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

- Response to Comments from the Editor -

General Remarks: *All line numbers in "Changes to manuscript" refer to the revised version. To differentiate from earlier changes made during the peer-review, new changes in the corresponding pdf of the revised manuscript are now highlighted in blue.*
Author's responses to the editor's comments are in blue.
All new references used in this text here can be found in the revised manuscript.

Comments from the Editor

The authors responded to reviewers adequately. However, this manuscript should be improved further to be accepted by the Cryosphere.

We appreciate the editor's effort to help us to further improve the manuscript at this late stage of the process. We provide a response below and present the improved manuscript. In doing so we believe we have further improved the scientific quality of this work and hope for a timely completion of the peer-review/editor review process.

Major scientific issues

1. The main conclusion of this paper is the presence of uninterrupted, spatially coherent layering, but the presented evidence is weak.

Demonstrating the existence of some spatially coherent layering is only one of the conclusions of the paper, which has also yielded the first map of ice thickness and permits volumetric estimation. This paper also provides a stratigraphic context for ice samples obtained during two prior expeditions, and which have yielded the first accurate ^{14}C dates of Kilimanjaro ice.

We made an attempt to provide additional supporting evidence for our conclusion regarding layer coherence by including i) additional visual evidence of layering at the wall stratigraphy (Figure 8) and ii) showing all 200 MHz profiles in a new supplementary

Figure (also meeting the editor's request in 1.1 below).

We believe the evidence we provide (see the now included full set of radargrams in the supplementary figure) is strongly supporting coherent layering. This finding is not solely based on the GPR investigation but clearly supported by visual evidence from wall stratigraphy all around the NIF (revised Figure 8). Following the two peer reviews, in the revised manuscript we took additional care not to overstate our point regarding the layer coherence. For instance we specifically state that, as far as the GPR layers are concerned, we are referring to roughly the upper 30 m only and discuss limitations to the visibility of GPR layers by noise from near-surface meltwater.

We made an attempt to provide additional supporting evidence for our conclusion regarding layer coherence by including i) additional visual evidence of layering at the wall stratigraphy (Figure 8) and ii) showing all 200 MHz profiles in a new supplementary Figure (also meeting the editor's request in 1.1 below).

1.1 Present much longer radar data. Now the authors show only 160-m-long profile (Fig. 2) and argue that the layering is well preserved in all profiles (it is said "all profiles" and later "nearly all profiles" or such, please be consistent). Apparently, the presented evidence is inadequate to support the claim. It is hard to see whether the radar reflectors are really continuous or not in Figure 4. Figure 9 can be more meaningful if more extensive radar data are presented.

The purpose of Figure 2 is to provide a characteristic example of 100 and 200 MHz processed GPR profiles over the same horizontal distance. We intentionally restricted the horizontal distance to 160 m for better visibility of characteristic features such as noise by near-surface meltwater. We show an additional 150 m of 200 MHz profiles in Figure 4. Regarding internal layering we refer to the 200 MHz profiles only, and argue that coherent internal layering is generally detected in all profiles. Attempting to quantify the extent to which the internal layers can be traced throughout the profiles we state on page 9, line 12-14 that IRH4 is the deepest reflector that can be traced in almost all profiles. This is accurate, since it was not possible to trace IRH4 unambiguously over two short intervals, towards the eastern end of the plateau area and above the rise of the crater rim towards the west (this corresponds to the data gap in Figure 9 b) vs. a)).

In order to adequately address the request for more data we have made a new Figure that should be added to the paper as a supplement. It shows the entire 200 MHz profiles collected, divided into individual segments to aid visual perception. In our view the data clearly shows the major reflectors that extend throughout all profiles and which we associate with dust bands .

We also feel that it is necessary to point out that, compared to the typical standard in GPR profiles obtained over the interior of the polar ice sheets, not the same degree of clarity of IRH can be expected at warmer small scale mountain glaciers, and in particular at Kilimanjaro's NIF. This becomes especially evident with respect to the different ice formation process and the occasional presence of near-surface meltwater. In this context we use the term "uninterrupted" as the opposite of deformed, macroscopically disturbed layering. We have added text to clarify this more.

We would also like to point out that, with respect also to the 100 MHz profiles, we have already uploaded the entire dataset of ice thickness estimation based on the GPR bed reflection to the Pangaea repository (including both TWT and depth).

Changes to manuscript:

- Added a new Figure as supplementary material to show all 200 MHz processed GPR profiles
- Page 9, Line 19: "(as opposed to deformed, macroscopically disturbed layers)"

1.2 Revise Figure 2b using multiple color (not gray scale) so that the layering structure can be more clearly seen.

We have tried different color schemes and do not believe any scheme provides better visibility of layers . We have left the gray scale for Figure 2 but chose a color scale for the Supplementary Figure, thus providing both options for the reader.

