

Overall, there is a neat and generally well written paper here that reports on the

The authors have made a substantial effort to respond positively to the three Reviewers comments. Several of the major comments raised by the reviewers were common to those reviews. A read through the authors response document (while evidencing some repetition) highlights they have considered all points raised, and made positive modifications in the majority of the cases.

In consideration of the variety and wealth of revisions the authors have fully and effectively detailed, it seems prudent to simply examine this paper very much as a new submission, and consequently, the review that follows adopts this strategy.

There are a few more significant points to note here that are worthy of highlighting:

1. The authors still do not seem to fully describe the seasonal (and synoptic) dynamics of the weathering crust. This was a point raised by several of the reviewers previously, and now in later sections the authors refer to the “transient” nature of the weathering crust (hereafter, crust) – however, they do not seem to give the reader a clear indication early on as to the causes behind the transience. See next comment.
2. The authors, in response to commentary in previous reviews regarding the supraglacial conditions over the summer season, have included wording regarding “supplementing ... measurements with ... meteorological... [records]” (see Section 2, P3). However, it seems these data are not presented, nor reported or discussed in any meaningful way. This seems to be a missed opportunity as, for example, they would be able to discuss the refreezing potential or synoptic variability immediately around their period of observations. One can't help but feel that by placing the ablation season meteorology (short wave radiation, air temperature, rainfall, ablation) in a figure, indicating the end of snow cover and timing of their observations - the authors could make a far more convincing argument relating to the refreezing, the transience of the crust and so on – points they are keen to expand upon in their results and discussion. It seems a very simple and effective way to really make the paper excellent with clear support for the arguments posited. And would demand relatively little material change to the main text, which is generally sound but could be bolstered by including those data to support arguments.
3. The general methods and calculations sections which were comment upon in the review process are now much more clear and robust. Similarly, the description and initial discussion of the core results are appropriate.
4. The discussion could potentially benefit from some further minor tweaking – this section could be condensed to make a more punchy and impactful paper here.

For far more minor observational comments, some of which relate to the points above, see the listing below.

P1.

Title: Ice Sheet should be capitalised.

L11: Contradiction with L14, either the ice is saturated or it is partially saturated at depth, and unsaturated near the surface. Suggest clarification or removal of saturated in L11, and move to L13.

L29: suggest “for Greenland”

P2.

L2 and 4: Perhaps clarify that the former is for the ice sheet, while L4 refers to valley glaciers elsewhere.

L5: “inferred to relate to the presence” – none of the studies have definitively demonstrated the role of the crust, and so suggest change “owing”.

L7: Yes, the crust is a substrate for the development and deepening of cryoconite holes, suggest reconsider use of “substrate” here – “locus”?

L8: Suggest splitting sentence relating to firm as its own. Refreezing is not transport, so there is disjunction here.

L14: suggest reference to Figure 1 here, or at least in this paragraph.

L15: The seasonal or synoptic development should be noted here. The crust forms in the melt season when glacier ice is exposed. And note that the as synoptic conditions vary over the ablation season, the depth and degree of evolution of the crust may vary.

L21: Not all the crust is saturated, is it?

L22: Isn't Irvine-Fynn et al (2011) a better citation here that describes the crust formation?

P3.

L1 or 7: For relevance, the Hotaling et al 2017 would be a sensible inclusion here, perhaps as an additional note as to the relevance the crust may have for glacier surface ecosystems. (Hotaling, S., Hood, E. and Hamilton, T.L., 2017. Microbial ecology of mountain glacier ecosystems: Biodiversity, ecological connections, and implications of a warming climate. Environmental Microbiology.)

L9: these? Unclear. For new paragraph. Surely better to phrase: “Despite the hydrological and ecological implications...”

L16: Delete “hydrologic system... balance” as repetitive of previous sentence.

L24-25: This seems to read awkwardly, consider condensing a little.

L28: You do not present any meteorological data here. This then becomes rather irrelevant as the reader can not access the rates of ablation, the radiation records or ascertain if night-time refreezing may occur.

P4.

L2: Not convinced “posting” is the correct word, and considering an international readership – a more standard word would be more prudent. Check dictionary – as “posting” typically refers to a singular location. Here it seems “interval(s)” is the more appropriate and accessible word to use. Please consider revising throughout. This was noted by previous reviewers.

L7: What is the manufacturers accuracy for the Acculab scale, perhaps cite this here?

P5:

L3: clarify “specific water storage”.

P6.

L2: Suggest checking TC formatting for quotes or subjective terms – if using “...” for a quote, perhaps consider ‘...’ for a more subjective term (as ‘true’ here). Not sure of formatting expectations, but may be worth considering or removing here.

L10: consider replacing “drill bit” with auger where appropriate, to better clarify between corer and auger.

L14: should i.e. be in brackets? Or preceded with comma.

L15: unclear – suggest revisiting for clarity, issues with “posting” again.

P8.

