The manuscript "Recent Precipitation Decrease Across the Western Greenland Ice Sheet Percolation Zone" by Lewis et al. presents large scale GPR transects and accumulation derivations thereof for more than 4400km of the Western GrIS. Such data are combined with firn cores to enable layer dating and accumulation calculations from density measurements. Vertical in-situ data allow accumulation derivations for the last 2 to 6 decades enabling trend assessments. In-situ trends are compared with RCM outputs to analyze for changes in accumulation and precipitation in relation with global temperature changes. The authors describe significant decreases in accumulation rates within the last 2 decades, which they attribute to shifting storm tracks reducing precipitation mainly for the summer months and increasing surface melt. I consider the presented work as novel and certainly significant for the scientific community especially because of the extensive data collection presented in this work. However, some redundancies, imprecise descriptions and the confusing structure of the manuscript prevent publication in the current state. I recommend to focus more on conciseness and maybe reconsider the total volume of the presented data. How about splitting into 2 manuscripts: one presenting the in-situ data including validation/ comparison with RCM results and the subsequent dealing with implications and atmospheric circulation simulations. Right now, the reader gets a bit lost in all the error/ uncertainty analyses combined with validation proofs for numerous statistics.
Thank you for your thorough and helpful review. We appreciate that the manuscript covers a great deal of ground between the extensive data collection and climate-based analysis. However, we have decided not to split the manuscript in order to keep the data collection and analysis together. We feel that the background and data collection are necessary to motivate the reader to think about recent GrIS SMB changes. We use the field measurements to validate RCMs, which we then use to examine widespread SMB changes across the whole GrIS. We do not think two manuscripts would be able to portray these important results as accurately as one longer manuscript.
We have shortened the manuscript and reduced the length of several sections, particularly the introduction, to reduce the total volume of information. We believe that the manuscript is more concise and will nicely fill a gap in the literature of recent GrIS SMB measurements.

Major points of criticism are: The structure of the manuscript is very confusing. The methods section comprises large fractions of discussion and data interpretation. Please revise the structure and attempt to shorten the manuscript whenever possible.
We agree that some of the text originally in the methods is too verbose and is not appropriate for this section. Specifically, we removed material about the average relative permittivity, clarified how meltwater percolation effects isotope signals, added a sentence about comparing thermistor measurements with MODIS satellite derived temperatures, and removed a sentence within the leave-one-out cross validation paragraph. We feel that the radargram and density plots, while technically results from this study, belong in the methods section because they help the reader better understand the accumulation calculations and TWT-depth conversions.

The introduction comprises almost 3 pages. It is clear to me that you want to introduce all relevant literature and topics, which are presented. However, if splitting into 2 manuscripts (see above), you could certainly focus more on less different topics. Parts, which could be shortened are L54ff and L89ff.

We have shortened the introduction from three pages to two pages and removed unnecessary background material. We have shortened much of the material discussed in L54-L89 because Greenland summertime melting has previously been thoroughly discussed in the literature and does not need to be explained here in great detail. We appreciate this feedback and feel it has made the manuscript more concise.

At least to me, it remains unclear how specific values are determined. For instance, epoch and annual accumulation values are hard to distinguish. It would be better to clearly distinguish in between these two.

Annual accumulation is determined from the firn core chemistry data and is only shown in the background of Figure 5. We do not use discuss individual annual accumulation rates in this manuscript.

Epoch accumulation (average accumulation over multiple years) is calculated from adjacent IRHs (equation 3) in the geophysical data across the entire GreenTrACS traverse. We use these values to determine changes in accumulation in sections 3.4 and 3.5.

We changed L330 to "We assume uncertainty in dating the firn cores from annual variations in chemistry…" to clarify this point.

Did you actually pick each individual layer in the radar data or just for specific locations where layer resolution is clear or just the 5 year layers as indicated in Fig. 5? This remains unclear, same for the accumulation calculations.

As can be seen in Figure 3, we trace these individual layers across the dataset wherever the penetration depth (and equipment malfunctions) allows us to do so. Accumulation is calculated everywhere along the GreenTrACS traverse.

