

***Interactive comment on* “On measuring snow ablation rates in alpine terrain with a mobile GPR device” by Nena Griessinger et al.**

Anonymous Referee #2

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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.

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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.

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?

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?

4) page 3, line 16: while mentioning the manual measurements here, you could refer

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to section 2.2.2. Otherwise the reader may expect those manual measurements to be detailed here.

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.

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?

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.

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?

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

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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.

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.

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.

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?

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).

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.

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.

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.

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

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.

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

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?

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.

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TCD

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