

Interactive comment on “Recent accumulation rates of an alpine glacier derived from firn cores and repeated helicopter-borne GPR” by L. Sold et al.

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This manuscript (MS) reports of snow surveys by GPR from a helicopter effectuated on a glacier in Switzerland over two subsequent years, and the analysis of the obtained data to unlock the information contained in layered material.

The main contribution of this study is the methodology to exploit the sequence of reflectors recorded by the GPR with regard to a chronology of annual accumulation. The method accounts for the compaction of firn due to gravitational settling and refreezing of meltwater. Using the simulated density enables calculating the propagation velocity of EM waves through the substrate and hence establishing a depth-traveltime relation-

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ship. IRHs are then attributed to summer surfaces to derive a multi-year accumulation history.

However, regarding this analysis, I have a major concern. The authors use detailed records from several firn cores to evaluate their results to find that the fundamental assumption of IRH = summer surface is not valid. IRHs occur at planes of sharp contrast of the di-electrical properties of the material, which can be caused by density/ permittivity contrasts but also by contrasts in electrical conductivity as caused for instance by dust layers. Therefore, the interpretation IRH = summer surface is ambiguous. Using the firn core records, the authors are able to avoid erroneous identification and their derived accumulation rates are therefore credible. However, in the conclusions, they state that the “approach is independent from external information such as firn cores”, which is a too strong statement since the credibility of the results critically depends on the firn core record, needed to resolve potential ambiguity. It is therefore questionable whether the proposed method is reliably applicable to other glaciers where such firn core data is not available.

The statement was removed from the conclusions.

In the revised manuscript we extend the discussion of a verification of the layer dating by comparison with inter-/extrapolated (modelled) mass balances (P4447, L3ff). We thus show, how our approach could be transferred to a glacier where no firn cores are available.

Furthermore, the firn compaction model seems to be affected by a mistake becoming apparent in eq 2, where the mass-balance rate is multiplied by the density of ice (and subsequently in eq 4). The reason for doing so is not clear. In the original formulation by Herron and Langway (1980) the mass-balance rate is in water equivalents, whereas Reeh (2008) used the mass balance in ice equivalents and introduced the ratio $\rho_{\text{ice}}/\rho_{\text{water}}$ to convert to water equivalents. Huss (2013) used the same model

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but made a similar mistake by multiplying the w.e. mass balance with the density ratio. Ultimately, this mistake will be accounted for by the calibrated value of f such that the final results most likely are not affected. Here, the calibrated value of f is much larger than the original value used by Herron and Langway (1980) and Reeh(2008), a fact that is not mentioned in the MS, but definitely needs to be discussed! Nevertheless, this presentation of the model (which is the backbone of the entire study) is highly confusing and needs to be clarified. Also, the units of the involved empirical parameters have to be specified.

We corrected Eq. 2 in the MS and in the model.

We now provide the necessary units in section 3.2.

We agree that the difference to previously published values of f should be discussed. This is incorporated in the revised manuscript.

Chapter 3.2 was revised and rewritten in order to clarify the modelling approach.

The stated aim of assessing spatial distribution is worthwhile but it barely addressed in the entire MS. The authors state the glacier was surveyed along 79km profiles in a regular grid covering the area 500m but the data presented here is only from a few 100m. I doubt that the surveys produced only so few repeat points, even if the grid navigation were maximally off.

Indeed only a very small part of the recorded GPR profiles could be used for the analysis. This is (1) due to the general limitation to the firn area (>50% of the GPR profiles for the monitoring of winter accumulation are in the ablation area), (2) because reflectors cannot be tracked over longer distances (as stated in the introduction), and (3) because the approach is based on layer counting, i.e. the observer must ensure that all annual layers exist at the analysed locations. Due to the latter, the approach is limited to areas with sufficient amounts of annual accumulation and, thus, to the upper

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accumulation area.

We add a discussion on this issue in the revised manuscript.

The other aim indicated in the title is to analyze recent accumulation rates, which is not at all covered here. So the title is misleading. In addition, since the analysis is based on a reduced dataset, the helicopter-borne aspect of the data is not relevant for the MS; the limited dataset presented here could have easily been achieved by ground-based GPR.

The title of the paper was changed to better match the manuscript ("Unlocking annual firn layer water equivalents from GPR data on an alpine glacier"). We agree that the helicopter-borne data acquisition is not fundamental for the presented analysis. However, we believe that even the reduced dataset could not have been obtained by ground-based GPR in the high alpine terrain of the study site.

I recommend reformulating the title to better reflect the content of the MS which also should be revised to appear more streamlined. In its current form the overall objectives appear splattered and need to be more focused. Do the authors want to address recent accumulation? Spatial distribution of snow from high-degree coverage by helicopter borne GPR or is it to unlock the layer information? From the MS the latter stands out as the primary objective and this needs to be clearly defined in the MS and reflected in the title.

