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Interactive comment on "Long-term surface energy balance of the western Greenland ice sheet and the role of large-scale circulation variability" *by* Baojuan Huai et al.

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General comments

In this paper, Huai and coauthors use an energy balance model forced with automatic weather station data to compute surface energy fluxes along two transects in the west Greenland Ice Sheet. A detailed comparison and analysis of the surface energy fluxes is presented, with focus on differences due to elevation and latitude (e.g. different transect). Furthermore the differences between the two transects are put into a broader context using reanalysis products (ERA-Interim and ERA5) and a regional climate model (RACMO). The connection between Greenland Blocking Index

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(GBI) and North Atlantic Oscillation (NAO) and the 2 m air temperature and melt flux is discussed. Finally a validation of the reanalysis products and RACMO is given using the results from the in-situ energy balance modeling for both near-surface climate variables and surface energy fluxes. While there are previous studies addressing the surface energy balance along the K-transect (e.g. Van den Broeke et al., (2008) and Kuipers Munneke et al., (2018), using data from the same AWS as in this study), the present work provides an unique contribution to the scientific community with its spatial analysis including the T-transect and the GBI/NAO indexes discussion. The manuscript is generally well organized and adequately presented. I would like to make two main comments/suggestions to be considered prior to publication in The Cryosphere:

1. **Section 4.2:** SEB evaluation in ERA5, ERA-Interim and RACMO2.3: this is a unique feature of the paper, as already commented by another reviewer it is "one of the first validation studies of the ERA5 reanalysis over the Greenland Ice Sheet". Yet only a half a page paragraph is reserved for the presentation/discussion of this evaluation, with all the tables shown in the Supplementary Material. While I do understand that this is not the actual primary scope of the paper, I think that the manuscript and the readers would benefit from a more exhaustive discussion of the evaluation, maybe including a summary figure/table in the main text. Perhaps some of the lengthy descriptions of the surface energy fluxes (Section 4.1.2) could be shortened. Similar results and discussions can be already found in previous works (e.g. Kuipers Munneke et al., (2018)) while the evaluation of ERA5, ERA-Interim and RACMO surface energy fluxes is a novelty. Please don't misunderstand me here, I am not suggesting to completely change the scope of the paper or its structure but just maybe to revise the balance of the results section.

2. **Validation of RACMO melt flux:** this is probably the main problem the paper has in its current version. Nowhere in the manuscript I have found a validation of RACMO modeled melt flux, yet this variable is used extensively in the discussion

section. In line 449 the reader is referred to Noël et al., (2018), whose SEB evaluation at sites S5, S6, S9, and S10 includes the melt flux as well (e.g. Tables 2-5). Ultimately modeling melt accurately is one of the final goals of any surface energy balance model and regional climate model used to study ice sheets changes. An often proposed and used approach to this type of validation is to evaluate point studies (e.g. like the SEB modeling results here presented) against in-situ observations and then evaluate regional climate models against the point studies. The current manuscript already uses this framework for all the SEB components, but in my opinion it needs to include the same analysis for the melt flux as well.

Specific comments

L52-58: I think there are other studies in the literature worth mentioning that specifically address the surface energy balance on the Greenland Ice Sheet (e.g. Charalampidis et al., (2015), Vandecrux et al., (2018)).

L76-88: the goals of the paper are not clearly stated before the structure is outlined. This paragraph could be streamlined to make the actual goals more straightforward. E.g.: we study the SEB at two transects . . . we put these results into a broader context using these products . . . which are validated in this way . . .

Table 1: I wonder if here ELA is used instead of elevation, also are some of the weather stations discontinued now? Would it be possible to put the full operational periods? (e.g. Start Date - End Date).

Section 2.2.1: are the AWS data processed? E.g. is any correction applied to the datasets before being used as model input? If yes processing procedures should be described here or referenced appropriately.

L121: to my understanding "emitted longwave radiation" is not used to drive the model but in the model evaluation.

L123-124: "where temperature is recalculated to the reference height of 2 m using the SEB model” this procedure should be better explained or an appropriate reference given, if it’s important.

L127-133: what is the surface height measurement used for? For model evaluation, as stated below, but is it also used as precipitation input for the model? It would be good to state what this data is used for and then describe its limitations and corrections applied.

L134-139: the phrasing of this paragraph could be improved to better deliver the message.

Figure 2: is this figure really needed just to show the data availability period?

Section 2.2.2 and 2.2.3: maybe these two sections could be merged to improve the readability of the paper. Also the title should include the ERA-Interim product.

L185-186: convoluted sentence, readability could be improved.

L192: what does the author mean exactly with the surface value of the calculated subsurface heat flux?

L215-217: the process of Smeets and van den Broeke., (2008) could be better explained or simply skipped (e.g. . . . following the study of Smeets and van den Broeke., (2008) a value of . . .).

L226-233: how is the subsurface part of the model initialized?

Figure 4: any comment on the fact that it appears that modeled surface temperature is 0C much more often than the observed, this could mean that modeled melt is overestimated.

L251-252: I find this sentence a bit misleading, there is still information to be retrieved from 0C surface temperature (e.g. see previous comment about Figure 4), however it is true that the amount of melt cannot be assessed just by using melting surface temperature as a proxy.

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Figure 5: modeled data and observed data axis are inverted compared to Figure 4, this should be avoided at all cost. Be consistent with the chosen convention (e.g. modeled data always on the x-axis).

L275-279: such generalization should be avoided in my opinion when site characteristics vary so much. E.g. at S5 the radiation penetration effect on total cumulative melt flux is neglectable but what about at a much higher elevation site like S10 where the melt flux is much smaller? Section 4.1.1 and Figure 6: why there are no model values of surface height change for KANU and S10? Also doesn't the model simulate accumulation? Why are measured height changes compared to modelled ice melt in Figure 6? Also some of the dashed lines are not continuous (e.g. S6, KANL, . . .) why is this the case? A better explanation should be given here. (The nature of this comment is similar to my general comment about RACMO melt flux validation)

L309-311: reference about the cloud cover product used here?

Figure 8: I would rather put T2m and q2m on the same subplot (but different axis) since they are correlated rather than q2m with the wind.

Figure 11: this figure needs a bit of work from the reader to be fully understood. Providing additional descriptive text (e.g. at L470) to assist the reader would help. Also consider keeping the y-axis symmetric and with the same range. This would help in assessing the difference between different fluxes.

L512-515: convoluted sentence, readability could be improved.

Figure 12 and 13: missing subplot titles and colorbar labels.

References

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