Response to reviewer 1 (Sergey Marchenko)

In situ measurements of meltwater flow through snow and firn in the accumulation zone of the SW Greenland Ice Sheet

Nicole Clerx, Horst Machguth, Andrew Tedstone, Nicolas Jullien, Nander Wever, Rolf Weingartner, Ole Roessler

Dear Reviewer,

We would like to thank you again for your thorough and constructive review of our paper, and the suggestions of how to improve its quality. Below, we respond point by point to all comments, and state how we would incorporate them in a final version of the paper. The responses (normal font style) to the reviewer's comments are written directly into the reviews (displayed in italic font style).

Nicole Clerx,

Fribourg, September 7, 2022

1 Specific comments

(1) Not sure, where the melt rate of 2 mm we per hour comes from?

Following the suggested assumption of 12 h of melt in one day, that gives 24 mm we of melt per day. With 35 day long melt season that is 840 mm we of melt in a year, which can't be connected with anything cited above.

One could read ca 600 mm we off Figure 2 in https://tc.copernicus.org/articles/13/1819/2019/ (cited just above) for the annual melt at KAN-U in 2012. That will give 17 mm we per day in summer 2012 on average and 1.4 mm per hour assuming a 12 h melt day...

It is exactly from the Figure 2 in the paper by Verjans et al. that you suggest that we got to this value. For 2012, we read off a total surface melt flux of 0.85 m w.e. yr^{-1} from this graph, which with 35 melt days and 12 hours melt per day gives a melt rate of slightly over 2 mm w.e. hr^{-1}.

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(2) Darcy flow lay assumes saturated (one phase) flow. While that is most likely the case for lateral flow described in this study, the vertical flow with extensive fingering is definitely not saturated when it comes to the bulk of the snow/firn mass. I think it is important to mention that here even if in the results section the associated calculations are presented as "saturated flow through flow fingers" and the discussion covers the topic in much detail.

Thank you for this suggestion, we will include a remark in this part of the manuscript as well.

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(3) To some extent this comment also continues the previous comment.
I would like to encourage authors to be as consistent and pedagogical as possible in using the terms velocity" and "hydraulic conductivity". Attentive reader surely finds his/her way through the material, but will also appreciate the effort, I believe.

LL389-396 is a good example: formally, there is no mistake, but intuitively it appears as two different properties are compared.

It is not immediately obvious as that flow rate numerically equals hydr. cond. for vertical flow. May be stick with one term (i'd go for velocity, easier to understand) and make a corresponding note.

In L250 area A is, perhaps, meant to be the area of the flow fingers, which is likely not known. That is ok, just make a note on that and possibly speculate that the flow velocity can be a lot higher, if one accounts for that and may be provide a "guesstimate" of what the relative area of fingering flow could have been.

Thank you for urging us to be as consistent and pedagogical as possible. We believe that in terms of consistency, we have been clear whenever talking about unsaturated percolation velocity or hydraulic conductivity (saturated flow velocity). In the methods-section we believe our explanation of the can not be significantly improved without compromising clarity and conciseness.

Regarding your suggestion on LL389-396: we think it is important to leave the phrasing as it is currently, to emphasize the fact that we are effectively comparing measurements that were done using different techniques, for different goals: in some cases it was merely the percolation velocity measurements that were the goal, other publications focused on

quantifying the material properties of the studied snow and firn and hence present real' hydraulic conductivity values.

As you say, in L250 A is indeed the total surface of the firn sample whereas it would be more appropriate to take the area of the flow fingers (which is impossible because we weren't able to measure the surface area of the preferential flow paths). We will incorporate a comment on this in the final version of the manuscript, including a remark regarding the potential underestimation of the flow velocity.

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(4) One question arising when interpreting the results from dye tracing experiments is: how long did it take for the tracer to go through the lower gate? The speeds derived using the time first rhodamine portions arrived to the lower gate yield the highest estimate possible. One could have possibly used the times when max R. concentration is observed or the midpoint between first and last portions of the tracer...

Given the simplistic nature of our RWT experiments (visual observation of rhodamine displacement), quantification of RWT concentrations was not possible. For future experiments it would indeed be better to use the maximum concentration instead of the first arrivals, but this wasn't feasible at the time.

2 Minor comments

(1) L5: "on the southwest Greenland Ice Sheet". Either southwestERN or add "of the"

OK.

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(2) L25, ref. to Marchenko et al., 2017: That reference does not quite fit the context it is used in. The article has little to do with firn in Greenland, although it does deal with water percolation. A good reference here could be the RETMIP paper by Vandecrux et al.: https://tc.copernicus.org/articles/14/3785/2020/ - an up to date overview of the reg. scale models at the GrIS and their performance.

