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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.

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The manuscript by Sold et al. submitted to the Cryosphere presents an approach to inversely relate radar signal reflections in an accumulation area of a glacier in the Swiss Alps to accumulation rates. To convert measured TWT values to SWE, they use a snow/ firn densification model together with a rather simple approach to account for meltwater redistribution. For validation and layer relation to summer surfaces, they use results from 2 firn cores drilled during the first radar campaign in 2012. While data on annual winter accumulation rates (point measurements) is available for this glacier, it is not presented in the manuscript. A comparison of these point measure-

ments (and spatial extrapolation thereof) with results for the most recent accumulation derived from GPR data could support the main conclusion from this manuscript that radar accumulation estimates have a much better spatial representation to assess the actual accumulation rates on glaciers. However, only a very small part of the recorded radar transects is discussed and presented in this manuscript.

Indeed, only a small part of the recorded GPR profiles could be used to extract firn layer water equivalents. Obviously, the approach is limited to the firn area (<50% of the total glacier area). Because reflectors cannot be tracked over longer distances, the analysed profile sections are disjoint. Furthermore, the approach is based on layer counting and, thus, 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 accumulation area. A discussion on that is provided in the revised manuscript.

For our study site several years of winter accumulation measurements are available from GPR and conventional snow probings combined with density measurements in snow pit. However, the presented approach does not involve the computation of melt rates and, thus, a comparison of winter accumulation measurements with annual accumulation rates is not meaningful. On the other hand, a comparison with annual measurements of accumulation are is shown in Fig. 5 and will be discussed in further detail in the revised manuscript.

In terms of language, the manuscript is well written, however, a couple paragraphs should be thoroughly revised (see below). Shorter and less nested sentences facilitate following your statements.

We avoided nested and long sentences in the revised manuscript in order to improve the readability.

The major point I come up with is that the presence of liquid water is disregarded in the manuscript even though it has a very strong impact on radar wave velocity even for very small volume fractions. I can see that this is beyond the scope of this paper but certainly needs to be discussed as you deal several times with it. I will explain my concerns more in detail: To convert TWT in depth, you use Frolov and Macheret (1999) to determine layer respective bulk propagation velocities. Here, you use only a 2 phase mixing formula empirically determined for dry snow/ firn conditions. The firn temperature however is set to be constant at 0C. Additionally, you define that a cold content transmitted from the surface into deeper parts is compensated by meltwater refreezing. Actually, to refreeze liquid water you need to have temps below 0C to compensate for the release of latent heat. For your assumption of isothermal conditions within the firn pack, you must assume liquid water being present. At the same time, you use equations for the conversion of EM wave velocity to density which are only valid for dry conditions. These 2 assumptions are contradictory and it means that you expect the firn always being at a certain state where all liquid water is already refrozen and the cold content of the overlying snowpack/ firn layers hasn't yet reached the layer you are observing. In my opinion these assumptions have to be discussed more in detail. This point is very crucial for your assumptions since even small portions of remaining liquid water alter the wave speed significantly (e.g. Schmid et al., 2014).

We agree that the effect of liquid water needs to be discussed in the context of this study and that the current formulation is inconsistent. The major problem is that no measurements of the liquid water fraction are available for our study site. Furthermore, liquid water content is expected to vary not only vertically and laterally in the firn, but also temporarily. The latter has a potential impact on the calibration scheme of the model. However, at the two firn core locations the dry-snow assumption still yields a good agreement between IRH traveltimes and ice layer depths – but this cannot be generalized for the entire accumulation area. Instead of an explicit solution for this issue we will provide a discussion and an estimate for the related uncertainty in the revised manuscript, using an adequate mixing formula to derive the GPR wave velocity.

In section 3.3 you state that no melting occurs in the accumulation area during the winter season. This statement is somehow useless unless you define winter season. And for several accumulation areas at Alpine glaciers you will find massive melt freeze crusts within the snowpack in late April early May. On page 4444 L.13-18 you describe weather conditions which usually produce melt-freeze layers at the snow surface.

