Dear reviewer,

we thank you a lot for your very valuable comments. This certainly helps to improves our manuscript. Before we start answering to your comments, some explanations to method and data, partially, independently of the reviewer's comments:

1) As also updated shortly after submission to the editor, blowing snow dataset of 10 m (there was an issue with the script leading to wrong mass flux) and Magnaprobe paths were corrected. As already mentioned, both adapted processing steps do not affect the conclusions.

2) Data that has been changed due to comments from a reviewer (omitting 3-sigma filtering, removing Y-axis shift for the HS-SWE function) do not affect conclusions, as well. All in all, the total difference is only a few mm.

3) We omitted standard deviation in the revised version (and the conclusions on the roughness) as indeed we find a skewed SWE distribution rather than a Gaussian distribution – so we cannot conclude on spread and snow surface roughness.

4) A few co-authors were added.

In the following we come to the answers to your comments.

I have the following questions/comments/concerns which should be addressed.
Length – This manuscript is made exceptionally long through the inclusion of the snowfall measurement device assessment. I completely understand why the authors felt the need to combine the two datasets (snow thickness transects and snowfall sensor assessment) but it makes the manuscript challenging to follow and keep track of all the different sensors and approaches. A quick random sample of 20 TC papers online makes this one the longest. I would suggest that the snowfall sensor assessment effort could be its own Brief Communication and use it to feed the snow transect SWE work as a separate full paper to TCD. That is a decision for the Associate Editor and the authors.
Indeed the paper got relatively long. However, the amount and the different sources of data mean that the paper has to be a bit longer - we see the combination of snow cover and snowfall in this manuscript as a key element. Nonetheless, we see some issues when reading the paper, as also suggested by the other reviewer. We came to the conclusion as well that the manuscript needed some consolidation of the method and data description that it reads with a better flow. Hence, we invested work in better readability rather than making the manuscript shorter. We moved some explanations to the method section and added a table for a better overview, among others. This is our suggestion to you and the associate editor.

Brine/salt as a term in snow mass balance equation (Eq 2) – It is well known that snow on first-year sea ice types entrains expelled liquid brine from the near-surface sea ice volume into its cover during the fall and winter seasons through vapour pressure gradients via capillary and wicking action. Some of this salt can also be entrained from frost flowers once they erode after formation. This well-known brine volume in the basal layer of the snow cover has significant implications for heat transfer between the sea ice and atmosphere and thermal conductivity of the snow, including microwave remote sensing and satellite estimates of the snow and sea ice thickness via altimetry. So, shouldn't brine in the snow on first-year sea ice be a mass balance input (term) in the equation (even though this study investigates mainly investigates SYI). I need to understand its magnitude relative to say sublimation or evaporation or diamond dust, which are mentioned throughout. I good starting point for assessing the magnitude of this quantity are the following four papers 10.1016/j.coldregions.2009.03.009 doi.org/10.1016/0165-232X(84)90034-X doi.org/10.1080/01431169208904280 10.1029/96JC03208 The latter paper by Nghiem et al., 1997 report 2-4 mm of discontinuous brine slush on the ice surface. If this is all wicked up, it may be a relatively significant input, especially if the snow is thin.

Thanks a lot for this input. We discussed this and for us the question is not easy to answer. Please also note that this paper is mainly about surface processes – however for the sake of completeness we described the mass balance terms in the introduction. Nonetheless, we adapted the introduction according to your suggestions. As written in the introduction, this slush induced by brine expulsion may re-freeze at some point to sea ice with a mass contribution on top. Therefore, we added "B" for brine as positive mass balance term in the introduction. However, "B" may transform snow into sea ice at a later point. We tried to take this into account in the revised introduction. However, due to the complexity of this question, we cannot go into more detail including elaborate research. For the future, for sure, with the data collected during MOSAiC, including snow-ice interface scans with the microCT, there will be lot of opportunities to work on this question.

We revised in the introduction:

"$dM/dt = P \pm E_s \pm E_e - E_D - R + B - I - \nabla \cdot D - L - S$,

… B is brine mass infiltration rate into the snow cover from below, …

… Considering brine infiltration B which is often accompanied by the expression of frost flowers, Nghiem et al. (1997) found in indoor experiments a 4 mm slush layer forming beneath frost flowers. However, when snow falls onto frost flowers or a layer of brine, it gets soaked by brine, transformed into slush and, when cold enough, it is often transformed quickly into snow-ice. Hence, we assume that brine only can be a positive mass term as long as a certain ambient temperature is not undercut, where the snow begins to transform into snow-ice."