The introduction is revised and rewritten to better define the objectives of this study and to better match the contents of the manuscript.

In my view, although based on an interesting idea, the MS does not live up to the expectation raised in the introduction. The MS needs major revisions to a) focus on a

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clearly defined objective and b) not to oversell their findings but honestly discuss the associated shortcomings and c) to improve readability and precision of the text by an extensive proof reading (English native speaker?). Further examples supporting this point are found in the list of detailed comments below.

Detailed comments:

1. P4432, Abstract, L13: *the SI units for density should be used, not only here but throughout the MS*

Changed throughout the MS.

2. L13/14: *"ACCORDING TO MODEL RESULTS, refreezing accounts..", ""* Changed as suggested

3. L16: *"in the same order AS.."*

Changed as suggested

4. P4433, L11: *"the low ELECTRICAL conductivity.." to avoid confusion with thermal conductivity*

Changed as suggested

5. P4434, L14: *"...to convert the GPR traveltime to depth...an estimate..of the propagation velocity is required. This velocity depends on density of the material, the latter also needs to be estimated or measured. This procedure introduces..."*

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Changed to "Additionally, to convert the GPR traveltime to depth, an estimate of the propagation velocity is required. Because the velocity depends on the density of the material, the latter needs to be measured or estimated (Plewes and Hubbard, 2001). Thus, density introduces an uncertainty, as it also does for conventional accumulation measurements that are based on the determination of layer thickness."

6. P4435, L1: *"the bulk density of firn layers" is very clumsy wording and confuses the reader. What has been estimated here, the bulk density of the entire firn volume/column or the density of layers? From my understanding, the latter applies here and the term "bulk" should be avoided when referring to the vertical profile. This wording appears several times throughout the MS.*

Removed "bulk" at P4435, L1, P4448, L9, P4449, L4.

7. L2: *"...where GPR intersect in subsequent years" a bit unclear, do you mean "where repeat surveys from subsequent years exist"? Anyhow, it is surprising how little repeat points have been produced (12) given the stated density of the GPR profiling in grids of 500m spacing. Obviously, the data have been filtered according to some criteria which need to be clearly stated. The statement made in the last paragraph of sec 1 is one of my major problems here: you need the information of the firn core to unambiguously associate IRHs with summer surfaces but then you claim that your method is only based on GPR and the firn densification model.*

"GPR intersect" was changed to "repeat measurements exist" throughout the manuscript.

A discussion on the small number of calibration points and the reduced dataset is added to the revised manuscript (see general comments).

In the revised manuscript we extend the discussion of the verification of the layer dating

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and the necessity of firn cores (P4447, L3ff).

8. L11/12: *"accumulation characteristics are strongly determined by the synoptic weather patterns" this is trivial and can be omitted*

Removed statement as suggested.

9. P4436, L1: *"...were taken AT Findelengletscher.."*

Changed as suggested.

10. 1st par: *here you state that both surveys were conducted along a regular grid of 500m spacing, covering the entire glacier. This must produce more than just 12 cross-over points? if the dataset was reduced, the filter criteria need to be stated. The stated coverage and density of the surveys are interesting but since >90% of the data are neither presented nor analyzed, this information appears obsolete.*

See general comments and comment (7). Also note that a large part of the dataset actually covers the ablation area and obviously cannot provide annual accumulation layers. This is now clarified in the manuscript.

11. L5: *"With a flying speed...measurements were taken from 5-10 m above the surface at time intervals of 0.02 s corresponding to a trace spacing of approximately 0.2 m."*

Changed as suggested.

12. L5/6: *"...the position obtained from a differential global positioning system (DGPS), a time window..."*

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Changed as suggested.

13. P4437, L20: *"An estimate for..."*

Changed as suggested.

14. L26: *"...the approach to MODEL firn compaction..."*

Changed to "We used the approach by Reeh (2008) to model firn layer compaction. It is based on the..."

15. P4438: L5: *state the unit of the parameter c is it consistent in eq 1 and eq2?*

Added "dimensionless". Because Eq. 2 was corrected according to the general comments the units of parameter c are now consistent in Eqs. 1 and 2.

16. L10: *What are the units of k and f?*

We now provide the units of temperature, mass balance, activation energy, gas constant, and f on P4438.

17. Eqs 2 and 3 are for $\rho_f \geq 550 \text{ kg m}^{-3}$, what happens with $\rho_f < 550$??

This was described in P4439, L11. The sentence is moved to P4438, L8 and slightly modified: "Here, we neglected the lower stage ($\rho_f < 550 \text{ kg m}^{-3}$), because the model will be calibrated and measured autumn snow densities were not considerably lower (see below)."

P4439, L14 removed redundant sentence.

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18. Eq2 is not identical to the similar equation used by Reeh, 2008. The b used by Reeh is in m ice equivalent and the ratio ρ_i/ρ_w is used to convert it to water equivalent. I assume your mass balance values are in $w.e.$ and do not need conversion, anyhow just using a factor ρ_i instead of the ratio would be wrong by a factor 1000!