The statement that "...no melting occurs in the accumulation area of Findelengletscher over the winter season" was removed from P4440, L26. However, from the field measurements in mid- to end-April (snow pits) we know that melt events are rare during winter. Instead of applying a constant offset of -4.9K we now use mean monthly offsets (e.g., -3.07K in Oct., -6.71K in Dec., -3.54K in Apr.) in order to estimate the snow surface temperature based on the measured air temperature (see comment 16). Thus, the effect of surface melt on surface temperature is now implicitly taken into account.

We revised the beginning of of section 3.3 to clarify the assumptions made to obtain the refreezing (P4440, L24): "Due to cooling from the surface during winter, firn can periodically have temperatures below 0degC even on temperate glaciers. The relocation and refreezing of meltwater from the surface alters the water equivalent of firn layers and affects their density. Due to the temperate conditions, we could assume that each summer all subfreezing temperatures are compensated by the refreezing of meltwater. Thus, the amount of refreezing is given by the firn temperature profile that is generated by the surface cooling during winter. We obtain a one-dimensional end-of-winter firn temperature profile at each GPR measurement location using heat conduction from the surface in the period from 1 October to 1 May."

Removed sentence accordingly from P4441, L4.

The 3rd point dealing with liquid water I am concerned with is that you do not account for lateral flow, mass loss through melt and percolation of liquid water from surface layers into previous accumulation layers. You just present a 9% density increase to *"affected" layers. The whole Section 3.4 needs to be revised and clarified, which layers are affected by when and at which date! Additionally, I would expect that mass loss is discussed. This manuscript presents a similar approach than the one from van Pelt et al., 2014 here for an Alpine glacier instead of polar/ subpolar glaciers. Please discuss differences.*

We agree that lateral flow of liquid water is a relevant process. However, it cannot be assessed directly here and, in our opinion, is beyond the scope of this paper.

We model the refreezing of water that originates from the melting of the winter snow pack throughout the summer. Based on the temperate nature of Findelengletscher we assume that all end-of-winter sub-freezing temperatures in the snow and firn pack are compensated by refreezing during summer. This has been supported by shallow firn temperature measurements in fall that indicated fully temperate conditions. The quantity of snow melt in the accumulation area during the summer months (known from direct mass balance observations) is generally >1m water equivalent per year. This amount of meltwater would be sufficient to completely heat up a 45m thick firn column (at 700kg m^{-3}) with an average temperature of -5 deg C to the melting point. On Findelengletscher the typical thickness of the total firn layer is about 10-20m and winter cooling penetrates to a depth of about 10m. This clearly indicates that our assumptions and our simplified approach are justified. In contrast to Arctic glaciers the limiting factor for refreezing is the firn temperature and not the amount of liquid water.

In the revised manuscript we discuss these rough considerations based on direct field observations and compare our approach used for temperate firn to cold firn. Furthermore, we provide more details on the effect of refreezing on layer densities and water equivalents.

The approach of van Pelt et al. (2014) is different to the method presented here. We use measured IRH traveltimes as input to a model to obtain firn layer densities. Then, water equivalents were derived from the IRH traveltimes (thus, it can be considered a “forward” approach). Van Pelt et al. use the IRH traveltimes to iteratively calibrate a mass balance model and inversely derive accumulation rates. We provide a discussion on the differences in the revised manuscript.

Some other major points that must be addressed:

A) the structure of the presented manuscript is not appropriate. There is no Result-Section. Eg Section 4.1 involves a large discussion part of the results of the chemical analysis of the firn core. I recommend changing the whole Section 4 to Results and Discussion and name 4.3 Data interpretation and error analysis instead of Discussion.

The sections were changed accordingly. To further improve the structure of the manuscript, the fragments on P4439, L24 and P4440, L5 were moved to the discussion section.

B) Neither you do present a number on the ice velocities of this glacier nor any reference dealing with this ("slow" is not appropriate here). However, you compare exactly ("intersections of radar transects") the same locations at the surface of 2 consecutive years. This is only possible when the ice velocity is 0m p.a. You need to discuss this, since your work is based on an Alpine glacier with a significant topography (Fig. 1).

