The paper describes how a melt parametrisation affects surface melting on the Greenland Ice Sheet for different forcing scenarios. It is a simplified version of a previously publishes version of a melt scheme and uses temperature and insolation as inputs to calculate surface melt rates. This scheme is fast and simple (in terms of its input) and can therefore replace the positive-degree-day melt scheme which is the current melt scheme used in the numerical ice sheet model PISM. This paper is well written and it can be a valuable contribution for the ice sheet modelling community and should therefore, certainly find a home in TC. However, I cannot recommend this paper being published in its current form as it leaves some open questions that I feel need to be addressed first. The authors have put a lot of effort in the experimental setup but, I think, they almost tried to do too many things at once. I would recommend to them sorting out priorities of what experiments add to the story they want to tell and why, and how they then tell it. Therefore, it requires major revisions for which I have some, hopefully helpful, comments and suggestions.

Find below a list of major and minor comments that should be addressed by the authors.

Major Comments

- Abstract: the relative changes in surface melt don't mean much without a reference value to relate them to. What do you compare your ice loss with the new melt scheme with? I assume the default melt scheme for PISM is PDD, so why don't you compare your results against that?
- It is not easy to follow the results (Sect 3 and 4). The flow of the paper is interrupted by the need too flip pages back and forth too many time to find the respective experiment for the respective results (flip between Sect 2.5 and Sect 3/4). My suggestions: Present the experiments in order with their results in the appropriate (sub)sections. Provide a matrix of the experiments and what you are testing with those (in a table) and briefly describe that matrix in Sect 2.6.
- Sect 2.5: It's not clear to me what the calibration experiments are (2.5) What model parameters have been calibrated and what parametrisations have been tested?
- Sect 2.6: Reading on from the previous section, are these experiments now coupled and what is turned on and off?
- What the major uncertainties/limitations of your melt scheme? For example, cloud cover is not explexplicitlyictly considered here. Is that a problem and how much can the transmissivity parametrisation account for that? (Extreme) surface melt "events" as mentioned in the introduction are another example. Do they even exists in your forcing? For how much surface melt do they account for?
- I don't understand how PISM is used here, offline or interactive simulations, and which experiments are what? In the abstract you refer to "dynamic simulations" of the GIS, but I can't seem to find which of your experiments are coupled and which are uncoupled

- One aim of this paper is to provide a fast scheme for centennial- to millennial-scale time scales. I was just wondering, in case it has been coupled, it would be interesting for the readers to see what the long-term effect of using the new melt scheme over PDD might be.

- It would be interesting to see what is the relative importance of the parameters τ_α, c_1 and c_2 in Eq 1, (and σ In Eq 3, is that a parameter or calculated from the daily temperatures over a month?) in their contribution the melt rate M would be. Could you show how that partitioning between the temperature-driven and the insolation-driven melt looks like (a spatial map of sorts, or a stacked time series line plot)?

- For the RCPs the surface mass loss is expressed as SLE but the comparison with MAR is done as melt rates. I suggest you show the same quantity in Fig 1.

- I'm not fully convinced that the RCP scenarios are helpful for the conclusions. Wouldn't you want to show that the new melt scheme is better than the old scheme in a controllable way? I understand that you get a bigger signal with RCP8.5 but that doesn't seem to be the point here. For instance, let's assume that PDD is the better approach, than whatever you show with an alternative model doesn't really matter. Of course, I'm pretty convinced that your melt scheme is superior. That's why I want you to make sure that this is the point of the paper and that you have demonstrated it.

- In your current experimental setup, you can't be certain that the optimal parameters for the different linear fits, i.e., the individual terms as diagnosed from MAR (i.e., one for melt, one for transmissivity, etc, Fig. A1 and 2), would minimise your error with respect to the melt rates. I would therefore suggest that you do one single optimisation sweep. The other parameterisations are just means to combine different terms into a single equation (Eq. 1). For example, there is no point in minimising τ_α but would be still a useful diagnostic to check after your optimisation.

- The albedo-melt relationship (Fig. A1) and the transmissivity-altitude (Fig. A2) don't seem to be centred very much around the line that has been fitted. Furthermore, these relationships are not time-invariant, which means that the surface response differently to local climate, depending when and where it is. Maybe summer snowfall events play a role (as you said, they interrupt the whole melt process) and you can show that in the MAR data.

- A thorough uncertainty assessment of your free model parameters is necessary, and, in my view, this would add credibility to your melt scheme and your paper. This can be done by randomly sampling from a multi-variate distribution whose mean is given by the optimised parameter set and whose scaling is given by the standard error thereof. I'm not asking for a full ensemble of coupled PISM simulations (that would be perfect, of course) but to analyse the variable space more systematically, e.g., in the (T_{eff}, S_ϕ, M) hyper volume

- Summary statistics for fits (Fig. A1,A2) are missing (R^2, standard errors or confidence intervals of slope and intercepts, etc.)

– Is it possible to compare the melt rate with observational (AWS) data and use AWS data as input, if they are available at all? e.g., PROMICE, GEUS

Minor Comments

– P2/L13: ",~~where~~ when large parts"

– P2/L32: reference to PROMICE is missing

– P3/L26: What is the present-day reference period?

– P3/L29: "which ~~is~~ are modelled"

– P3/L32: How is the snowfall determined from rainfalls and near-surface temperature?

– P4/L1: Where does the "0.047" come from?

– Eq 1: the α in τ_α suggests link with albedo, maybe change the subscript to "a" or "A" for atmosphere

– P5/L8: add "(TOA)" after "top of the atmosphere"

– L15: It is unclear whether the cosine approximation is used here or the version by Berger (1978), which you refer to in A2. The Berger (1978) values are likely to differ from that present-day approximation.

– P5/LL1: I'm confused as to why the melt module is evaluated weekly if T_{eff} is monthly

– how is refreezing calculated?

– P5/L6: Can you explain why ice/snow can melt below freezing point?

– P5/L29 and P6/LL1: There is something wrong with this sentence

– P6/L3-5: I understand that "several iterations" mean that melt rate converges to some equilibrium value. Is that it?

– "Beckmann and Winckelmann" is a pre-print available for others to read? Otherwise, you need to explain the details.

– P8/L22 "as an~~d~~ example"

– L25/26 there is a duplication of "as described in Section 2.5"

– P11/L9 move the comma to after "Here" in "Here we analyse, how"

– Fig. 4: The caption doesn't say what the shading means (check the other figure captions, too)

– P13/L1,2: Too many "also"

– P16/L27: Add comma after ”In dEBM”

– P17/l1: remove ”classical”

– P18/L15: ”parameterized”