

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

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Reviewer 2 provided a helpful and critical review of our manuscript, for which we are grateful. Many of the comments appear to be aimed at encouraging us to expand our manuscript by adding details in a number of areas (e.g. about the site, sampling strategies, comparisons with other studies). We discuss below how Reviewer 2's comments have led to improvements in our revised manuscript. Our responses start with “»>”.

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

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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.

»>This is a fair point. We had deliberately tried to keep our manuscript short with the original intention that it would get our point across succinctly. When writing interdisciplinary manuscripts (microbiology, glaciology, sediment systems in our case), it can be challenging to present the work in a succinct manner whilst also maintaining transparency and comprehensibility. However, since both reviewers' comments have requested more detail, we are happy to expand the paper to enhance understanding and potential impact of the work. By addressing the comments of both reviewers, this manuscript will expand significantly. We hope, therefore, that we have addressed this particular comment from Reviewer 2.

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.

»>The reviewer is correct in asserting that there are, at present, a number of proglacial water bodies at Svínafellsjökull. However, this has not always been the case – for ex-

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ample, aerial photos from 1994 illustrate that most of the glacier was in contact with moraine, so the glacier foreland at that time may have received significant contributions of basal ice microbes. The contact zone between the glacier and proglacial area is dynamic. What we hope is that our paper begins the process of quantifying cell discharge from basal ice, and makes the point that this could be important for soil development. Hopefully, future studies will allow similar quantification from other sites with differing dynamic conditions. Basal ice melting out from the current glacier margin could contribute microbial material to moraines and ice-marginal sediment accumulations where it could accelerate pedogenesis, but we agree that in many areas microbes will be transported away by rivers or stored in lakes. Again, the implications of microbial loss/storage and contributions to pedogenesis have barely been studied, hence the novelty of our study. We hope that much more will be done in the future. We will add extra information to the manuscript about the dynamics of the glacier forefront. In addition, we will include a new figure of the sampling sites.

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).

»>We argue that introduction sections often comprise elements of literature review – what is the state of the science? What do we know? What don't we know? What will the present study do to address what we don't know? That is a common format that

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we are content with.

With respect, we disagree with the implication that several studies of basal ice sediment discharge/flux have been undertaken. The use of the word “several” by Wainwright et al. (2015) is, in our view, unfortunate. Take the most recent of the 2 example references used in that quote, i.e. the reference to Knight et al. (2002) – in that paper it is stated that:

“relatively little work has examined how the flux of debris through the basal ice layer contributes to glacial sediment budgets”.

And,

“...their review revealed...the limited information about debris flux through the basal ice layer”

Indeed, it is fair to say that the dearth of information on sediment fluxes from basal ice was the very rationale for the paper by Knight et al. (2002).

A few other papers have been published since 2002 on sediment fluxes through basal ice – those of Cook et al. (2010, 2011a) and Larson et al. (2006). Indeed, one of the current authors (Cook) is writing a book and a separate manuscript on this very subject highlighting how few studies have been undertaken. All of this does rather depend on one’s definition of the word “several”, as used in Wainwright et al.’s quote above. In our view, this should say “few”. Compare the number of studies of sediment flux through basal ice to the number of studies on sediment flux through rivers, or glacial rivers even, and one will find that such studies are extremely rare, unfortunately. This is one of the reasons why Svínafellsjökull is an ideal study site – Cook et al (2010, 2011a) quantified sediment discharge here. So we contest the reviewer’s point that our Introduction section is “a bit dubious at times”.

We do, however, concede that more references could have been used. As we explain above, we had tried deliberately to keep this manuscript short, but on the recommen-

dation of the reviewer (both reviewers, in fact), we will several references to the introduction. Also a new table will be added in which cell counts from different studies have been included.

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.

»> The proposal to combine data from Knight et al. (2002) and Yde et al. (2010) does not seem very feasible. Knight et al. (2002) quantify sediment discharge across a stretch of the ice margin, which comprises both dispersed and stratified facies ice – the stratified ice is further divided into 3 sub-facies (solid, discontinuous, suspended). The study by Yde et al. (2010) describes a range of ice types, but the debris-bearing ice types are banded facies and solid facies (importantly, dispersed facies was not observed or sampled despite it accounting for around 1/3 of the sediment discharge reported in Knight et al., 2002). Cell counts are reported only for the solid facies by Yde et al. (2010), and not the other 2 sub-facies of the stratified facies. Given that Knight et al. (2002) found that solid facies accounts for only ~56% of the sediment discharge from the basal ice layer, it would not be possible to present a full story of the microbial cell discharge from the basal ice at that site by combining the datasets from these studies.

Most studies try to outline what it is about the work that constitutes originality. One of the original elements of our work is that, unlike the problems outlined above, we can for

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the first time present an integrated glaciological and microbiological dataset that allows us to estimate cell discharge. This is the first time that this has been attempted, and we would like to be able to continue to highlight this as an original aspect of our work.

MINOR COMMENTS

»>Thanks for spotting these!

