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The Cryosphere Discussions

Interactive comment on "Ground-penetrating radar reveals ice thickness and undisturbed englacial layers at Kilimanjaro's Northern Ice Field" *by* Pascal Bohleber et al.

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Bohleber et al. surveyed the Northern Ice Field of Kilimanjaro for reconstructing its bedrock topography, ice thickness and internal stratigraphy, using ground-penetrating radar (GPR) at various frequencies. Despite GPR being widely used in glaciology nowadays, this work is the first of its kind on Kilimanjaro, and therefore represents a novel approach in the exploration and investigation history of this mythical mountain. This study is well written, and I believe that the conclusions are scientifically sound and will contribute significantly to the future investigations of local, and other tropical, glacier recession dynamics.

As a general advice for improving this manuscript, I would suggest the authors to

strengthen their point where it is not stated carefully, or where the implications or interest for the scientific community are overlooked. These comments do not diminish the quality of this work though ; therefore I recommend publishing this paper with minor revisions as described below.

- Page 1, Line 7: "indicating an undisturbed internal stratigraphy within NIF's central flat area".

Whereas other statements of minor importance have been stressed more cautiously, I believe that this statement is too assertive and should be rephrased more carefully. Clearly some unknown uncertainty remains in this regard and, without drilling a new ice core between the former drilling sites and the edge ice cliff, without the result of the ice cliff dating work mentioned in the paper, and without carrying ice flow modelling investigations, no clear or solid information is available to certify that the internal stratigraphy is undisturbed. The influences on ice flow dynamics through time and space of, first, near-surface and internal meltwater and, second, fumaroles, still need to be better documented in order to fully appraise potential issues on the ice stratigraphical integrity. This comment also stands for the sentences on Page 9, Line 6 "We thus conclude that the internal stratigraphy within the NIF central flat area is generally undisturbed", and on Page 9, Line 32 "[. . .] revealed an undisturbed internal stratigraphy".

- Pages 4-5, "2.3 Uncertainty considerations" section

Here the vertical error in internal reflection horizons (IRH) tracking is discussed. How about the horizontal uncertainty related to the various GPR pulse triggering methods used (wheel, time, manual)? In other words, what is the horizontal extent of potential bedrock/stratigraphical discontinuities that the method used might omit while progressing on the glacier surface? This is of potential significance in regions of increased meltwater/fumarole activity, where electromagnetic coherency is more prone to disturbance.

- Page 5, Lines 12-14: "Assuming 0.3 m uncertainty in the length of the rope at 16 m (mainly resulting from knots tied into the rope)".

From personal experience, the error stated seems rather low. In addition to the tied knots mentioned by the authors, the type of rope, its elasticity, and the mass of the dead weight at its end will certainly contribute. The uncertainty given here is therefore clearly a lower estimate.

- Page 7, Lines 21-22: "The low ice thickness is likely a result of the surface gradually sloping off towards the west outside the caldera. A distinct rise in the local GPR bedrock reflection appears where the location of the crater rim below the ice is suggested by satellite images (Figure 6, and small insert therein)".

The size of Fig. 6 inset is way too small to be able to observe this. This inset could certainly be resized to the dimensions of the main figure. In fact, it should, given the importance of the authors' point here.

- Page 7, Lines 23-24: "This finding implies that the local bedrock relief features may have affected past ice build up and decay through limiting exposure to solar radiation and wind".

I find this argument somewhat weak here – one would either need to check this limiting exposure effect with e.g. an insulation model, or provide more (visual?) details.

- Page 7, Lines 28-35: "Considering additionally the coarse resolution used in the kriging approach, we regard the values derived from this method with caution only. The estimates of total ice volume obtained from the Grid approach and DEM-only are (12.0 ± 0.3) and (14.3 ± 1.3) 106 m3, respectively. Evidently the main contribution to the difference in ice volume comes from different mean ice thickness values (using the 2012 surface area the mean ice thickness obtained from the Grid method gives a volume of (12.3 ± 0.3) 106 m3). The decrease in mean ice thickness suggested by the comparison of the two interpolation methods is not supported by surface height change measurements 2012–2015. Since both interpolation methods use the same

surface topography supplied by the DEM as input, the difference in mean ice thickness has to come from differences in determining subglacial bedrock. Consequently, the difference in ice volume estimates is not used to infer a rate of ice loss."

I wonder what is the added value of discussing the 'Kriging' method here, given its obvious flaws at such a low sampling resolution. There are various other interpolation techniques worth trying I think, that are not involving such a coarse resolution data grid.

