The revisions made by the authors highly improved the manuscript. Most, if not all, the comments raised by the two reviewers have been addressed adequately making the paper mostly suitable for publication.
Despite this there is one aspect of my initial review which hasn't been completely addressed as I hoped. Being this one of the main issues I had with manuscript I feel the need to point out the concerns I have again. The aspect I am referring to is the point 2 "Validation of RACMO melt flux" of my initial review. For reference I am reporting here my initial comment as well as how it was addressed by the authors:

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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.

Reply:

(1) A validation of SEB model against in-situ ablation rate observations is presented in Figure 5 (Average 10-day modeled and observed ice melt). This only works for ice with known density, i.e. at KAN_L, KAN_M, S5, S6 and THU_L which are situated below the equilibrium line. See for the detailed descriptions Section 3.2 (SEB model evaluation) and discussion of Figure 5.

(2) We added a row in the evaluation tables in which RACMO2.3p2 melt rate is compared to that from the SEB model for station KAN_L, KAN_M, KAN_U, THU_L and THU_U. Previously, an extensive evaluation of RACO2.3p2 ablation rate with all available observations from Greenland S5, S6, S9 and S10 was performed by Noël et al., (2018) and also showed good agreement.

While the authors added the comparison of melt rates from RACMO and the SEB model (e.g. reply (2)), a proper validation of the SEB model at sites above the equilibrium line is not exhaustively addressed in my opinion. I do understand that this type validation is more challenging due to not knowing the density of snow and firn, as the authors properly comment later on in their reply to my review. Despite this, if I understand correctly, the SEB model used

has a subsurface module based on SOMARS (e.g. L241) which, if I am not wrong, should simulate, among other variables, snow and firn density. This can be used to compare observations of e.g. relative surface height change (which are recorded by AWS) with simulated values of melt, as it is done for sites below the equilibrium line. If for some reasons this is not possible and not presented in the manuscript, conclusions regarding these sites (e.g. accumulation zone of the ice sheet) should be presented in a more careful manner. The ablation zone and the accumulation zone highly differ in how they are affected by the surface processes described by SEB models and I think we cannot take for granted that models that work well in one region work well also in the other without proper ground truth validation.