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

»>CHANGED

1,17; 1,19; 1,20; 1,30; 1,31-2,1 and other places: Include more references to support these statements. »>ADDED 1,20 and other places: Insert comma after “et al.”

»>ADDED

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, except for where the microbial diversity is relevant for culturing of cells. A third paragraph could be on debris fluxes from basal ice.

»> We will these new paragraphs. However, we think that the text flows better if we reverse the order and add a first paragraph on sediment fluxes and a second one on the microbiology of 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.

»>We do now mention in the text the geomorphological setting. Nonetheless, there

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are still sizeable areas where the glacier is contact with the moraine/till, and hence microbes can contribute to soil formation. As we discuss above, this has not always been the case – in the past, a much greater proportion of the foreland was dominated by discharge to till/moraine, so the results of the present study still have general applicability under these circumstances.

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?

»> Certainly, we can add some of these sorts of details to provide additional context about the site. However, some of this information seems to us to be somewhat superfluous. Again, the geomorphological conditions can change dramatically over time, but the point remains: microbial discharge has contributed significantly to proglacial soils in the past, and continues to do so where the ice is in contact with till/moraine. Certainly, we can clarify the nature of these changing conditions in our revised manuscript.

2,9: The period mark is red.

»>SOLVED

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.

»>Different measures have been used to describe sediment content in basal ice in different studies. Both these data, and data pertaining to particle size distributions

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have been reported elsewhere (Cook et al., 2010, 2011a,b). We will, however, give careful consideration about what information to include here.

2,19: Insert “a” before “layer”.

»>ADDED

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

»>ADDED

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?

»>Pervasive grain-boundary melting and refreezing (e.g. Tison and Hubbard 2000). This is a commonly used term (e.g. Hubbard and Sharp, 1995).

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 C4 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.

»> In an ideal situation it would have been great to have undertaken sampling in the manner described by Reviewer 2. At Svínafellsjökull, basal ice is not always exposed or well-exposed, and in many instances is not safe to access. During the 2015 campaign, three places showed conveniently accessible stratified facies for sampling, although this facies was observed in several locations. We were also driven to sample dispersed facies from the same locations (example, S3-D3) in order to be able to compare directly between ice types. However, this placed a further constraint on sampling.

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 Svínafellsjökull. Therefore, I will suggest that the authors consider switching the two paragraphs in section 2.4.

»>Respectfully, we disagree. We think that it would be better to start with the conditions at Svínafellsjökull since previous studies have calculated debris discharge introducing the way we have performed our calculation. Also, to clarify this point the equation formulae for sediment and cell discharge will be included.

3,16: Repetition. Delete this sentence.

»>DONE

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?

»>We will add further details about this – thanks.

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

»>This has been reported previously in Cook et al. (2010). We can add details about this in the revised manuscript.

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.

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»>We are not entirely sure what the rationale is here. Grain size distributions have been presented elsewhere (Cook et al., 2010, 2011a,b).

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).

»> This is a fair point, but in practice is rather difficult to achieve. A key point that we make here is that previous studies on glacial microbiology fail to adequately describe basal ice types, making it difficult to draw comparisons. We will consider this point, however, and add any further pertinent details and comparisons that we can.

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.

»» The aim of this research is not addressing the relationship between supra and sub-glacial environment or between basal ice and glaciofluvial or glaciolacustrine environments. We could expand the information about the presence of microorganisms in the glacier foreland, because it would be relevant for this work.

5,5: Delete “bacterial”

»>This number corresponds exclusively to bacterial counts, since it pertains to the counts of CFU in 1:10 TSA with cycloheximide, which prevents the growth of fungi.

What is meant by “who also found that cell counts increased with sediment content”? This relationship is not determined in the present paper.

»>Montross et al. (2014), found that the number of cells in the ice increased with the sediment concentration. Our results indicate that microorganisms are more abundant in the sediment entrapped in the ice (stratified is debris-rich, dispersed is debris-poor).

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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.

»>Basal ice facies are commonly described and differentiated by sediment content – different basal ice facies deliver different amount of cells to the glacier margin as a function of the different sediment content. As Reviewer #2 has highlighted that our explanation was not clear enough and for the final manuscript we will re-phrase in the text to make it more understandable.

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.

»>We cite one reference here because Lawson et al. (2015) is the only study that is similar to ours (i.e. which quantifies cell concentration). We could speculate about how other factors might affect microbial ecology here, but much of it probably falls beyond the focused remit of our study. We would be happy to add a new table comparing cell content in basal ice in different glaciers if wanted.

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.

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»> We are not sure if we have understood this comment correctly, but we think this is similar to an earlier point about integrating data from other studies. We outlined earlier, in perhaps the best studied site (Russell Glacier), that there are large gaps in the dataset that would need to be filled in order to derive these cell discharges. It is a good idea but we are not aware of suitable data to make these calculations with any confidence.

7,6: Is there a name missing here?

»> No, but we do need to remove the “J.” – thanks for spotting this.

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

»> That text is accrediting the source of the image

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