We have added to L269-271: "Each horizon is traced throughout the traverse, except in areas where the attenuated signal makes it too difficult to interpret."

You state that 1m fractions as well as 3cm parts of the cores are analyzed (L233ff) in the field and lab. Were those core fragments further cut for more highly resolved density measurements?

Core fragments were measured and weighed in the field as well as in the Dartmouth College Ice Core Laboratory freezer to calculate depth-density profiles. We repeat these measurements in case cores are lost or melted in transit, to double check for measurement errors, and to reacquire measurements in a controlled laboratory setting. We measured pieces along natural core breaks during the drilling process and did not further cut these pieces for higher resolved density measurements. For more information see Graeter et al. (2018).

In addition, average melt rates in Fig. 11 and discussed in Section 3.5 are not adequately explained. I don't see how such values are generated (derived from RCMs, calculated in accordance to observed ice lenses as in L581?).

Melt rates were measured from collected firn cores and published in Graeter et al. (2018). We measured ice layer thickness for each core using a light table in the Dartmouth College Ice Core Laboratory freezer. We then total the ice layer thickness per year using the chemistry derived depth-age scales.

We have added text to L213-214: "We measured melt layer thickness in the laboratory following Graeter et al. (2018)."

RMS values describing deviations from RCMs lack an explanation for the uncertainty range.
We have added the following text to L466-468 "Averaged over all 4436 km of the traverse, the RMS difference (± 1σ) between each model and GreenTrACS accumulation over corresponding data periods…"

In summary, I must admit, I got lost with all the uncertainty values being presented. What are sigma_epoch errors, how are these values related to sigma_accumulation-rate? I recommend to work carefully on the respective sections and maybe include a sketch of the applied workflow to derive accumulation data from radar IRHs.
σ_{epoch} is the uncertainty in accumulation rate for any single epoch. This combines all the individual uncertainties discussed in section 2.6 into one general uncertainty that we can use to compare our accumulation rate for a specific epoch with RCM accumulation rates.
$\sigma_{n\text{-}epochs}$ is the uncertainty in accumulation rate for multiple epochs. We use this uncertainty when comparing our accumulation rate over multiple epochs with RCM accumulation rates.
We have clarified equations 5 and 6 to simplify these complicated concepts.
L341-350 now reads "We find the total accumulation rate uncertainty for each epoch to be 0.0709 m w.e. a^{-1} from equation 5.

$$\sigma_{epoch} = \sqrt{\dot{b}^2\left(\left(\frac{\delta h}{\Delta h}\right)^2 + \left(\frac{\delta t}{\Delta t}\right)^2 + \left(\frac{\delta \rho}{\rho}\right)^2\right)} \qquad (1)$$

… To calculate uncertainty for accumulation averaged over multiple epochs ($\sigma_{n-epochs}$) we divide our uncertainty σ_{epoch} by the square root of the number of traced layers (n) at that location.

$$\sigma_{n-epochs} = \frac{\sigma_{epoch}}{\sqrt{n}} \qquad (2)."$$

Just for clarification: The accumulation rate uncertainty is 71kg/m2/a, I interpret this value as the max accuracy you can achieve from GPR transects The RMS deviation to IceBridge accumulation rates is 39kg/m2/a, which is within the error margins. For annual accumulation rates in Fig. 5, I would expect to have error margins as stated above being included. How reliable is a 5-year standard deviation in accumulation rates?
Thank you for the clarification question. The GPR accumulation rate uncertainty for any single epoch is 0.0709 m w.e. a^{-1} and the average RMS difference from IceBridge accumulation rate is 0.0387 m w.e. a^{-1}, so they are statistically indistinguishable from each another.
The error bars in Figure 5 represent those uncertainties.
The five-year standard deviation in firn core accumulation rates accurately captures the variability of year-to-year fluctuations in accumulation throughout this region.

