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 for your thorough and constructive review of our paper, and all the suggestions of how to improve its quality. Below, we respond point by point to all comments, and state how we plan to incorporate them in a revised 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). Revised figures are also included in this document. Technical corrections and/or replies to comments and suggestions made in the accompanying .pdf-file will be incorporated in a revised version of the manuscript.

Nicole Clerx,

Fribourg, June 21, 2022

1 General comments

In the present manuscript authors address the questions of melt water drainage from the areas above the equilibrium line of the Greenland ice sheet. On the basis of results from two field campaigns at 1700 - 2000 m asl on the K transect in SW Greenland conclusions are made regarding such properties of the snow/firn/ice as density, statigraphy, hydraulic properties. Most notably quantification of both vertical and lateral water flow speeds through snow and firn is reported from multiple field experiments.

Given the increasing melt rates reported from the Greenland ice sheet and projected for the years to come, domain above the equilibrium line undergoes rapid changes with the general pattern of glacier zone migration upwards. Field evidences from the area, particularly those of quantitative nature, are important for a better understanding of the ongoing processes and are crucial for their formal description in numerical models.

The manuscript is based on extensive field data, is well structured, presentation is generally logical and consistent. Results can not be said to report anything that was not observed earlier and do not allow to make conceptually new generalizations. At the same time this is a carefully prepared quantitative account on processes of melt water infiltration and runoff from a very dynamic part of the ice sheet and in my oppinion deserves to be made available to a wider audience.

It appears to me, however, that the manuscript requires a number of clarifications, additions and edits before publication.

Thank your for your thorough review. We agree that clarifying certain sections and adding/editing some of the information provided would improve the manuscript.

2 Specific comments

(1) LL33...: I think that readers will appreciate a description of the observed and expected changes in glacier zones at the Gr ice sheet. The fact is that in a warming climate all zones move into higher altitudes. Cold firn is replaced by either warm firn or superimposed ice zone or even ablation zone depending on the regional climatic conditions. Otherwise it is not obvious how the slush conditions discussed here are related to the slush of the cited PFAs.

We will include a more thorough description of the glacier facies on the Greenland Ice Sheet in general and where this study is situated in terms of the described glacier facies (at the moment and potentially in the future).

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(2) Ch. 3: I think that here a few references are missing. First three equations, as well as the Darcy flow law are given without any citations. Smth. classical like:

- *first snowpack paper: Bartelt, Lehning, 2002, https://doi.org/10.1016/S0165-232X(02)00074-5*
- *Jordan R, Albert M and Brun E (2008): Chapter: Physical processes within the snow cover and their parameterization. In Armstrong Richard and Brun Eric eds. Snow and Climate: Physical processes, surface Energy Exchange and Modeling. Cambridge: Cambridge University Press, pp. 12-69.*

will be good here.

OK, we will include those citations in the revised manuscript.

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(3) Eq. 4: It appears to me that equation 4 might need corrections: density rho and grav. acc. g are missing in the rightmost component.

Indeed, thanks for spotting this error. Will be corrected.

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(4) L104: Since quantification of hydraulic conductivity is one of the focuses of the study it would make sense to spend some words explaining its physical meaning. The fact that it has the same units as the water flow velocity may be confusing and further justifies such a clarification. Smth. along the lines: property that describes the ease with which given liquid can move through porous media. It depends on ..." (from Wikipedia).

Agreed. We will include a better definition of the hydraulic conductivity.

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(5) L139: It would be good to specify what was the sprinkling pattern at the surface of snow. Did water come out as a single jet or it was sprayed in small drops in a 3D fan pattern. Also how many injectors were used and what was the distance between the injectors and the snow surface. These are issues that potential readers may be wondering about in connection with the preferential flow patterns reported later. Particularly since "deep holes" are mentioned at line 147. The overall question is: "is it even possible that the observed preferential flow formation is caused by the spay pattern?"

