., 2015)" (1,29-20), whereas Wainwright et al. (2015), in fact, write that "several studies [e.g., Hunter et al, 1996; Knight et al., 2002] have measured actual debris flux through the basal layer" (see page 1182 in Wainwright et al., 2015). I recommend that the authors change the form and include many more relevant references and use them in an active way (e.g.,

“Cook et al. (2010) found that . . .”). A full literature overview of cell counts conducted on different basal ice facies would be relevant either in the Introduction section or the Discussion section (maybe as a table).

4. It is stressed out throughout the paper that this is the ‘first’ quantification of cell flux from basal ice. If it is so important to provide the first quantification of cell flux from basal ice, the authors could just have combined the debris flux in basal ice provided by Knight et al. (2002) with the total cell count in basal ice provided by Yde et al. (2010) to produce an estimate for the basal ice delivery of cells at Russell Glacier in Greenland. This will have saved them all the fieldwork. Although it may be true that this first to make this estimation, I will suggest that the mentioning about being the ‘first’ paper to quantify basal ice cell flux is toned down and replaced by quantitative and qualitative comparisons between the results from Svinafellsjökull and estimates of debris fluxes and cell counts from other glaciers.

Minor comments:

1,9 and 1,10: These numbers of cell flux and cell abundance in the Abstract should be similar to numbers found in the Results section.

1,17; 1,19; 1,20; 1,30; 1,31-2,1 and other places: Include more references to support these statements.

1,20 and other places: Insert comma after “et al.”

1,20-21: “. . . there is a dearth of information on the delivery of organic material, including microbes, to the glacier margin”. I disagree in this statement, as there are several studies on DOC in glacial rivers.

1,25: A second paragraph should provide a literature overview on microbial abundance in glacier ice, including basal ice (see e.g. Irvine-Fynn and Edwards, 2014). There is no need to go into details about basal ice microbial diversity, expect for where the microbial diversity is relevant for culturing of cells. A third paragraph could be on debris

fluxes from basal ice.

2,1-3: “Our aims were to . . . and confirm that viable microbial inoculum are transferred between glaciers and proglacial ecosystems”. It is well known that viable cells are transported by subglacial river to the proglacial environment, so it must be specified that the aim of the paper is focusing on basal ice transport of microbes to a fluvial proglacial ecosystem.

2,7-12: The site description must provide more relevant information, especially regarding the proglacial river and ice-marginal lakes. What is the meteorological regime? What is the distance of glacial retreat since the Little Ice Age? What is the contemporary average frontal retreat rate per year? What is the river discharge and suspended sediment load? Is anything known about the supraglacial or proglacial microbial communities?

2,9: The period mark is red.

2,18 and 2,23: With regards to debris content, it is more correct to use “by mass” instead of “by volume” and to present supplementary information on grain size distributions. This will also be consisted with the use of mass in the calculations (3,28-29). If the stratified or dispersed facies contain gravel, stones or boulders, it should also be noted.

2,19: Insert “a” before “layer”.

2,25: How far up-glacier is the icefall?

2,26: I don’t understand the term “strain-related metamorphophism”. Isn’t all change of ice crystals in solid-state (i.e. metamorphosis) strain-related?

2,27-28: In my opinion, the most obvious sampling strategy would have been to select and survey one to three basal ice profiles perpendicular to the basal ice layering, and then collect samples for cell counts of various basal ice facies at regular intervals. Total cell counts are easy to do and cheap, so there are no obvious reasons to restrict the number of samples to just six samples. The reasoning behind the sampling strategy and selection of sampling sites needs to be explained, so that it is clear to the readers why the applied sampling strategy is better than sampling along profiles and why six samples are sufficient to estimate the abundance and variability of cells in basal ice.

2,29: Insert "assumed to be" before "similar".

3,2-3: Repetition. Delete this sentence.

3,15 and below: I think that it will be more logic to present the calculation method (3,27-4,2) before writing about the conditions at Svinafellsjökull. Therefore, I will suggest that the authors consider switching the two paragraphs in section 2.4.

3,16: Repetition. Delete this sentence.

3,16-25: This is very central for the calculations and the associated uncertainty estimates, but unfortunately the explanation presented in this paper is not very clear to me. I think that the calculations and assumptions by Cook et al. (2010, 2011b) should be presented in much more detail and with better descriptions of the estimates of each variable. It should not be necessary for readers to consult the two papers by Cook et al. to understand, for example, what is meant by stratified ice formed by glaciohydraulic supercooling. What is the length of the basal ice exposure around the glacier margin? How was the length measured? Was the length corrected because of glacier retreat between the study of Cook et al. (2010) and the present study? What is the ablation rate and ice velocity at the glacier front?

3,25-26: What is the spatial distribution (in %) between the different ice facies? How what it estimated and what are the uncertainties?

3,27: Delete "m-1".

4,5-6: It is difficult to assess these results without information of differences in grain size distribution and the content of content of gravel and larger particles.

4,7: Delete extra spacing.

4,21: Separate the Discussion section from the Conclusions section.

4,23-26: This comparison with other cell counts from basal ice facies from glaciers must be expanded and discussed in detail in relation to environmental differences and similarities (e.g., lithology, basal thermal regime, basal ice facies, debris concentration and grain size distribution).

4,28-29: This comparison with cell counts from supraglacial, glaciofluvial and terrestrial proglacial environments also needs to be expanded and discussed in context to deliver of cells from basal ice to adjacent environments.

5,5: Delete "bacterial".

5,7-10: Repetition. Delete these sentences.

5,12: What is meant by "who also found that cell counts increased with sediment content"? This relationship is not determined in the present paper.

5,13-14: "It is clear that different ice facies deliver different amounts of cells to the glacier margin". Why is this clear? I thought that the main conclusion from this study (based on six samples) was that stratified and dispersed ice facies contained similar amount of cells per gram of debris, making the distribution of different basal ice facies an insignificant variable. The main control on cell flux would then be the debris concentration in basal ice. Have I misunderstood something? If not, I think that this sentence should be rephrased to emphasize that debris concentration is the important parameter and that there is no need to consider various basal ice facies.

5,18-22: Again, this discussion of the role of different subglacial factors needs to be expanded and include a proper literature analysis of the microbiology in basal ice rather than being restrict to a single reference.

5,22-23: "Hence, we recommend that similar studies be performed at other sites with different glaciological characteristics to gain a better application of cell transfer to the margins of glacier". Where it is possible, cell delivery should be calculated from existing debris fluxes and cell counts from other glaciers, and the results should be compared with the results from Svinafellsjökull. Based on this comparison, the authors may recommend more studies on cell delivery from basal ice.

7,6: Is there a name missing here?

Figure 1a: Difficult to read the text at the bottom of Figure 1a. Is this text necessary?

Referred literature not mentioned in the paper:

Hunter et al.: Flux of debris transported by ice at three Alaskan tidewater glaciers, J. Glaciol., 42(140), 123-135, 1996.

Irvine-Fynn and Edwards: A frozen asset: The potential of flow cytometry in constraining the glacial biome, Cytometry A, 85(1), 3-7, 2014.

Knight et al.: Discharge of debris from ice at the margin of the Greenland ice sheet, J. Glaciol., 48(161), 192-198, 2002.

Yde et al.: Basal ice microbiology at the margin of the Greenland ice sheet, Ann. Glaciol., 51(56), 71-79, 2010.

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-471, 2016.