The RMS deviation to RCMs is 48-82kg/m2/a and again within the error margins of the radar. Annual trends in precip are at 7kg/m2/a2. Consequently, you would need at least a 10 year period to reach the error margins for deriving trends, right?
You are correct in that the average RMS difference from RCM accumulation is 0.0475 to 0.0822 w.e. a^{-1}, although these differences are much larger in certain regions of the traverse (see Figure 9).

Our GPR accumulation trends are 0.009 ± 0.005 m w.e a^{-2} from 1996 to 2017, while RCM accumulation trends are 0.0016 to 0.003 m w.e a^{-2} larger than that. While these trends are an order of magnitude smaller than the RMS difference between GPR and RCM accumulation, we have shown both the validity of our measurements and their agreement with RCM trends. Therefore, we are confident that both our measured trends and RCM trends exist.

How is the vertical resolution limit of the 400MHz antenna calculated? For firn of roh_s=550 kg/m3 you would receive a v_mean of 0.2m/ns resulting in a wavelength of 0.5 m. Resolution limits are sometimes defined as half of the wavelength or 1/4*lambda. How do you come up with 0.35m?
The interface separation resolution is defined by the bandwidth, which controls the pulse duration, and not the center frequency (see Appendix C of Marshall and Koh., 2008, which is applicable to both FMCW and impulse radars). GPR systems usually have a bandwidth on the order of the center frequency. For a velocity of 0.2 m ns^{-1}, we can use the equation for range resolution, v/(2*bandwidth) = 25 cm. We could not distinguish separate features within less than 0.35 m in our radargrams, so we conservatively choose a resolution limit of 0.35 m.

You discuss several times errors introduced by percolating melt water. Heilig et al. (2018) measured the seasonal mass flux from snow into underlying firn at Raven to be at >50kg/m2 (in your preferred units >0.05m w.e.) for summer 2016. Can you clearly date back ice lenses or is the mentioned ice lens from 2003/04 a result of several melt seasons? What about summer 2012? Shouldn't there be a thicker ice lens arising from this melting event? How deep did water percolate within this summer season? I would expect at least a paragraph dealing with such uncertainties, apart from the given uncertainty of 0.5a for layer dating, which represents a strange value dealing with IRHs generated from end-of-melt-season surfaces.
We cannot be confident dating the ice lenses to a particular year, as meltwater typically percolates to depths greater than 1 m (Benson, 1962; Cox et al., 2015; Harper et al., 2012). The ice layer located within any given year may have been generated from that year or a following year. However, we can confidently date the surrounding snow, as the oxygen isotope and major ion signals remains relatively unperturbed (see Neff et al., 2012 – Journal of Glaciology; Avak et al., 2018 – Journal of Glaciology).
We have updated the text to "While meltwater percolation smooths the signal of some of these tracers, we can still confidently determine the depth-age curve using nearly-unperturbed oscillations in $\delta^{18}O$ and dust."
The ice lens from the 2012 event is likely thicker throughout the traverse than ice lenses from other summers. We can still confidently calculate SMB over 5 year periods from this method by analyzing the amount of mass between adjacent IRHs.
The 0.5 year uncertainty arises from dating the firn core using isotope and major ion chemistry, not from counting IRHs annual layers like Medley et al. (2013) or Koenig et al. (2016).