The sprinkling head on ROSA contains 84 outlets that each have a diameter of 3.5 mm, and are arranged in a diamond grid. Ensuring they all contributed to irrigation more or less equally was part of the pre-experiment checks that were always carried out before starting an experiment. The sprinkling head was located 1 m above the so-called ‘dripping plate’ on which all samples rested. The total distance between the injectors and the snow/firn surface was roughly 85 cm on average (depending on the thickness of the sample, the distance between the base of the sample and the injector head was constant at 1 m).

The deep holes mentioned at line 147 are specifically due to (consciously) not moving the sprinkling head laterally w.r.t. firn block. During all other experiments the sprinkling head was moved in 5 cm-increments every 3 minutes to prevent these holes from forming. Furthermore, in the first two trial experiments, which are not described in the manuscript, we tested whether ‘free fall acceleration’ of the water droplets due to the distance between the sprinkling head and the top of the samples would affect the flow paths. We found this not to be the case, since the dyed water spread out laterally over the surface of the snow/firn samples quicker than initial vertical flow of water (i.e. the surface would be pink before any signs of vertical meltwater percolation could be seen on the sample sides, as well as before any outflow occurring).

We will include a better description of the sprinkling head and ‘water delivery’ to the samples in the revised manuscript.

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(6) L198: Is it right that the discharge of water collected by the lysimeter and measured by the tipping bucket during 15 min before the supply cut off was divided by the sample area? If yes, that’d be good to express that more explicitly. This technique is close to the constant head permeability test: http://www.geotechdata.info/geotest/constant-head-test.

Yes, the volume of discharged water was divided by the sample area. It is true that this method is close to the constant head permeability test, but not exactly the same. The constant head permeability test works when being able to put the sample in an almost closed-off container to allow for full wetting/saturation, which was not the case for the ROSA set-up. Another difficulty in applying this method to our experiments is that, even though outflow at some point stabilises, densification still occurs, which means that the sample is not yet fully saturated. This was confirmed by visual evidence after all experiments, none of the samples were fully pink so there were always unsaturated patches left. This means that resulting permeability values calculated in this way would always be a (significant) underestimation of the 'true' saturated snow/firn permeability.

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(7) L208, reference to eq. 10: It is nowhere specified how the SSA appearing in equation 10 was quantified.

The SSA was quantified using $r_{es} = 3/(SSA \cdot \rho_i)$, assuming that $2 \cdot r_{es}$ equals the average grain size. We will include this in the revised manuscript.

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(8) Eq. 11 and the method of velocity quantification through conductivity measurements: Time between what events?

Perhaps it is the time between the measured max conductivity and conductivity value C...

Then, since multiple time-vs-concentration data points can be chosen, one can get multiple estimates of q from the decay curve. This guess is confirmed by the figure A1 showing multiple dots.

First of all, readers can not be left guessing, more transparency in a description of the applied routines is needed. What is also apparent from the figure is that the curve is far from being linear. That implies that choosing different values for time and concentration one may get vastly different values for q. A comment on that is crucial for reporting and interpreting the results.

Equation 11:

$$q = -\frac{\pi r}{2 t_i \alpha} ln(\frac{C}{C_0})$$

The time t_i used in Eq. 11 is the time after the tracer injection. Rewriting this equation gives:

$$ln(C) = -\frac{2\alpha q}{\pi r} t_i + ln(C_0)$$

The dependence of the logarithm of average interval tracer concentration on time (i.e. the gradient/slope of the linear regression of $ln(C)$ vs. time) is proportional to horizontal flow velocity:

$$q = -0.5 * 1 \cdot \alpha \cdot \pi \cdot r \cdot slope$$

This is better explained in Pitrak et al. (2007), and also in section 9.4 of Freeze and Cherry (1979).

Note that in the current version of the manuscript, we give the apparent velocity, which is not the same as the average linear velocity of meltwater flowing through the pores (Eq. 9.28 in Freeze and Cherry, 1979). We will improve the description of this method/calculation in the revised manuscript, and also give the average linear velocity instead of the measured apparent flow velocity.

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(9) Ch. 5.2.2: It is likely that in the described setting the lateral variation in the potential energy is the driving force for water drainage through the snow matrix.

It would thus be valuable to present information about the slope and aspect of the surface terrain and of the ice layer on top of which water is drained, if there is any of such info available.