L6: suggest rewording to: “to unweathered glacier ice” and strongly suggest citing Muller and Keeler (1969) and Irvine-Fynn et al., (2011) here too. Most of the examples show density only over a few decimetres at most, with glacier ice density typically being reached within this depth, whereas the deeper scenarios relate to stagnant ice where there is no replenishment (see Fountain and Walder, 1998).

L9: Tighten language here as not quite clear, and might using “respectively” be appropriate, rather than “vs.”.

L10: Drilling, coring – unclear. Perhaps seek to clarify or note this is both for cores and augers.

L23: A reference that reviews ice fabric or structure here might be sensible. Support for the notion would be appropriate.

L24 and L27: should km and cm both be written in full here?

L31: Could be the optical properties of the crystals – if crystals are large and clear and devoid of air bubbles or other inclusions, then the internal melting may be reduced and shortwave radiation simply passes to the next more optically opaque layer. Suggest a minor clarification here, if appropriate. Or later.

P9.

L18: again postings is unclear – “locations” and reduce confusion with the systematic intervals used for the profile vs. the other sampling locations.

L19: These ‘dry’ observations were not all on one day while ‘wet’ were other days? Might be worth just noting that, if the case.

P10.

L7: “ice comprising two layers...” perhaps

P11.

L5: You don’t really describe the transience in the introduction, and so perhaps need to note this there, so that this does not come in unexpected or unjustified.

L9: Comma not needed after Tedstone here. Check references throughout.

L24: Perhaps consider opening the discussion with a brief summary of what the core “take home” results you’ve presented are, then move on to the comparisons. Again, just short text addition here

which may be beneficial. Also suggest initial subheading for the overview section (crust form/structure?), that then leads to the melt storage volume and other implications.

L29: Repetitive with “storage” – consider using “transient reservoir in Greenland’s bare ice ablation zone”.

P12.

L6: Suggest include Munro (2011) here and remove Fountain and Walder (1998).

L8: Previous work hasn’t reported ice structure so “unlike” is not really correct. Consider noting that this study has also reported ice structure, which hasn’t been reported in previous studies of the weathering crust. You used this assertion elsewhere, and it might be prudent to downplay this criticism.

L11-14: explain a little more, see earlier comment regarding optical properties of crystals and density of interfaces.

L15: centimetre-scale in full

L16: do you mean melt not weathering?

L15-20: Here, and in previous sentence(s), the use of and position references seems unusual. Consider revisiting and tidying up and condensing a little.

L17: delete “terrestrial lithology” phrase, it is unhelpful.

L26-28: Temperature conditions are very different in Antarctic setting, are they not. And the complexity here is not well presented. The refreezing process releases latent heat, which may lead to melt that off-sets the ‘sink’ described. Consider being more physically accurate and detailed here.

P13.

L7: Think this could be more clearly written here. E.g. Our study catchment is equivalent to 2% of the 2800 km³ contributing area draining to the Waston River. But not sure what this statement adds. You’ve highlighted the water volume, so why does the contributing area matter? Surely you can state that the water volume is equivalent to 1hr of peak Q in 2012, or is the same as Nhrs of Q (see Overeem’s Q data series perhaps).

L9: Consider “density of porosity more widely over the GIS ablation zone” – vast is subjective.

L11: “en-, sub- and proglacial systems”

L12: While relevant and appropriate, this might be viewed as moving slightly away from the data itself and tending toward conjectural discussion, and could be condensed while maintaining the effectiveness. You are speaking beyond the data here, and it would perhaps be more effective to keep this to a shorter section which is more concise and focused. Fundamentally, you are simply saying that by finding a crust on GIS, you now know that ablation measurements that fail to include subsurface melt may be problematic to use for validation, that water storage may vary over time due to the dynamic behaviour of the crust, that assuming rapid runoff may not be appropriate, and the hydraulics of the crust may have impacts on impurity and ecological mobility and residence time (ie. albedo and biogeochemistry). Can you not simplify the section?

L15: Neither LaChapelle or Muller & Keeler are ice sheet studies. Remove “sheet”. And surely Munro (1990) here too.

L24: not sure dubious is the correct wording here. “less reliable” or “less robust”?

L32: But other models have included supraglacial ponds etc. so this seems to be a little misrepresentative.

P14.

L5: Doyle’s work is for late summer when the crust is likely degraded, so perhaps not as appropriate as hoped. Could be removed entirely.

L20: Not sure entrainment is the correct word. Perhaps “retention”.

P15:

L26: UAS – only use here, know this is drone imaging, but be clear throughout.

Tables and figures:

Figure 1b – the density curve given seems slightly at odds with the cores and previous estimates (LaChapelle 1959; Muller & Keeler, 1969). Suggest slight reduction in the exaggeration of the low density surface portion of the plot.

Figure 2 – consider a-d to clarify insets, and in caption. Could present lower panels at top, and site below (ie. reverse the figure). Suggest scale given either on image or in text for insets b and c.

Figure 8 – indicative scales would be beneficial. Explain the various images fully please.

Table 1 – define mu symbol