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Interactive comment on "Microbial processes in the weathering crust aquifer of a temperate glacier" *by* Brent C. Christner et al.

Anonymous Referee #2

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General comments: The authors present a detailed study of near-surface ice of a temperate glacier in Alaska. The specific focus is the microbial habitat or ecotone that this near-surface environment may represent. Numerous recent studies have now begun exploring this, conceptually or otherwise, but this is the first paper to conclusively demonstrate this environment is active for a temperate glacier surface. Data include observations and modelled estimations of ice temperature, biogeochemical measurements and microbial community composition. The paper concludes that the weathering crust that the near-surface represents is a viable location for microbial activity, and estimates of biomass growth are made. The paper is well written in the main, and methods are fully detailed. There are no critical experimental flaws to note, and data presented appear robust and sound. Figures are fair and reasonable in general. Some aspects

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of the introduction and discussion/conclusion could perhaps be strengthened to help benefit the paper's narrative, focus and impact. That said, the paper was a constructive and informative read.

Specific comments: The more significant observation is that the use of borehole thermistor data from 15m to 45m seems to be somewhat inappropriate. This is outside the zone of interest, and the discussion of these results alludes to weaknesses in both the thermistor data itself and the modelled ice temperature, at least to make confidence in both aspects rather clouded. This detracts and confuses the paper, and takes the reader into areas that simply add little to the focus of the paper. Recommendation would be to simply use the modelled surface temperature profiles to 15m as a proxy for the temperature conditions. The other thermistors lie outside this, so while useful to test the broader scale applicability of the thermal model, the deeper measurement points can not really be used to validate the near-surface model, particularly with the uncertainties discussed, and so seem to be simply adding data unnecessarily (nor essential) to the paper. Suggest removal of all information on the uncertain borehole data. Simplify this to the use of the model as a proxy estimate for the surface conditions. Discussion can then allude to the need to better characterise the thermal behaviour of the WCA, and instrument the uppermost few metres, including adding the potential influence of snow cover on thermal regime over the full annual cycle. P2: Opening paragraph, seems to slightly confuse the goal and focus of the study, and could be seen as rather weak and less targeted than perhaps might be achieved. Recommend revisiting and reworking. Now, the concept of glacier surfaces as an ecosystem (e.g. Hodson et al., 2008; Stibal et al., 2012, Nature Geoscience; Hotaling et al., 2017, Environmental Microbiology) is well-accepted, and the references particularly to firn storage rather distract from the core topic here which is the shallow near-surface ice in the ablation zone. Preference might be to keep the study's focus clear from the outset to better guide the reader. Perhaps better to consider Cooper et al. (2018, Cryosphere) and Smith et al. (2017, PNAS) who focus on water storage in bare ice in Greenland, or delayed runoff in mountain glacier settings (Munro, 2011, Hydrological Processes).

Consider opening with need to understand the ice surface as a locus of biological activity, relevant with recognition of water storage and delay. This would better guide the reader into the material that follows. L25: Perhaps reference to earlier LaChapelle (1950s) work on this, and Munro's (1990) examination of subsurface melting would be appropriate to evidence knowledge of this phenomenon. Similarly, Muller and Keeler (1969) were the first to use "weathering crust" specifically for glacier ice, although others used ablation rind, honeycomb ice, ablation crust etc. (see earlier discussion texts regarding glacier ice structure) – perhaps revise citation position to highlight and better evidence this and source of the terminology. P3 L2: Particularly for temperate glaciers – could be emphasised, given this is one aspect of novelty here. Note, here, given Larson's (1977, 1978) studies on water balance and meltwater storage in the near-surface of an Alaskan glacier, it is surprising these references are absent, and they could be helpful for defining or better justifying the depth range of of WCA zone examined and sampled here. See earlier comment regarding maintaining clear focus in the opening sections. See also Fountain and Walder's (1998) comments on this ice 'zone'. P4 L14: The authors hoefully should be aware of Stevens et al. (2018, Hydrological Processes) as well as Cooper et al. (2018, Cryosphere) – perhaps here, and/or elsewhere, recognition could be given to work examining porosity and permeability of the WCA. This is a current topic, and updates to sources cited could be included to keep the paper contemporary. P5 L23: Perhaps missed something in this section, but the "water content" aspect isn't perhaps as clear as could be effected. This section seems to describe temperature in detail, but does not quite give a sufficiently direct indication of water content calculations, especially if calculated for the model domain rather than the porous layer itself. Suggest clarification for readers less familiar with thermal models for ease of accessibility, given water volume is later reported in figures. L5 L24: For the WCA, typically only a few m, the use of a 45 m borehole seems to be excessive and rather misaligned with the zone of interest (see earlier comment). Similarly the model of 100 m seems to lie outside the region of observations. Note misalignment of information between the 45 m borehole and the 20 m of thermal data presented, update