In L293 water table height is mentioned. What exactly is meant here? Is that height above the sea level (= geoid) or rather depth below the glacier surface or something else?

We agree that variations in the total hydraulic head (i.e. potential energy) is the driving force for water drainage through the snow matrix. Due to the very large heterogeneity in ice slab surface and the scale of the measured water depths (both are in the order of centimeters) compared to the overall slope of the terrain (0.30°, i.e. a depth difference of 0.5 cm per meter distance), it seems likely that local heterogeneities/undulations at the surface of the ice slab are more important in locally determining the direction than the slope and aspect of the surface terrain. (On a larger scale, we believe that the overall surface slope controls the flow direction.)

We define water table height as the thickness of the water column on top of the ice slab, after the water level has nearly instantaneously equilibrated following drilling of the borehole and snow removal from the hole.

We will include more detail on this in the results and discussion-section of the improved manuscript.

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(10) Ch. 6.1., paragraph 1: This paragraph is very confusing. Readers are likely to be lost in the many mehtods and directions of the water flow in snow and firn.

The chapters presenting results above contain velocities:

- *vertical, from ROSA experiments: 0.167 - 0.438 m / h*
- *lateral from salt experiments: 1.3 - 14.2 m / h*
- *lateral from dye tracing: 3.5 - 15.1 m / h.*

On top of that come hydr. cond. quantifications, which are, of course, not the same thing, but they do have the same units and in case of vertical water flow are the same as flow velocity, if I understand it right... A reader may be wondering: "in Ch. 4 at line 215-216 reported velocities are claimed to be derived "using the lag time and sample height". How is that related to hydr. cond-s?"

Discussion will benefit from more precise formulations and also from a more thorough and consistent description of the background theory, which highlights the comment to line 104.

Thanks for pointing out that (this part of) the discussion is not clear. We will improve the discussion section in the revised manuscript.

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(11) Methods chapters and L366: As far as i understand the permeabilities assessed from Darcy flow law rely on the results of the infiltration experiments yielding the K values. At the same time the k values parameterized following Calonne et al. (2012) rely on the measured density and SSA values. It is, as a matter of fact, nowhere explained how the latter are constrained.

This makes an important difference between the two kinds of k values, that is not properly highlighted in the text. The k values coming from Darcy flow law and K are, in a way, based on a more solid empirical dataset, but assume the validity of the D. flow law for the conditions of the experiment.

The latter fact calls for a more thorough explanation of the D. fl. law: what assumptions are implied and in what cases is it commonly used and was shown to do a decent job.

We will include a clearer description of how we established (measured) density and SSA values. Also the assumptions and validity of Darcy's law will be expanded upon to better explain the methods we used to calculate and compare hydraulic conductivity- and permeability values across the two datasets.

(12) This paragraph starting at L390 is largely a reiteration of the statements in the results chapter. Some explanation is expected here. It is a big thing when results from one method are off from results coming from another method by 10-1000 times. So something is seriously wrong with the quantification of the lateral water transport rate using different approaches. Either in the measurements/calculation routines or in the assumptions assumed by the methods.

Here and also in the results chapter, getting to the same conclusion from different ends (comparing velocities or permeabilities) appears more as a double check that one does to validate routines. But these are intermediate results providing auxiliary infomration that is important but not necessarily relevant in a publication. Readers can assume that results are solid and not be bothered by double checking.

Although we strongly agree that the mismatch in permeability values resulting from our measurements/calculations is a major concern, we would also like to mention that permeability ranges in other natural porous media can cover multiple orders of magnitudes and permeability therefore is generally represented on a logarithmic scale. Sandstone permeabilities, for example, range between 10^{-10} and 10^{-15} m^2. In that sense variations of 3 orders of magnitude would not be unexpected. Nevertheless, since both the Darcy-based calculation and the Calonne-parametrisation were applied to the same samples, this discrepancy should of course not be there. We will further investigate this matter and provide a better discussion of the differences in permeability values and calculation methods in the updated version of the manuscript.