1.3 Explain why 100-MHz radar data show less uninterrupted layering than 200 MHz. In general, lower frequency (longer wavelength) radar show more continuous layering. Why does this frequency difference occur, and why can you argue uninterrupted layering despite of limited features imaged by 100 MHz radar? (or why do you trust 200 MHz data more than 100 MHz data)

The answer to this is that while lower frequencies can penetrate deeper into the glacier, higher frequencies such as 200 MHz have better vertical (and horizontal) resolution. Thus, 200 MHz provides the better image of the dielectric contrast produced by thin (dust) layers. As an example for NIF, Thompson et al. (2002) report the most distinct visible dust layer to be 3 cm. This still results in a distinct reflector at the vertical resolution of 42 cm at 200 MHz. However, the 100 MHz profiles (at 84 cm vertical resolution) do not reveal a clear individual reflector anymore. We have added text to briefly explain this interpretation. Worth mentioning along these lines, other studies typically also chose to use frequencies of 250 MHz for investigating internal layers at small scale glaciers (e.g. Eisen et al. 2003, Konrad et al. 2011).

In general, the editors question opens a wide field, best to be answered by multiple frequency surveys and measurement of dielectric properties at an ice core in high resolution. This is clearly off the focus and out of the possibilities of this study, but we will take that as suggestion for our next alpine coring.

Changes to manuscript:

- Page 6, Lines 32-33: "(due to the coarser vertical resolution at lower frequency)"

1.4 Abundant presence of meltwater found in shallow cores (P10L7; by the way how shallow are they?) infers the presence of isolated scatterers (percolated waterbodies into the deeper ice) and possible disturbance of the ice stratigraphy. With this shallow core evidence and inadequate presentation of the radar data, I cannot immediately support author's argument on the uninterrupted layering.

As stated on page 10, line 10, our shallow drillings reached only to typically about 0.6 m depth. Drilling deeper was severely hampered by water filling the holes. We discuss on page 10, section 3.3 that the presence of meltwater has been observed intermittently over the past years at NIF. This means at other times the glacier appears entirely frozen. At the time of our survey, meltwater was being produced at some locations and the GPR profiles show this accordingly (we agree that meltwater produces isolated scatterers and hence noise in our GPR profiles). The added Figure as a supplement shows the full extent of this effect, both laterally and in depth.

Regarding the effect of meltwater on internal layers, however, we believe that our conclusion regarding layer coherency remains valid, although meltwater-introduced noise near-surface can make the detection of IRH at depth more difficult (page 8, lines 22 ff.). Notably we are already pointing out the relevance of the detected meltwater presence with respect to ice core records, i.e. especially concerning stable water isotopes that are known to be easily disturbed by meltwater.

2. Ice thickness is estimated towards the western side of the NIF, where no radar data were collected (Fig. 7). However, ice thickness in that region is not at all data supported, and this affects the estimate of ice volume (Grid method). The authors discussed uncertainties in ice thickness, but such discussion can be valid within the area where data are present (central flat area). The sudden increase in the slope may be associated with the elevated bed near the boundary of the flat and steep areas (I.e. dam-up of the ice).

As we discuss in section 2.1, it was not possible to walk everywhere with the GPR antennas due to rough surface terrain. We attempted to achieve the best possible coverage with our profiles, and our 100 MHz profiles extent over large portions of NIF, not just the central flat area. Nonetheless we are aware that the coverage is incomplete and the need for interpolation arises. This is in fact why we combine the GPR data with the DEM to interpolate ice thickness and estimate ice volume, because this means that additional constraint for interpolation was provided by the DEM.

Major presentation/structure issues

1. Stake height changes are presented in Figure 5 and constitutes a major part of discussion in Section 2.5. However, it is not mentioned at all in the methods and suddenly appear in the results section. Please mention stake methods (i.e. locations of the stake, measurement

periods etc) and AWS in Section 2.1.

We thank the editor for pointing this out. Since Figure 5 was added somewhat late to the manuscript and is mostly based on published data we had not added details on the method. Section 2.1 is exclusively dealing with the GPR survey setup, hence we decided to add details on the stake measurements at the respective first mentioning in the Introduction as well as by extending the caption of Figure 5. Locations of the stakes and measurement periods are all summarized in Figure 5 and we have added the location of the AWS in Figure 5 and also in Figure 1 b).