We agree that this is a critical point and, actually, mean IRH traveltimes within a 25m radius were used in order to reduce the effect of ice flow – but also of the positioning imprecision and GPR footprint size variations.

Added at P4442, L2: “To reduce the effects of ice flow, a potential imprecision of the positioning and differences in the GPR footprint size, the model was applied using the mean IRH traveltimes within a distance of 25m from the common location.

We now provide information on measured ice velocities at the two accumulation measurement locations on P4447, L13: “The firn densification model was calibrated with the observed changes in IRH traveltime from spring 2012 to 2013 at 12 locations and for 35 firn layers. Thereby, the change in IRH traveltime at individual locations is affected by horizontal ice motion. At the two accumulation measurement sites (Fig. 1), flow velocities are about 35m yr^{-1} (northeastern site) and 15m yr^{-1} (southwestern site). For the upper accumulation area measurements are not available. However, due to a smaller ice thickness, velocities at the model calibration locations are expected to be considerably lower and within the 25m radius that was used to obtain the mean IRH traveltimes.”

C) Concerning the topography, for steep reliefs, your radar data processing scheme is not adequate. See http://www.sandmeier-geo.de/Reflex/refl2da.htm for parts where you do make significant errors for airborne radar data analysis if you lack a proper topography migration/ correction. It is impossible for the reader to identify in Fig. 1 with 100m contour lines whether such a correction is necessary or not. A minor point concerning the processing routine but nevertheless essential for the presentation of good radar data is a proper surface correction. In Fig 2 bouncing surface signals are recognizable while zooming in. This can easily be corrected in ReflexW! Applying such a static correction enables further processing to remove noise and enhance continuous reflectors (running average filters etc.)

The terrain at the analysed GPR profile sections has a mean slope of 10deg (15deg 95% percentile, 19deg maximum, extracted from 10m DEM). Careful migration of the data using the reflex software was performed, however, we did not find any clear improvement of the GPR data quality by means of migration.

Added sentence on P4436, L25: “Migration was found to not improve the data quality for the given survey design and the moderate surface slopes at the analysed GPR measurement locations (95% less than 15deg).”

We provide a improved Fig. 2 with a new surface correction for several GPR traces. However, we are not entirely sure if this is what Referee #1 referred to by "bouncing surface signals".

Minor points that should be addressed:

1. the title indicates that this paper addresses mostly accumulation rates. Please modify to present the major part of this work which is the methodology.

The title was changed to "Unlocking annual firn layer water equivalents from GPR data on an alpine glacier".

2. please use SI units and indicate for whenever values have to be converted to fit models

Changed to SI units throughout the manuscript. Also stated units of parameters as suggested by Referee #1.

3. P4432 L3 this is not completely "new" - see van Pelt et al., 2014

Removed "new".

4. L10 IRH correspond to density max and/or liquid water occurrences. Changes in liquid water in snow can dominate any density gradients - see Schmid et al., 2014.

We agree that liquid water can generate strong IRH. However, the given sentence describes our findings from the firn cores, where IRH corresponded to density maxima. The fact that GPR detects changes in dielectric permittivity such as from density, but also water content and impurities, is stated in the Introduction (P4433, L14).

5. P4433 L24-26 this sentence is hardly understandable please rephrase!

Rewritten: “Past accumulation rates are typically estimated from a set of pronounced IRH that do not correspond to annual layers. Based on a given a depth–age relation and the density, an average mass balance is obtained for the period that is covered by each pair of IRH.”

6. P4434 L8 comment while taking melt into account for temperate glaciers. I don't think you can fully neglect residual liquid water in the firn pack. See major point above.

Discussed in the general comments and taken into account.

7. L20-27 I am missing the point here

Revised paragraph: “Studies that analyse IRH in firn along GPR profiles are typically complemented by cores to provide density, layer dating, and potentially, dielectric characteristics (Pälli et al., 2002; Arcone et al., 2004; Eisen et al., 2006). However, firn cores are expensive in terms of cost and effort and provide point measurements only. Physical models can be used to obtain firn density profiles along GPR profiles. They require various input data such as temperature, precipitation, wind, and terrain to obtain the accumulation rate (Ligtenberg et al., 2011).”