An additional factor possibly delaying runoff may be saturation of the likely thicker snow and firn higher up by the melt water before is becomes equally mobile as in the estimates presented in this study (7 m per h). Likely at the early stages of melt water can't move equally fast.

Comparison between vertical percolation (through preferential flow fingers) and lateral flow velocities shows that velocities are comparable, but there likely indeed is a threshold for the minimum amount of generated melt water required before lateral flow starts (i.e. sufficient volume to overcome local undulations in ice slab surface and build up a substantial water column).

In future work we will investigate changes in flow behaviour with various amounts of melt and different scenarios for melt input timing.

(14) Reference list: I am not sure if TD standards allow including “in prep" publications in the list of citations. There are two such references: Machguth et al., Tedstone et al.

Thanks for pointing these out. The references will be updated or removed in the revised manuscript.

3 Comments to figures

Figure 2

- *May be break the figure in 4 panels A, B, C, D?*
- *Give names to the axis on second panel, easting, northing*
- *Clarification seems to be needed for what the readers can see at what is now panel b. The upper (and earlier) image appears to have more bright blue spots than the lower (and later) one. Intuitive interpretation also confirmed by the name of the plotted property (ndwi) is that these bright blue spots is surficial water. Then one may be wondering why is there less water later on in the melt season. That's counterintuitive: as cumulative melt increases one expects to see more water at the surface. It could be good to clarify this, alongside with pointing to the fact that melt water is seen higher up in the terrain on the later image (L 70). Or is it simply clouds that block the surface in the lower reaches of the later image?*

Please find below an updated version of the figure and its header, as would be incorporated in the revised manuscript.

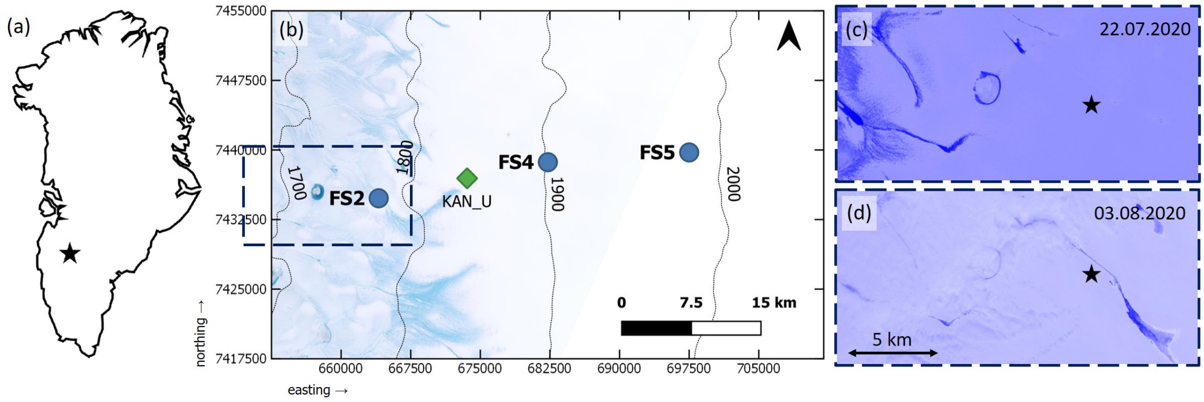


Figure 2: (a) Overview map of Greenland, with the black star indicating the approximate field site location. (b) Map of the study area, showing the various sites on the Greenland Ice Sheet (FS2 for summer measurements, FS4 for spring data collection, both sites and FS5 for firn stratigraphy and KAN_U for meteorological data). Thin black lines represent elevation contours from the ArcticDEM modified to show elevation in m a.s.l. (Porter et al., 2018). The background image is a Sentinel-2 true color composite from 12.08.2019, around the time of peak melt that year. The dashed dark blue rectangle indicates the outline of the composites shown in panels c and d. (c) Sentinel-2 NDWI composite showing the liquid water presence (in bright blue) on the ice sheet surface on 22 July 2020. (d) Sentinel-2 NDWI composite showing the liquid water presence on the ice sheet surface, again in bright blue, on 3 August 2020. Note that the surface meltwater in the lower areas is masked by the presence of clouds. For (c) and (d): the black star indicates the location of field site FS2, the NDWI composites have not been corrected for cloud artefacts. For (b), (c) and (d): source: sentinelhub Playground.