Changes to manuscript:

- Page 2, Lines 5-8: “comprehensive automatic weather stations (AWS) and network of mass balance stakes…”
- Added AWS location to Figure 1b) and Figure 5
- Page 7, Line 17-18: “the cumulative surface height change measured by two ultrasonic sensors at the AWS, close to NIF2, is -4.24 m.”
- Caption Figure 5: “Ice surface elevation change at NIF derived from ablation stakes with at least two consecutive measurements (increasing from n=1 to n=19 stakes, in 2000 and 2015, respectively). The AWS and spatial coverage of stakes at NIF are shown next to the legend in the upper left (black and red triangles, respectively). In the top plot, grey box plots represent the distribution or change in ice height (median, quartiles) at vertical or near-vertical stakes (< 30 tip; height measured along stake). Thick horizontal blue lines show the mean height change, or when only 1 measurement (i.e., 2001-2004).”

2. The surface topography is shown only in Figure 6 but the authors say “flat central basin” from the beginning of the paper. Please re-arrange the figures so that the satellite image and surface topography are presented in Figure 1 to give the full topographic framework. Both of them are not author’s original work so it can be presented as background knowledge.

Although we see the logic behind this comment, we doubt rearranging the Figures would be beneficial to the reader, since we have deliberately chosen to show the individual details of the Figures for the following reasons: The reason for not showing contour lines of topography in Figure 1 is that having a second set of lines makes it more difficult to recognize the GPR profile lines – which is the more-important element of the paper. The reason why the satellite image is in Figure 6 is the discussion of the crater rim, and we have enlarged the image to become a separate part of Figure 6 following one of the referee's comments. The surface topography is shown again together with the GPR ice thickness in Figure 6 because these are both input datasets for the interpolation of ice thickness in Figure 7.

Minor points

1. "internal" and "englacial" are used in an inter-changeable manner. Please use either of them consistently throughout the manuscript.

Wherever interchangeable, we have changed "englacial" to "internal" throughout the manuscript. However, we would like to keep the original title of the manuscript.

2. P1L6: add depth ranges of major englacial reflectors associated with dust layers.

We have already added in the revised manuscript making references to the depth ranges in the abstract: Page 1, Line 8 "at least for the upper 30 m"

3. P1L13f: Cite Figure 1 at the beginning part of Introduction (e.g. P1L17). Also, rearrange the figure so that Figure 6b (GeoEye-1 satellite imagery of Kilimanjaro) is presented as part of Figure 1 (see the major structure point #2 above).

We have added citing Figure 1 on Page 2, Line 11. For the reasons stated above (major point #2) we are not rearranging the Figures.

4. P2L8: change "bed conditions" to "bed topography". Conditions sound like that the authors are primarily interested in whether the glacier has the cold bed or wet bed.

We wanted to also point to the fact that little is known about the bed conditions, although we are of course mainly interested in the topography. We have changed this accordingly, now saying on Page 2, Line 9 “bed conditions and topography”.

5. P2L9: remove “total”

Done.

6. P3L9: add “vertical” in front of discontinuities

Done.

7. P4L8: Please clearly mention that there is no/insignificant firn here, because firn affects the radio-wave propagation speed.

Done. Page 4, Line 11-12: “Because of the insignificant amount of firn at NIF,...”

8. P4L27: how much of firn was found in the core? The authors simply said “negligible” but is it possible to shows an approximate fraction of firn and ice in the core?

Judging from Figure S1 of Thompson et al. (2002) and assuming firn was defined by its density, the firn part in the ice core is less than 10 cm deep. It is also worth mentioning that, if firn is defined as snow which has endured an ablation season, became more dense, and was buried by subsequent accumulation, there is none this century at NIF. Snow on the NIF either sublimates, or melts and then either runs off and/or down – or the meltwater refreezes at the surface as superimposed ice, see Hardy (2011) for more details on this.

9. P4L29: the authors interpreted the scattering near the surface exclusively caused by melt water. However, such scattering can occur with other causes, such as off-nadir crevasses or any structural features too (not in the plane of the radar profile).

Based on our experience with the drilling attempts in the field, melt water seems the most likely cause. This is also due to the fact that, with one exception, we did not observe any crevasses, cracks etc.

10. P6L13: typo? “2011.46”? may be 2011.06??

No, this is a decimal date as it is used in the original publication by Cullen et al. (2013).

11. P6L21-24: please revise. What do you mean by “all points”?

We changed “all points” to “all data points” to make this more clear.

12. P7L2-3: cannot fully agree. Figure 1 shows patchy firn distributions (in the picture/image) and the vertical wall is in the blue ice area. The agreement at the wall does not validate the propagation speed and ice thickness measurement at the firn-covered area. Cross-over checks do not validate the propagation speed (as the same speed is used for both frequencies).