Changed beginning of the next paragraph accordingly: “In this study we use IRH traveltimes measured by 400MHz helicopter-borne GPR measurements on Findelengletscher, a temperate alpine valley glacier in Switzerland, as input to a simple transient model for the density of firn layers and the refreezing of meltwater. The model does not require precipitation data and is calibrated at locations where GPR repeat measure-

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ments are available in subsequent years."

8. P4435 L22 indicate in Fig 1 where the AWS are located or give distances in the manuscript.

Changed to "Meteorological data is available from automatic weather stations at Zermatt (1638 m a.s.l. , 6km west of the glacier terminus) and Stockhorn (3421 m a.s.l., 2.5 km south of the glacier terminus)."

9. P4437 L11-18 again, the glacier has to be stationary for this kind of analysis, you need to comment on this!

See general comment (B).

10. L23 (e.g. Kovacs...)

Added "e.g."

11. P4439 L5 you do have sufficient data to prove this is an appropriate assumption, right now it is just a number

Changed to "We set its density to rho_f,0 = 400kgm-3 , which is in line with observations in spring 2010–2013 (395 +-32kgm-3 from 14 measurements > 3200ma.s.l.)."

12. L5ff this is kind of too fast here. Please present equations and detailed steps how you convert TWT to accumulation in w.e.

Section 3.2 was revised to clarify the modelling approach and we provide an additional equation for the conversion from traveltime to water equivalent.

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13. L14 what is "considerably lower" quantify!

Changed to “. . . were not considerably lower (490 kg m^{-3}, see below)” on P4438, L8.

14. P4440 L3 this is confusing I think you want to rephrase this sentence

Section 3.2 was revised and does not contain this text fragment anymore.

15. L28ff next page; again the reader would benefit from a more detailed description and presentation of equations which you use e.g. what is the characteristic length scale, time scale etc?

The text fragment was revised (P4440, L27ff) and now contains a more detailed description of the firn temperature estimation scheme: “A vertical density profile with a resolution of 0.1 m was set up, consisting of the winter snow cover and the underlying firn layers. The winter snow cover is built up linearly between 1 October and 1 May to reach the thickness estimated from the IRH traveltime. The density was assumed to increase linearly from 100 to 400 kgm-3. For the firn section the respective modelled end-of-winter density profile was used. The thermal diffusivity of snow and firn was estimated from an empirical relationship with density (Calonne et al., 2011). The heat equation could then be solved numerically in hourly time-steps with a forward in time, central in space scheme.”

We believe that the manuscript would not benefit from the presentation of the heat equation or its FTCS approximation.

16. P4441 L10 any citation that can support your assumption that T_ss=T_A-4.9 is always valid. I doubt this especially for melt conditions.

We now use mean monthly offsets between air and surface temperature observed at the nearby high-altitude weather station in order to take seasonal variations into account.

Changed to “In order to account for differences between the air and snow surface temperatures we applied a mean temperature offset for each month. The offset was obtained from the observed monthly mean difference in 2002–2010 at the Stockhorn weather station that was estimated from the outgoing longwave radiation. The offset is largest in December (-6.71K) and decreases to -3.54K in April.”

17. L13ff please rephrase to enhance readability

Revised: “However, the two models for temperature and density were not fully coupled because they run on different domains. The densification model primarily steps through the layers from top to bottom, whereas the heat conduction is solved over time.”

18. P4449 L16ff well you could present a plausibility check to prove that it is impossible that this layer does represent a former summer horizon. If you feed your model with this IRH what is the accumulation output. Is it a reasonable value and corresponds more or less with manual measurements? This could be performed almost everywhere, where an AWS can be used to relate radar derived accumulation with precip measurements. I think presenting such a plausibility check may allow you to present the statement in the following lines. Otherwise, you do need firn cores and complex data analysis to relate IRH to summer surfaces. And this is not an efficient data acquisition!

In the revised manuscript we extend the discussion of a verification of the layer dating by providing a comparison with measured mass balance with and without the false IRH included in the analysis. Thus, we now show how our approach could be applied if no firn cores were available.

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