

We would like to thank the reviewer for the feedback and the useful comments.

Please find below our replies as inserted blue text.

Kind regards,

Nena Griessinger, Franziska Mohr and Tobias Jonas

The paper by Griessinger et al presents spatially-distributed measurements along transects (of typically 100 m in length) of both snow depth and snow water equivalent (SWE), and their variation over time, considering periods from a few days to a couple of weeks. Those measurements were done with the help of a new mobile ground penetrating radar (GPR) device. The paper starts with a brief introduction on GPR technique overall and its application to snow hydrology. Section 2 describes the methods used by the authors: the two study areas considered, the measurements with the mobile GPR device (including the manual measurements for calibration and validation) and data post processing. Section 3 presents the results on snow depth and water equivalent measurements, and those results are discussed in section 4. Finally, section 5 concludes the paper.

General comments

It seems to me that the design of the mobile GPR device and its performance shown in the present paper are of interest for the snow hydrological research community. As such, the topic addressed in the paper could be worthy of publication to The Cryosphere journal. However, I believe that the manuscript in its current form is far from being suitable for publication because it suffers from a lack of detailed information on a number of crucial points regarding the methods, and there are some confusing parts additionally. I found the paper much too short. I have tried to summarize my concerns in the list of the specific comments provided below. I strongly encourage the authors to revise and extend a number of parts of their paper in order to make it self-contained, clearer and more convincing.

Specific comments

Abstract:

1) line 7: "over time" is seemingly too vague... please mention the typical time scales (from a few days to a couple of weeks) which you considered in your field study. Also, it would be nice to add in the abstract an information on the typical length of the profiles considered.

We will adapt the abstract accordingly.

Section 1:

2) page 2, lines 20-23: I found the discussion on the techniques used to measure snow properties, in particular density, and SWE, much too short. Please could you extend it and explain how density and SWE are measured in your study?

Thank you, we will extend the introduction. We will provide more information on our methods in Section 2 in order to keep the structure of the manuscript clear.

Section 2.2:

3) page 3, line 15: please could you include example of radargrams and illustrate how your reference points are efficient for synchronization?

Thank you, we will include a radargram. The reference points are fix positions marked in the field and recorded in the radargram. The positioning of traces within radargrams of repeated overpasses was forced to exactly match at these points. This way the positioning accuracy in between those points and consequently along the entire transect can be limited to below 5 centimeters. We will add this information in Section 2.3.

4) page 3, line 16: while mentioning the manual measurements here, you could refer to section 2.2.2. Otherwise the reader may expect those manual measurements to be detailed here.

Thank you, we will refer to Section 2.2.2.

5) page 3, line 20: this appears to be a limitation of your mobile GPR device. Would you have any suggestion on a way of overcoming this in the future? Could you please make a comment (maybe you could include this point in conclusion too). Furthermore, I wonder how this "temporary" crust is thick and may affect the measurements, particularly concerning the assumptions made for estimates of density and SWE.

Thank you for this comment. The mobile GPR device itself could also be used on any snow surface. However repeated surveys may only make sense if the snowpack remains as undisturbed as possible to allow meaningful differential measurements. Hence our strategy was to limit measurements to mornings after a clear night.

Section 2.2.2:

6) page 4, lines 22-23: please could you show this in a table (or plot)? What was the typical standard deviation? Standard deviation (SD) between repeated LWC measurements within transects were 0.48 % VWC (mean SD), 0.75 % VWC (max SD).

Section 2.3:

7) page 4: the section is much too short. I invite you to go into much more detail on your method for data post-processing. In particular, I strongly suggest you to include radargrams corresponding to each of the main steps.

We agree and will extend this section.

8) page 5, lines 2-10: this part needs strong revision. I understand here that you need your manual measurements of snow depth, density and SWE for calibration/optimization. Please explain better the optimization procedure. Could you show for instance a cross-comparison between the results with a direct wave velocity of 0.3 m ns⁻¹ and the results with your optimization?

We will give more details on the calibration procedure in the methods section.

9) page 5, lines 10-12: this part is again too short and is confusing me. If I am not wrong I understand here that you finally chose to use an optimization procedure but not to consider manual measurements of SWE as a direct input to your GPR measurements. However, in the previous lines (see comment 8 above), you are saying that you used those manual measurements for fits. Please explain better your calibration/optimization procedure. In particular, it is not clear to me if there are some data points from 'manual measurements' displayed on Figs. 4, 5, 6 or 7 which were used for calibration/optimization. If yes, you should label them explicitly. In the absence of much more detail on this, the frontier between calibration and validation concerning SWE is seemingly tiny.

We are sorry for the confusion. The points in figure 4, 5, 6, and 7 display the position of the reference markers which are collocated with locations of manual measurements. Added information on the calibration procedure will clarify why manual snow measurements are not used as direct input but yet involved as part of the processing.

10) page 5, last sentence: please could you give detail about the estimation proposed by Tiuri et al. (1984) to make your paper self-contained.

We will consider providing further details from Tiuri et al. (1984).

Section 3:

11) page 5, lines 24-25: please could you show the radargrams! It is quite frustrating not to see any radargram in your paper.

Yes, we will include a radargram.

12) page 6, lines 9-10: I am not sure you need to repeat the two plots without S1. The snow depth on S1 works well but not the SWE. Please could you comment on that?

We will consider this suggestion. However it seems interesting to us to demonstrate the effect of a single problematic transect on the overall results (and how they can differ between snow depth and snow water equivalent). Misalignment problems between repeated overpasses at S1 had a considerably impact on differential snow water equivalent measurements which was not the case for differential snow depth measurements. This is because snow depth measurement can and were repeatedly taken at the exact same position (marker points) which was not possible for (destructive) snow water equivalent measurements.

Section 4:

13) Page 7, lines 3-5: the calibration/optimization procedure for density and SWE needs much more explanation (see my comments 8 and 9 again).

See above.

Section 5:

14) Page 8: the last paragraph of that section appears to me as a reiteration of the last paragraph of section 4. I would suggest you to remove this paragraph from one of the two sections.

Thank you.

Technical corrections, typing errors, etc.

Abstract, line 10 (second-last sentence): I would suggest you to either remove the content between brackets or keep it but not between brackets.

We will consider your suggestion.

section 1, page 2, lines 26, 27 and 29: in line 27 you are defining the term "snow ablation rates" for "change rates" but in line 26 (just above) you already use "snow ablation rates" AND in line 29 just below you use another term "snowmelt rates"... please fix this.

OK.

section 2.2.1, page 3, line 28: there is a typo ('parings')

OK.

Figures:

Fig. 1: could you please increase the size of the top and bottom right pictures? and try to improve them overall? I must say that they are not so easy to read.

OK.

Fig. 3: would be nice to indicate in the right photo -with an arrow for instance- the typical distance between two flags.

Thank you for the suggestion.

Fig. 4, 5 and 7: could you please add typical error bars for both the curves showing the continuous GPR measurements, and the data points for the discrete manual measurements?

We will consider your suggestion; note however that there was only one manual snow measurement per reference point.

Fig. 6: I suggest you to remove the two bottom plots without S1. I believe it is sufficient to keep the two top plots and indicate briefly in the caption why S1 is an outlier for SWE, and refer to text for explanation.

Thank you.