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caption to reflect this – and perhaps reflecting earlier suggestion of reducing the use of less certain thermal data. While use of deeper temperature records to seek to validate the thermal model is admirable, there seem to be significant uncertainties both in the model and the thermistor data that render this approach equivocal and conjectural. See comment above, which may help here. P16 L20: Fountain and Walder (1998, Reviews Geophysics) discuss the WCA and Irvine-Fynn et al. (2011) phrase this as a "perched aquifer". Also see Stevens et al. (2018, Hydrological Processes), Cooper et al. (2018, Cryosphere), and Smith et al. (2017, PNAS). L27: Consider revising as the WCA may be shallow (see LaChapelle's earlier work), and while lower density (by volume) ice may exist at the surface, typically the water table lies below this. See Muller and Keeler's (1969) conceptual diagram of this, and as evidenced by water depths in cryoconite holes. It would therefore be unlikely that ice would show a 50% liquid water by volume, rather this might be the very weathered surface ice, from which melt drains to the denser near-surface. Similarly, the Antarctic comparison seems to understate the thermal conditions both at surface and subsurface, which would influence WCA development. Recommend adjusting the wording here to reflect this and making comparisons more relatable. P18 L4: given the temperature model excludes snow cover (see Methods description), is the 7.5 months realistic, and does this tally with in situ snow cover, which itself will affect WCA development and closure? Perhaps revise values here to reflect this, or caveat that this neglects the influence of snow on the WCA – a process yet to be reported. P20 L12: This section becomes more conjectural and loses focus on content of paper. Suggest revising conclusion to reflect the data and relationships shown. Nothing here directly reports on biological influence on the albedo of glaciers. Rather, identification of a functional microbial environment on a temperate glacier – which supports ideas and aspects revealed in Arctic and ice-sheet settings – is important. Note, there are perhaps two 'definitions' of "biological darkening": one potentially introduced in Irvine-Fynn et al. (2012, Environmental Microbiology) referring to microbe-mineral retention; and secondly, the more recent phrasing used when referring to the 'bio-albedo' and apparent darkening of some ice surfaces by active

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biomass (e.g. van den Broeke et al, 2017, Current Climate Change Reports) which was termed "biotic acceleration of glacier melt" (Koshima et al., 1993, IAHS). However, the processes underlying these definitions are subtly different. Importantly, none of the references cited specifically use the phrase "biological darkening". Much of the bioalbedo is related to ice surface algae, not necessarily the same community as that in the WCA. Please use quotations correctly, and recommend retaining focus on actual data and findings of the paper rather than seeking to link to other topic areas.

Technical comments: A number of more stylistic, but nonetheless important observations include: P2 L20: Scott et al. (2010, Ann Glaciology) show microbial nutrient turnover in supraglacial streams, which may be relevant here. L21: Please use multiplication not the letter 'x'. Noted elsewhere throughout (e.g. P6 L14). P6 L8: Check journal style, but perhaps revise unit to °C. P7 L3: Confirm KPAR is K in equation. P9 L9: Check journal style for unit / constant here. P10 L30: Define rs and rp here, as used elsewhere in Results section. P11: Results, please check stylistics as throughout there are contrasting uses (or absences) of '0' before decimal points. A pet peeve is some journals/publishers that have removed zeros from quantities – whether numeric measurements or statistical values. P11 L17: revise and condense. Near-surface is unlikely, by definition, to be 45 m at depth. P12 L15: Slight repetition from Methods. Suggest simply presenting equation in methods including citation, and referring to this here with result. P12 L19: See Larson (1970s) references to support this depth of WCA or photic zone, if thermally still below zero. Consider further in discussion. P13 L12: see earlier comment re. definition of correlation coefficients. Condense. P14: Check style for p-values. Italic elsewhere. See also P19. P17 L11: unit consistency, elsewhere L/m2, later here, use of superscript negatives for 'per'. Recommend check and edit. P17 L13: Repetition of P12 L11-20, and immediately above. Revise, avoid repetition. P18: L7: Again, perhaps see Stevens et al. (2018). L18: useful to consider Xiang et al. (2009, FEMS Microbiology Ecology) as this is not altogether a new concept and has been discussed in the literature. L30: "emergence of ice in the actively melting ablation area" might be a stronger phrasing. P19 L9: Unclear why access to

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bed is relevant here, suggest simply noting the fracture networks present in temperate ice may provide an explanation for near-surface to englacial linkage might be sufficient, particularly for emergent ice in the ablation area. L31: use “more than” rather than > here.

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