1. Please note that the satellite image was recorded at a different date than the GPR survey. More importantly, however, the amount of firn is generally negligible, as argued above. During the GPR survey, the surface conditions at the wall were highly similar to the interior surface (Figure 8, a)). Accordingly, we are convinced that, as compared to other glaciers not being of the tabular structure, the wall does in fact provide a unique opportunity to check ice thickness sounding and have made an attempt to take advantage of this.

2. We mainly used the cross-over checks to demonstrate consistency in bed detection using 100 and 200 MHz. We have clarified this. Page 7, Line 6: “…values for ice thickness are consistent within their uncertainty”

13. P8L22: revise to “with the presence of larger scattering near the surface” (it is not necessarily meltwater)

Considering our reply above considering meltwater being the most likely cause of the near-surface scattering, we have changed the text to: Page 8, Line 24: “…coincide with a large amount of near-surface scattering, presumably due to the presence of near-surface meltwater.”

14. P8L26-28: The current flat surface does not imply the past flat surface (especially in this case where the ice is shrinking rapidly). Variable layer thickness can be caused by strain in the past. Also, ablation can happen from the surface or bottom but not inside of the ice body.

We appreciate the input but are not sure if there is actually a disagreement here. We were not trying to say that ablation happens inside the ice body (hard to imagine how this would work) but in fact, our point is that we believe the observed features are related to ablation as opposed to rheology.

15. P8L29: please present the data. I cannot see any radar data supporting such localized layer convergence in the manuscript. Or do you refer gradual layer thickness change presented in Fig. 4?

We are not referring to the gradual layer thickness change but mean actual convergence of two layers into one layer, which can only be observed close to the crater rim. As requested we are now showing the respective data in our supplementary Figure (Profile D). No layer convergence is seen towards the ice cliff or in the interior.

16. Table 1: are samples for 200 MHz CMP measurements correct? Figure 3 looks like that there are more samples than 5.5 nsec/sample (= 100 nsec/18 samples). If it is not a typo and the sampling rate is so low, the data are not fully useful to determine the radio-wave propagation speed. Also, clarify "samples"; I understand that it is the number of samples within a time window (vertical range). Is it correct?

In case of the CMP, the number of samples refers to the number of shots of the CMP, e.g. the number of times the antennas were repositioned. Thank you for pointing this out, we have clarified this in the Table caption.

17. Table 3: does "relative depth" show the depth relative to the local ice thickness? Please clarify. And why are relative depths (in addition to the absolute depths) important for this context?

Yes, relative depth means relative to local ice thickness (which is always at 100%). This change was made in the revised manuscript specifically to meet a reviewer's comment, suggesting this for aiding the comparison of IRH depths.

18. Figure 1: fill the area of tabular cliff with half-transparent color (or hatch). It is not easy to find out tabular cliff areas only using the outlines currently presented in this figure.

We do not believe the reader would benefit from adding any more detail to Figure 1. As said earlier, the main purpose of Figure 1 is to show the locations of the GPR profiles. However, we made an attempt to address this comment by adding to Figure 8 more pictures that clearly show the cliff locations on NIF.

19. Figure 1: is it possible to add surface elevation contours to Figure 1? "the central flat area" is mentioned in Sections 1 and 2, but data supporting these sentences appear only in Figure 6. In general, the surface topography (and tabular cliffs) should be explained early in the manuscript, probably using a single paragraph in Section 1 (between ".... Kilimanjaro's glaciers to climate variability." and "This especially ....:" (P2L10). Also, include the AWS location in Figure 1 (it is referred several times in the text but its location is not shown).

See the comment made above regarding visibility of the GPR profiles, we believe it is better to leave out contour lines in Figure 1. However, we have added the position of the AWS to Figure 1 b) and also Figure 5. We have also added text to the introduction explaining the surface topography earlier in the text: Page 3, Line 1-2: "Typical for the tabular glaciers on Kilimanjaro's summit (cf. slope glaciers) the NIF topography is characterized by a central flat plateau area and near-vertical ice margins (Kaser et al., 2004; Cullen et al., 2006; Hardy, 2011)."

20. Figure 4: The two core sites NIF2 and NIF3 are shown at the end of the profile. Please include radar data beyond these points so that radar data in the both sides of the core sites are presented.

We have included this request in the new supplementary Figure showing all 200 MHz profiles. We have indicated the positions of NIF2, NIF3 and the intersection, analog to what is shown in Figure 4.

21. Fig. 5's caption line 4: change "thick horizontal blue lines" to "thick horizontal blue markers", "bars" or such (confusing with the blue curves in the lower panel).

Done.