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Discussions

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

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The following is a review of "Modeling surface response of the Greenland Ice Sheet to interglacial climate" By D. Rau and I. Rogozhina.

This manuscript is a brief communication describing a new surface mass balance parameterization, implemented within the SICOPOLIS Greenland, based on daily standard deviation of temperature. Standard deviations vary spatially over the ice sheet, and the characterization of this variation changes throughout the yearly cycle. The authors implement the new parameterization and compare results with regional-climate model-derived SMB and with other parameterizations available within SICOPOLIS. The authors show that when taking daily standard deviations of temperature into account, their estimates of SMB are greatly improved, especially spatially. The new parame-

terization yields total estimates of SMB from 1958-2001 that agree regionally with the RACMO2 SMB product. The investigating of SMB parameterization is an important piece of the current advancements in ice sheet modeling, and the authors present interesting results that highlight the need for methods better than the traditional PDD. However, there are a number of things that remain unclear about the methods used in this study, and the authors should address them before being recommended for publication in TC.

The biggest problem of this manuscript is the vagueness of its subject. For instance, this paper is about an exciting, new parameterization of SMB for Greenland using SD, and the title should reflect this. Similarly in the methods, the "Modeling Approach" section should be titled something that indicates that it is the key section that describes the new parameterization being introduced. Perhaps there should be a short section on the ice sheet model and then a section on the SMB parameterization. There should be a clear indication of which section is the one explaining the new method being introduced.

Below, I offer some more specific comments:

Page 2707: Line 1-9: This section should make it clear how the new approach was derived and why the assumptions (if any) are justified. I think what the authors are saying here, is that the SD is usually assumed constant over the ice sheet, but their method actually varies the SD with values derived from their analysis of ERA-40 temperatures. But the fact that I am not sure suggests that there should be clearer step-by-step explanation. For instance, in section 3.2, line 25-27, the authors should be able to reference the section that describe the methods for the new approach. These three lines in section 3.2 are not sufficient enough explanation.

Page 2707-2708 Section 2.2: There has been much discussion about the simulation setup and what biases might exist because of the spinup. In all honesty, this paper is not about what the ice sheet model does at all. It is about comparing the different SMB

parameterizations. In my opinion, the authors could solve this by holding the surface constant throughout the transient simulation. This is the only way to ensure that the SMB models are all being compared equally. Since the models are eventually evaluated against RACMO, it makes sense to just keep the ice sheet surface at the same elevation, since that is what the RACMO simulations do. RACMO and ECMWF both assume a fixed ice sheet surface, so allowing the surface to evolve only complicates the comparison between these products and all of the ice sheet model's SMB parameterizations. Perhaps comparing how the ice sheet surface evolves (dynamically?) due to the different forcings is another paper. But, for now, it is important to establish the differences in SMB only, in order to introduce the new method and establish its validity.

Page 2708 Line 9: Section 3.2 does not give details about the parameterization, though it does describe the spatially varying SD. There should be more details about the varying SD method in the methods section, so that it can be referenced in instances like this.

Page 2709 Line 25: As mentioned above, the description of the SMB parameterization based on SD should be first introduced in the methods, not in the results or discussion, and more detail is needed. The results of the analysis of SD, however, should be presented and then discussed here.

Page 2711 Section 3.3: This section is very confusing. RACMO is a regional climate model that provided SMB and its components, not ice flow or discharge. What is the product providing discharge? InSAR? Please make sure to properly cite the remote sensing and satellite data being used. It is not enough to cite Sasgen et al. 2012 because they use this data for analysis, the authors must make sure to give credit to the source of the data.

Page 2722: Fig. 4: A time series of the might be interesting to include as well, instead of just a comparison over the reference period. Regional climate model simulations of SMB suggest that there is significant interannual variability from 1958-2001, so it would

be interesting to see how the parameterizations compare (E.g. Is the skill of the new method better in large accumulation/melt years? Or average years?). Also, it is not clear why this timeperiod is considered as the reference period and why it is justifiable to assess the skill of the new method with averages over this period.