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

Anonymous Referee #1

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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 relationship. 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.

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_i/rho_w to convert to water equivalents. Huss (2013) used the same model 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.

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.

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.

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.

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 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:

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

L13/14: “ACCORDING TO MODEL RESULTS, refreezing accounts..”, L16: “in the same order AS..”

P4433, L11: “the low ELECTRICAL conductivity..” to avoid confusion with thermal conductivity

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...”

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.

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.

L11/12: “accumulation characteristics are strongly determined by the synoptic weather patterns” this is trivial and can be omitted

P4436, L1: “...were taken AT Findelengletscher..”

1rst 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.

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.”

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

P4437, L20: “An estimate for...”

L26: “...the approach to MODEL firn compaction...”

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

L10: What are the units of k and f?

Eqs 2 and 3 are for rho_f>=550 kg m-3, what happens with rho_f < 550 ??

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 rho_i/rho_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 rho_i instead of the ratio would be wrong by a factor 1000!

L12: “...is an empirically..” (check spelling)

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.

P4439: L 6: “water equivalent was then derived from...”

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

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

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

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

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

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

P4442: L1: "At locations where GPR repeat measurements are available..."

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

L 15: "the outer part" of the core?

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

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.

P4444: L10-21: this is discussion material

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!

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

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

P4446, L26: “verification” change to “evaluation”.

P4447, L10 “unstable verification” what do you mean by that?

L15: “...is not exceptionally stable...” do you refer to numerical stability or robustness of the results?

L27/28: “For the following...” I do not understand this sentence.

P4448: L3/4: “the particular weather conditions in general”. . .clumsy wording. Is it “in particular” or “in general”? cannot be both at the same time.

L15: how can “external refreezing” occur within a layer? What is meant by this?

L23: unclear what “temporal breaks” refers to

P4449: L7/8: again. “bulk density” or “density of each layer”?

L9/10: “refreezing under temperate conditions” sounds mysterious, reword!

L12/13: “our approach is independent from external information such as ice cores” but actually you need the firn cores to unambiguously relate IRHs to annual layers. So this statement is simply wrong!

Fig 1: I expected to see the grid of the GPR surveys, but it is probably not of relevance for this study and presumably therefore not shown on the map. Consider removing the corresponding part of the text.

The insert map does not aid locating the study region.

Interactive comment on The Cryosphere Discuss., 8, 4431, 2014.