Figure 5

- *symbology is clear but presentation is not consistent: Change the font color for the left vertical axis and label to blue to make it apparent that the hydr. cond values are to be read on the left. This will also allow to get rid of the first legend entry. Alternatively: all fonts in black, but two more entires in the legend.*
- *It may be possible to give more space to the data curves by reducing the vertical axis labels and titles: they are the same for all panels and the grid will likely keep the curvves readable even after keeping the axis attributes only at the very left and right of the figure.*
- *titles of the panels of this and other figures. I'd suggest to start with the name (e.g. firn4) and give it in italic font to match the text style. Then give the date and time. Year can be skipped and given in the figure caption.*
- *in the firn2 experiment the "pump off" time marker is missing? Did it get lost on the way or there was something special about this experiment?*

Thanks for your suggestions, we have incorporated them in the updated figure & caption below.

The "pumps off" marker for *firn2* is missing because we left this experiment running for longer than is shown in the figure. The sprinkling head was not moved during this experiment, and this leads us to believe that the last part (i.e. beyond 02:15) is not representative anymore: at this point supplied water would likely simply 'fall' through the deep holes that were created by keeping the irrigation points at fixed locations.

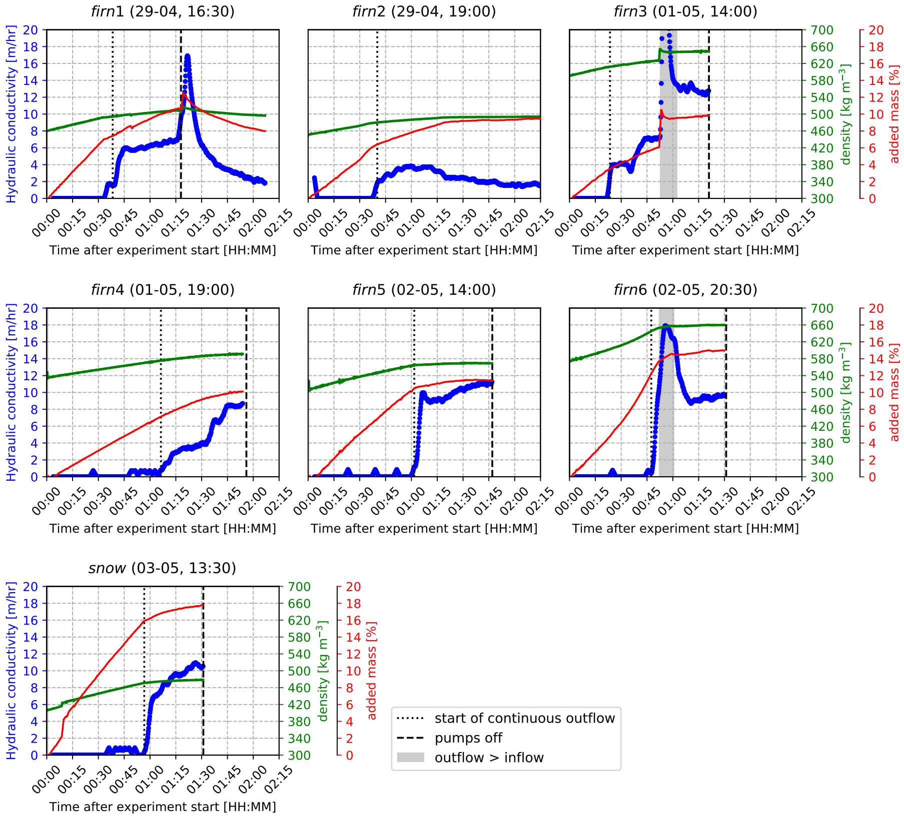
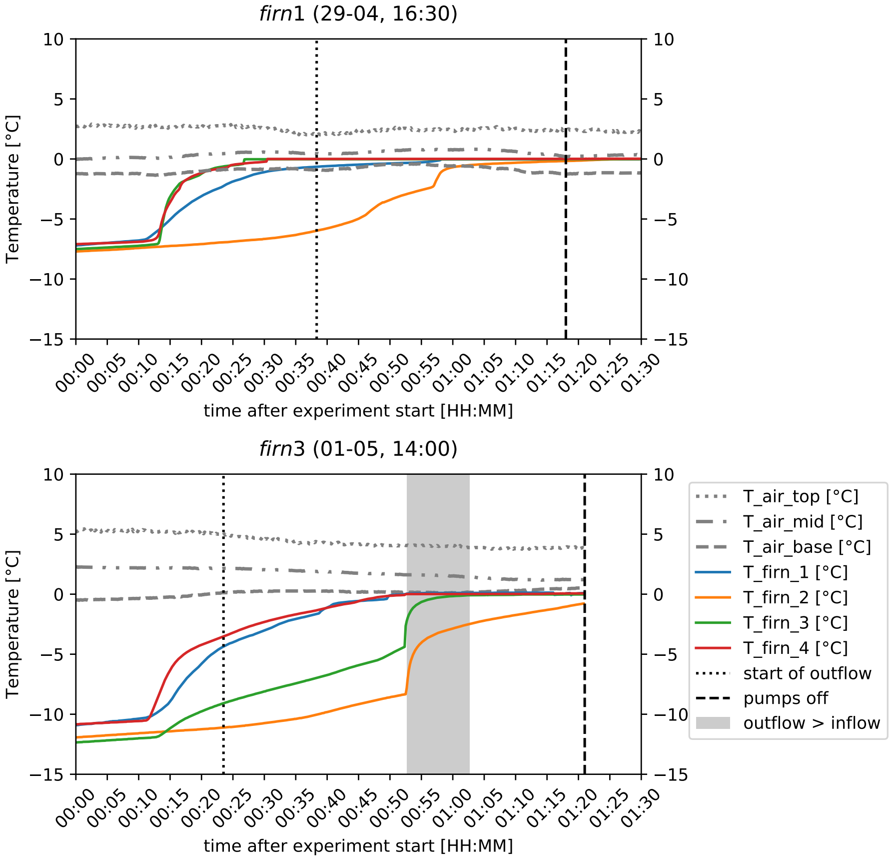


