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Comment

Interactive comment on “Modeling surface response of the Greenland Ice Sheet to interglacial climate” by D. Rau and I. Rogozhina

Anonymous Referee #3

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Rau and Rogozhina modify a classical positive degree day (PDD) surface mass balance (SMB) modelling approach in order to improve simulated SMB for the ERA period (1958-2009 AD). By using a spatially variable temperature standard deviation (SD) in the PDD formulation, the match to results from a regional climate model with a physically based energy balance approach is improved compared to simulations with constant SD values.

My main concern with the manuscript is the limited scientific significance of the presented work. The important value of the PDD approach still is (after the advent of regional climate models like RACMO and MAR) its simplicity and applicability for long-term paleo studies. Selecting spatially variable SD values for the present day climatic and geometric configuration is equivalent to tuning the model for the present day and

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thus limiting it severely. Furthermore, I am not convinced the improvement compared to other PDD approaches is as clear as it appears in the manuscript.

Mayor points:

Introducing spatially variable SD values is ultimately equivalent to tuning the model to the present day climatic and geometric configuration. Since information of variable SD is limited to the recent past, the application of the model is reduced to that time period as well. The given SMB comparison strictly applies for the period 1958-2009. There is no reason to believe that the derived spatially variable SD values should apply in any other geometric or climatic configuration of the ice sheet and surrounding. The reference to "interglacial climate" in the title and other parts in the MS is therefore misleading and should be removed.

It is not clear to me why the authors do not at least mention the use of spatially dependent PDD factors (PDDFs). Arguably, PDDFs are the most important tuning factors of the PDD approach. Similar to SD there is no reason to assume PDDFs to be spatially or temporally constant. I think the authors should address how simulated SMB values would look like if spatially variable PDDFs and SD would be used together to make the analysis more complete. This would also limit the possibility of a coincidental good match with RACMO using variable SD values.

For the evaluation of the SMB model discussed in the MS all aspects of ice dynamics, geothermal heat flux, rheology etc. are completely irrelevant. So is the question on how to initialize the ice sheet model. For the comparison with RACMO or any other SMB model, the PDD models should be run on the same fixed present day ice sheet geometry (and same mask; see comment below). Any details about the ice sheet model aside from the SMB model proper should be removed from the MS.

An important conclusion of Vernon et al. (2013) is that using the same ice sheet mask is crucial for quantitative comparison between different model studies of SMB. This aspect has not been explicitly discussed in the presented MS, which leads me to be-

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lieve that identical masking has not been guaranteed in the comparisons. The authors should make sure masking issues are addressed and solved for the presented comparisons.

The new PDD approach is presented to better reproduce observations when in fact it is better matching another model, in this case RACMO.

I have strong doubts whether the experimental setup (Fig 4) allows for a "fair" comparison between the different PDD models. In other words, it is likely the improvement of including variable SD is over-estimated. Taking the given models (and most importantly their specific parameter settings) out of context of their paleo application and applying them for the present day may not be the best way of comparing them. For example, the model parameters may have been originally chosen as compromise to optimise both present day and LGM constraints and thus be sub-optimal for the present application. One could e.g. envision a retune to match present day total SMB and then compare the spatial patterns.

Minor points:

In many cases the order of references in brackets seems arbitrary. To my knowledge older citations should precede the newer ones unless there is a good reason for another choice.

I find the distinction between SEB and SMB models in 2705.20ff confusing since in my understanding both PDD based and SEB models are SMB models. This should be clarified.

In my understanding a PDD model (2706.18) "parameterizes surface melt rates of snow and ice" not only "as a function of the number of days a year when mean daily air temperatures rise above 0 C", but also *by how much* temperatures rise above 0 C. This should be clarified.

Authors should describe how the conversion to snowfall and rain (2707.8ff) is actually

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done.

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