Minor editorial comments

Introduction – I'm quite surprised the work of S. Savelyev et al., 2006 is not reviewed, especially since it deals with blowing/drifting snow on sea ice. doi.org/10.1002/hyp.6118

Thank you for suggesting, we were not aware of this paper. We added this in the introduction and referred to the high relative humidity measured that probably goes along with low blowing snow sublimation rates.

L193 – add an 's' to transect

Revised.

L217-19 – Why not just say standard deviation instead of saying z-score? It's a clunky statistical term that some may not be familiar with.

Revised – we removed the processing step completely – so not required anymore.

Figure 3 caption – a bit odd to start your caption with 'All used'. Why not just start with 'Magnaprobe …'? Also, near the bottom of the caption, change 'origin' to 'originate'.

Revised.

L233-34 – 'Furthermore, as we made for each snowpit ….' Reads awkward. Consider revising to 'Furthermore, for each snowpit we made at least …'.

Revised.

L352 – I would argue that snowfall is estimated as opposed to retrieved.

It is common to use the word "retrieved" or "retrieval" for computation of parameters based on remote sensing, no?

L361 – 'off' should be 'of'

Revised.

L368 – 'As we found for 280 m the highest snowfall rates …' is awkward. Please revise.

Revised.

L385 – I don't think 90 o C is correct … 90 o ?

Thank you - revised.

L416 – remove 'take' .. or add 'into'

Revised.

L451 – rounded rounded

Revised.

L463-464 – Not so sure you should mention 'saturation' here. Why not just save to

introduce in the Discussion. If you do so, take L459-63 with you.
Revised – removed in the result section (but was already in discussion).

L475 – confusions … I'd make singular
Revised.

L508 – add 'one' before another
Revised.

L516 – Starting this sentence with 'Especially' is awkward, Just start with 'The'
Revised.

Fig 11 caption – areas … isn't there just a single green shaded area?
Revised.

L549 – 'than' … not 'that'
The sentence is: "While the apparent overestimation of the sensors in Fig. 13a is likely due to erosion (and hence strongly biased), the different magnitudes of RMSE reduction (Fig. 13b, Tab. 2) suggest that PWD22PS is less affected by overestimation **due to high wind speeds that accompany blowing snow**, compared to PWD22MC or Pluvio2, both of which were installed near the surface." – So we mean "that" here.

L551 – only one 'a'
Revised.

L558 – Not sure this is a sentence
Revised – we changed to "Taken together, we detected five significant snowfall events. If we use PWD22PS as reference..."

Fig 12 caption – 'and' before 'KAZR'L560-61 – this sentence is identical with L595-6
Revised.

L599 – decreased … remove the 'd'
Revised.

L600 – 'From 20 February 2020, also the standard deviation stabilized' … is an awkward sentence, please revise.
Revised. Indeed, we

L613 – 'strengths' is spelled incorrectly
Revised.

L618-19 – Please revise … reads awkwardly
Revised.

L628-20 - Furthermore, the used bond strength parameter of A = 0.18 that we used in the computation was found by (Clifton et al., 2006) in a wind tunnel, with a temperature range of -16 to 0 â¦C which was undershot 620 most of the time during our investigation period of MOSAiC (Fig. 12c). Not sure why this isn't in Section 2.22 or 2.3.
Revised.

L635 – 'where values found of 30% less … ' is awkward, please reword
Revised.

L636 – replace 'as' with 'at' … the end of MOSAiC …
Revised.

L743 – spelling 'buoys'
Revised.

L751 – spelling 'reason'
Revised.

L795 – They
Revised.

L796 – '… first validation of the ERA5' is a bit of bold statement (I would say Wong et al., 2019 and Graham et al., 2019 evaluated it with buoy data or other validation data among others)
Revised – we changed to "However, to our knowledge, we present the first validation of ERA5 snowfall for the high Arctic, based on a combination of repeating snow depth transect and a set of in-situ precipitation sensor data."