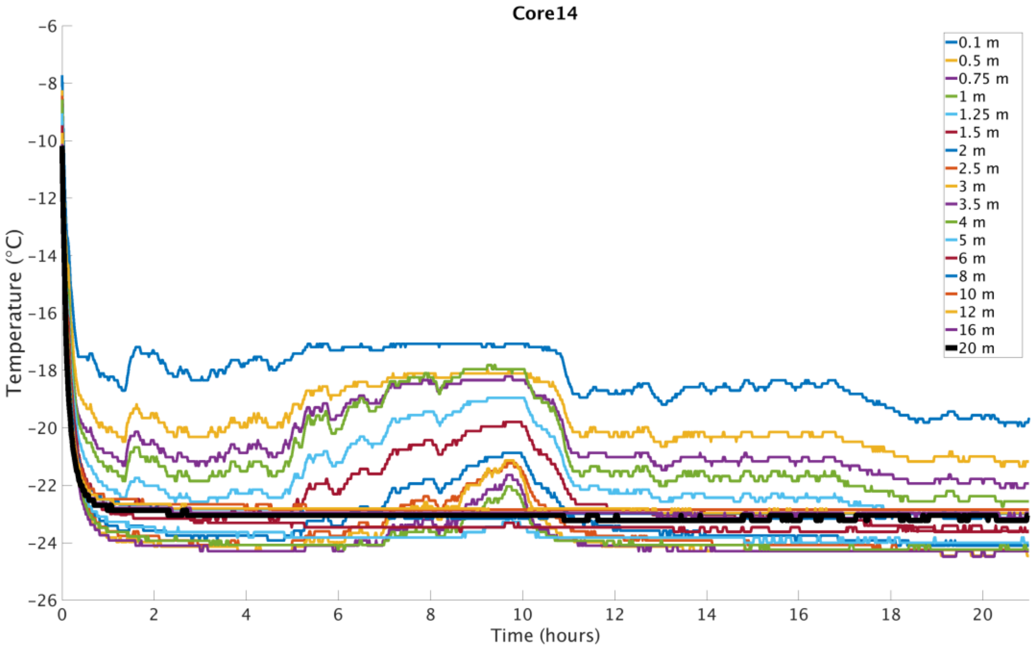
The layer picking remains a bit unclear. What happened for the 2011 IRH after Core 14? The indicated layer is almost horizontally flat, which certainly does not correspond to the layers underneath or above. Zooming in, I cannot follow the 2011 tracked reflection horizon. I would

certainly pick the IRH from 2014? or 2010? layer instead, which are much more prominent. Can you comment on this?
The resolution of this image is too low to clearly see the undulating IRHs along the 2011 layer. We have double checked the layer picks in Figure 2 and observed a small error in the 2011 layer. We have fixed that IRH and recalculated accumulation across that region, noting that none of the accumulation measurements change by more than 0.01 m w.e. a^{-1}. After reexamining the rest of our layer picks, we are confident that they are correct. Note that we will publish both our GPR data and layer picks with this manuscript so that others can verify our interpretation of the data.
This image serves as a subset of the traced IRHs from the entire 2017 traverse to highlight the high spatial resolution of our dataset. We purposefully traced these IRHs throughout the dataset rather than tracing specific prominent horizons for short distances.

Values in Section 2.2 are not correct. Here, you mixed up digits a bit. A RELATIVE DIELECTRIC (please consistently use this phrase) permittivity of 1.26 would correspond to a bulk density of rho_s=145kg/m3, which is certainly not the case for firn. Please correct accordingly and also correct the derived depth ranges.
Thank you for pointing out this error. We have corrected the usage to “relative dielectric permittivity” throughout the manuscript.
We have removed this sentence entirely as it is confusing to the reader. The derived depth ranges were not calculated using a constant relative dielectric permittivity, and are not affected by this error.

There are several parts, where I would like to see quantifications (e.g., L24, L132, L169, L475).
We have added quantifications to these locations to indicate the recent decrease in accumulation. The text now reads “…show decreasing accumulation and precipitation of 2.4 ± 1.5 % a^{-1}” and is easier to understand.

Thermistors in boreholes need to settle before they can provide reliable numbers. I can see that this is impossible for the field approach you chose but can you provide comparisons of thermistor with MODIS annual temps? You should at least mention difficulties of an open borehole for temp data.
Correct that borehole thermometry is usually conducted over periods longer than 24-48 hours. However, the thermistor at 20 m depth (thick black line on figure below) is able to asymptotically equilibrate within 24 hours to within ±0.1 °C and provides a temperature that we are confident can be used to drive a Herron-Langway density profile. Please see an example of the data from Core 14 below.
We added the following text to L225: “These measurements agree with MODIS satellite derived mean annual temperature (Hall et al., 2012) to within ±1 °C for each firn core location.”



Please revise the manuscript carefully for punctuation marks. I found numerous missing commas.

The manuscript has been revised for missing commas.