Figure 5: Hydraulic conductivity, added mass and density over time for 7 individual experiments. In blue the calculated hydraulic conductivity, in red the firn sample mass as a percentage of its mass pre-experiment, and in green the density over time. The dotted line indicates start time of continuous outflow, the dashed line shows the time at which water supply was stopped (note that for experiment *firn2* the 'pumps off'-label is missing, since this experiment was continued for longer than the time displayed here). Grey shading shows where outflow>inflow. The title shows the name of the experiment and its starting date & time [dd-mm, HH:MM], all measurements were carried out in 2021.

Figure 6

- *the symbols for the lower and middle air temperature curves are not distinguishable;*
- *the order of the three different air temperature legend entries is counterintuitive;*
- *the order of the air and firn temperature legend entries is counterintuitive, i'd suggest to have air on top in the legend box.*

Please find an updated version of the figure & caption below.


*firn*1 (29-04, 16:30)

*firn*3 (01-05, 14:00)

Figure 6: Air- and firn temperature over time for experiments *firn1* and *firn3*. The blue dashed line indicates start time of continuous outflow, grey shading shows where outflow>inflow. The black dashed line shows the time at which the experiment was stopped (when pumps were turned off). Coloured lines show temperature evolution at 4 locations within the firn block, ∼1 cm above its base. Grey dashed and dotted lines represent air temperatures next to ROSA.