Corrected (see general comments).

19. L12: "...is an empirically.." (check spelling)

Corrected.

20. L 21: "the traveltime-thickness" is very awkward, merging two fundamentally different quantities into one expression. This needs to be fixed also at the many other instances in the MS.

Changed to "IRH traveltimes", P4438, L21, P4442, L3, L6, P4445, L4, P4449, L11, P4458, Fig. 3.

21. P4439: L 6: "water equivalent was then derived from..."

Chapter 3.2 (Modelling firn density) was revised to clarify the modelling approach, following the general comments.

22. L14: $c_{(i+1)}$ cannot be the compaction rate, alternatively the variable has changed meaning since its usage in Eq 1. Please clarify.

Changed to "proportionality factor" in P4439, L9, L14, removed "change rate" in P4442, L7, removed "c" in P4447, L15.

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23. Eq4: same comment as for eq2, what is the role of multiplying mass balance with ρ_i ?

Corrected (see above).

24. P4440, most of the material in the paragraph before 3.3 seems to be discussion material.

Section 3.2 was revised and does not contain these statements anymore. The statements are incorporated in the discussion section.

25. L13 ff: details of the "conservative uncertainty estimate" should be specified.

Changed to "We obtained a conservative uncertainty estimate of $\pm 61 \text{ kg m}^{-3}$ from the mean standard deviations of (1) multiple density measurements within the same snowpits, i.e. the measurement error ($\pm 21 \text{ kg m}^{-3}$), (2) within single years, i.e. the spatial variability ($\pm 31 \text{ kg m}^{-3}$) and (3) at locations with annual repeat measurements, i.e. the temporal variability ($\pm 48 \text{ kg m}^{-3}$)."

26. P4441, L5: "negative temperatures" change to "subfreezing temperature"

Changed as suggested.

27. L11/12: "the amount of refrozen meltwater" it is unclear how this amount was derived or estimated. Please explain.

Added explanation: "For each year, the amount of refrozen meltwater was calculated from the specific heat capacity of snow and the latent heat of fusion and was added to the layer water equivalent. The given layer thickness then allowed updating its mod-

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elled density.”

28. P4442: L1: “At locations where GPR repeat measurements are available...”

Changed as suggested throughout the manuscript (P4432, L8, P4435, L2, P4442, L1, P4449, L11, P4458 (caption Fig. 3)).

29. L7: “the optimal scaling factor”, optimal in which sense? Also the entire sentence is unclear and needs rewording.

Changed sentence to “The model was calibrated with a scaling factor of $f=2900$ (Eq. 2) that was found by minimising the root-mean-square deviation of the modelled and measured IRH traveltimes of 35 layers at 12 locations (Fig. 1).”

30. L 15: “the outer part” of the core?

Changed as suggested.

31. P4443: L1 ff: if the cores cover the period from summer 2008 - 2012, how can the dust layer deposited in May 2008 be found?

Here, “summer 2008” referred to the lowermost pronounced high-density layer. Changed to “winter 2007 / 08”.

32. Sec 4: the results are presented in a different order than the associated methods have been described. The structure of the MS would benefit from keeping the same sequence.

In the methods section the firn core analysis was moved to the beginning in order to

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follow same sequence as the results section.

33. P4444: L10-21: this is discussion material

The chapters were changed as suggested by referee #2: 3. Results and discussion (formerly “Results”), 3.4 Data interpretation and error analysis (formerly “Discussion”). We believe that the structure of the manuscript benefits from keeping this discussion fragment next to the respective results.

34. P4445, L2: “the model was applied to each GPR trace individually” this must be an excessive computation, given the stated trace spacing of 0.2m and the entire profile length of 79 km. clarify!

Changed to: “As described above, the firn densification model was applied to all GPR traces individually where multiple IRH were found that correspond to subsequent previous summer surfaces. Layer water equivalents were then derived from the modelled densities, the IRH traveltimes and the density-based GPR wave velocity estimates.”

Added “where multiple IRH were found” in P4449, L13 to make clear that the model is not applied to all individual traces.

35. L24ff: Refreezing: can you specify how much of the refreezing occurs within the annual layer and how much below that (=internal accumulation)?

We provide more detailed results on how refreezing affects individual layers in the revised manuscript.

36. L29ff: you claim that the model uncertainty is only slightly larger than that of in situ measurements. This is a strong statement but cannot be judged by the reader as one

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quantity is presented in relative and the other in absolute values.

Rewritten and toned down: "For conventional glaciological accumulation measurements the density measurement error is approx. 4% ($\pm 21 \text{ kg m}^{-3}$ from repeat measurements). In contrast, the combination of GPR with a firn density model provides a considerable spatial coverage for four annual accumulation layers with a small trade-off in terms of uncertainty."

37. P4446, L26: *"verification" change to "evaluation"*.

Changed as suggested.

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