- Page 7, Lines 31-33: "Evidently the main contribution to the difference in ice volume comes from different mean ice thickness values (using the 2012 surface area the mean ice thickness obtained from the Grid method gives a volume of (12.3 ± 0.3) 106 m3)."

There should also be another source of error introduced in the volume calculations through the fact that ice cover area is simply multiplied by ice depth here, which is valid for a rectangular prism. The numbers given are thus upper estimates of the glacier volume.

- Page 8, Line 2: "we regard the ice volume estimate of the Grid method as most accurate".

As mentioned for Page 7, Lines 28-35, this statement is somewhat trivial here.

- Page 8, Lines 12-13: "It is worth noting that the vertical cliffs show instances of tilted and converging layers in close proximity to bedrock".

Instead of 'converging' layers, the pattern in question rather looks in my opinion, from visual inspection of Fig. 8, like a layer from which another layer is swelling as a result of a rheological discontinuity (e.g. localized shearing), as often occurs at the margin of glaciers. This has potential implications not only for the detection of deep reflectors as stated by the authors, but also for the integrity of the ice layering. This comment, which I believe needs to be discussed in the manuscript, highlights my former comment on Page 1, Line 7 regarding the authors' rationale and uncertainty analysis on the argued 'undisturbed internal stratigraphy'.

- Page 8, Lines 14-15: "[. . .] where ice thickness decreases rapidly due to the crater rim".

I do not think that the presence of the crater rim is the only reason for this 'ice thickness decrease'. In the case where, say after a period of increased accumulation rate, more ice would flow towards the ice rim, ice thickness could in fact increase as a result of the blocking effect by the rim. In the case discussed by the authors, it is probably the conjunction of the rim vicinity and stagnant flow that causes the ice to reduce locally in thickness.

- Page 8, Lines 20-23: "It is plausible that the according change in the electrical conductivity of the ice layer produces a strong reflector seen in the GPR data (Sold et al., 2015). Accordingly, this strongly suggests dust layers being a main physical cause of IRH at NIF. Thompson et al. (2002) and Gabrielli et al. (2014) report visible dust layers in the NIF2 and NIF3 ice cores".

If the change in electrical conductivity expected from the ammonium and chloride documented by Thompson et al. (2002) results indeed from dust layers, a consequent change in ice crystal texture should also be expected, given the retardation effects of micro-particles on grain boundary migration and recrystallization. IRH might thus represent "iso-chemical" AND "iso-crystalline" reflectors.

- Page 8, Line 33-Page 9, Line 8: discussion on IRH 1-5 tracking.

This discussion could be somewhat improved and made much clearer with the use, for instance, of a table giving (1) the expected depth of these horizons from previous ice cores, and (2) their depth detected by GPR. The total lengths between the drilling sites, the ice cliff, and the locations where the IRH tracks are lost would also be helpful in order to appraise the layer continuity/extension.

The ratio of vertical distances separating the IRH discussed at various locations would also help evaluating the vertical stratigraphical dilatation/shrinking along the studied

profiles.

- Page 9: Lines 9-19: discussion on continuous layering.

It is not clear, from this paragraph, where the authors want to lead the reader. It is only after reading the Conclusion section that one is able to get the authors' point regarding the importance of stratigraphical continuity between the former drill sites and the ice cliff: they are concerned about the possibility to efficiently and confidently relate the results from former ice cores to the results of the ice dating work along the ice cliff. This concern is totally justified here, and should be wrapped up more tightly in this section.

- Page 9, Lines 15-19: "Although qualitatively going in the same direction as the adjustment of the NIF2 and NIF3 stable isotope records (i.e. in comparison with Figure 2 in Thompson et al. (2002)), tracing IRH between NIF2 and NIF3 suggests tie points that are systematically at greater depth in NIF3 as compared to the ice core stable isotope matching."

Do the authors have an idea about why the ice stratigraphy is stretched at NIF3? Differences in accumulation cannot really be invoked here given the small distance between both NIF2 and NIF3 sites. Ice flow would probably play a role, which is difficult to determine without ice flow modelling though.

- Page 9, Lines 26-29: "Hence our GPR profiles demonstrate a highly heterogeneous presence of meltwater near the surface, apparently a wide-spread feature at NIF related to spatial and temporal variability in surface characteristics and processes (Hardy, 2011). This finding is of relevance for any new ice core drilling efforts at NIF in the future, and an important consideration for energy and mass balance modelling efforts."

Although this section is called "Effects of near-surface meltwater", these effects are not really discussed. The authors are only referring to this issue as "of relevance for". I suggest that they either discuss this important issue more thoroughly, or suppress this section. This comment also applies to Lines 11-12 in the Conclusion section.

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