Figure 13

- *figures in publications usually have no titles. Their function is taken by the caption.*
- *i'd suggest to "repack" this figure and adopt a structure that is closer to a table: move the references to the right or left so that they make one more column along. The other two columns will be the method and the actual values.*
- *combine the info from Kattelman (1987) by bringing closer together the individual data "pieces": I do not see why Ambach et al 1978 and Vallon et al 1976 need to be wedged in between. This will also allow to get rid of another "dimension" of the figure - grey shading behind references from Kattelman (1987) - in the updated more "table-like" structure that info can be given in the "references" column.*
- *regarding the rows of the "table". The existing structure is logical: methods make the higher order subdivisions, then references can define the lower-level subdivisions with a few grouped by the Kattelman (1987) figure bracket or similar. The original values first reported by this study could be either fitted in this structure or presented as a stand-alone group to make it more obvious what the study's contribution. In either case i think 2 more lines can be presented in this figure: vertical infiltration rates derived at time delay before onset of runoff after the start of spraying divided by the sample thickness (0.17 - 0.44 m / h) and lateral flow speed from dye tracing experiments.*

Thanks for your suggestions. We would propose to keep the figure more or less in it current form and not change it into a table, but have modified it to make it clearer.

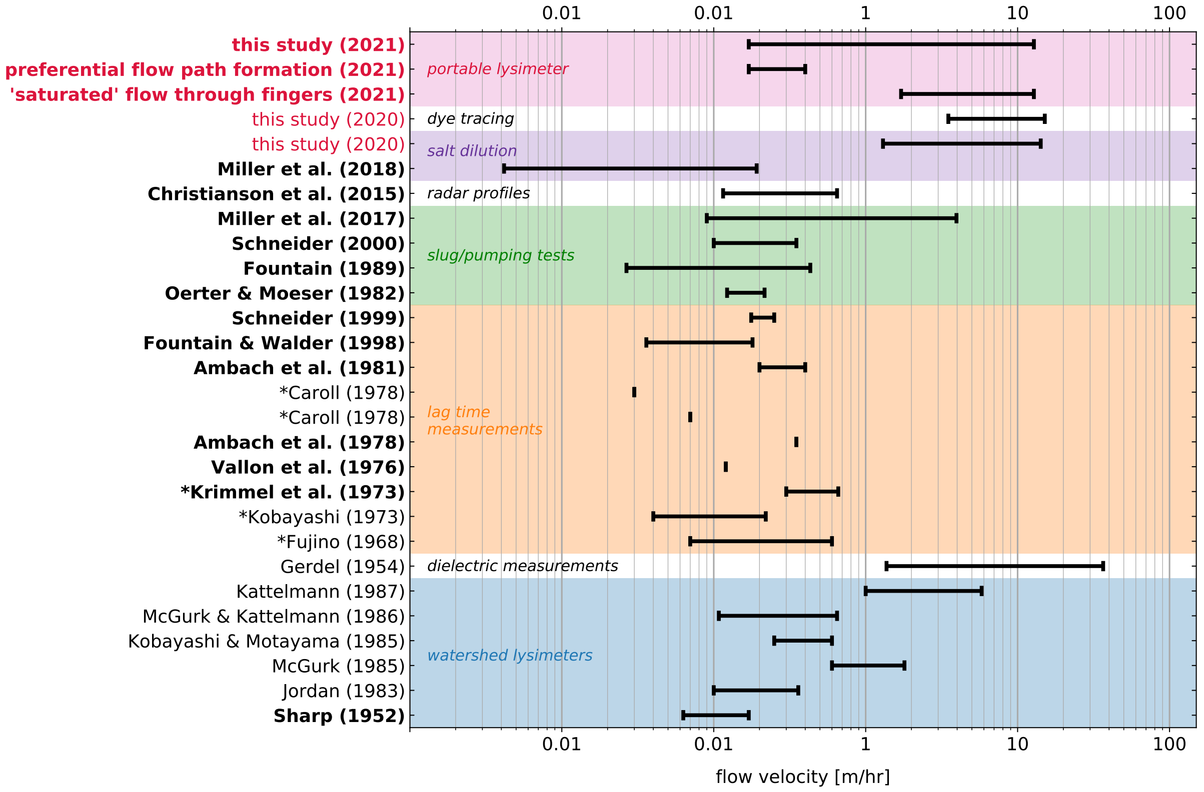


Figure 13: Flow velocities through snow and **firn** as measured in this study, compared to other values published in literature. Author names preceded by an * indicate that the original papers were not available, quoted values were found in Kattelmann (1987).

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