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Interactive comment on "Recent Precipitation Decrease Across the Western Greenland Ice Sheet Percolation Zone" *by* Gabriel Lewis et al.

Anonymous Referee #1

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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 espe-

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Discussion paper

cially 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. Major points of criticism are: âĂć 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. 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. âĂć 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. 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. 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? 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?). RMS values describing deviations from RCMs lack an explanation for the uncertainty range. 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. âĂć 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? 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? Max. single epoch errors of 79kg/m2/a are found for Herron-Langway comparisons with RMS deviations of 46kg/m2/a 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 0.5m. Resolution limits are sometimes defined as half of the wavelength or $\frac{1}{4}$*lambda. How do you come up with 0.35m? âĂć 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. âĂć 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? âĂć 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. âĂć There are several parts, where I would like to see quantifications (e.g., L24, L132, L169ff, L475ff) âĂć Thermistors in bore holes 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 bore hole for temp data. âĂć Please revise the manuscript carefully for punctuation marks. I found numerous missing commas.

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