Where this reference is relevant is the text at LL127 - 131, since the study showed that the bulk irreducible water content above the flow finger front can be as low as 1% and less as extensive dry (and cold) pockets exist in-between the preferential flow features.

Thanks for these comments. We will change the original citation to the RETMIP-paper, and include an additional comment + reference to the paper quoted initially.

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(3) LL44-45: When reading the definition of the runoff limit, the verb "begins" seems misplaced. May be define runoff limit as the "highest elevation from which runoff occurs", alternatively as "...part of the meltwater present leaves the ice sheet."?

This was the initial definition that we had thought of, but after the various observed melt seasons in the area we realised that it is not certain that (all) meltwater present leaves the ice sheet: refreezing can occur during/after colder episodes, even below the runoff limit. Hence we would propose to stick to the current formulation.

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(4) Caption to Fig. 1: check phrasing of the second sentence. Something is odd there, "eventually" is in the wrong place.

OK.

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(5) L105: "and is situated" can be easily skipped

OK.

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(6) L132: Consider rephrasing "is analogous to". Water flow through snow literally is flow through porous medium, and ample details on that are given just above)

Indeed. We will rephrase this sentence.

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(7) L189: is "WT" = water tracer? As a matter of fact, it is nowhere explicitly said that it is. Perhaps good to spell the abbreviation out the first time it is used.

Correct, WT is the abbreviation for water tracer. We will include the full definition of the abbreviation in the manuscript.

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(8) L197-198: "the distance between the base of the sample and the injector head was constant at 1 m" appearing in parenthesis can be skipped.

OK.

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(9) L203: "are": here and throughout the chapter, make sure that tenses are used consistently. So far narration was in the past tense, here we see present, which lower down becomes past again.

We will check and homogenize the tense(s) used.

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(10) L212: "sensors were inserted into the firn sample." can be skipped to have "Before the start of each percolation experiment four temperature sensors were inserted horizontally to about 20 cm into the sample ∼1 cm above its base.

OK.

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(11) L262-263: "...assuming that 2·res equals the average grain size observed in the sample": if that assumption is made, then it is not clear why is the SSA term needed here at all? It does not appear anywhere else, so one may as well get rid of the parameter at all.

We included the SSA-term for completeness and in particular to adhere to the original definition of the equation by Calonne et al.

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(12) LL260-261 and 268-270: not sure what motivates repetition of the unsaturated flow velocity values.

Thanks, we adjusted this to remove the repetition.

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(13) L309: "full cloud cover" = overcast?

Correct, we intentionally use the term "full cloud cover" here since it is more specific than overcast (which can mean partially cloud covered as well).

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(15) Equation (12): is C_0 defined later in L325 as "the background conductivity of the meltwater in a borehole..."? Could be good to explicitly define the term.

The reason that "background conductivity" is only mentioned later is because the equation is valid for any type of tracer and hence the fact that it concerns background conductivity in this case is experiment-specific.

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(16) "Some residual water...": The irreducible water content can be quantified using the density based parameterization from Schneider and Jansson, 2004 Journal of Glaciology, Vol. 50, No.168).

Yes, if one would know all the parameters in the equation they use:
$S_{wi} = \frac{\theta_{mi} \frac{\rho_f}{\rho_w}}{\phi}$
With S_{wi} residual water saturation [-], θ_{mi} irreducible liquid water content [-], ρ_f sample density [kg m^{-3}], ρ_w water density [kg m^{-3}] and ϕ matrix porosity [-]. In our case, it was unfortunately impossible to determine the irreducible liquid water content exactly, given that we don't know the difference in density of the matrix before and after full saturation.

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(17) L382: "Hydraulic head variations between individual boreholes throughout the field work period were calculated based on measured water table heights along the transect." Are the water table heights determined as described in LL303-304? If yes, then it is most likely of little relevance here, as the water table heights are referenced to the ice slab surface, which can be undulating and highly sloping as stated in ch. 6.3. It is the absolute heights that are important here as they are directly related to the Earth's field of gravity driving the water flow.

Correct. However, we decided to stick to our recorded values for water table height referenced to the ice slab surface. For clarity, but principally because we don't have a DEM at our disposition that is accurate enough at the scale of our field measurements to convert these water table depths (and snow heights) to their absolute 'height'.

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(18) L384: "...are relatively high compared to existing values": may be "earlier/previously published values

Thanks, we will rephrase this.

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(19) L506: "...is the period during meltwater can travel..." add "which" between "during" and "meltwater".

OK.

Response to reviewer 2

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