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Interactive comment on "Influence of albedo parameterization on surface mass balance in the perspective of Greenland ice sheet modelling in EC-Earth" *by* Michiel Helsen et al.

Anonymous Referee #3

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This study explores the simulation of Greenland surface mass balance (SMB) and ice sheet elevation in the EC-Earth model, associated with different snow albedo parameters. In particular, the parameters that are varied in this study determine maximum and minimum snow albedo, and the rates of albedo decay with time following snowfall in dry and wet snow. The study also determines a set of albedo parameters that lead to an optimal simulation of present-day ice sheet thickness, an exercise that is motivated by the fact that albedo has a strong impact on simulated SMB and therefore ice thickness. I commend the authors for preparing a very well-written manuscript and helpful discussion. I describe a few issues below, however, that I believe warrant further consideration before the paper should be published in TC.

Major issues:

(1) I appreciate that reasonable simulation of ice thickness is critical for an Earth System model, but I do not think SMB and ice thickness performance should be the only criteria used to evaluate snow albedo schemes. The authors identify other important processes that can strongly affect SMB, such as the refreezing of meltwater. This particular process is neglected in the current version of the model, so I am concerned that once this process is accounted for, the albedo scheme determined to be optimal in this study will no longer be optimal. The same argument applies for the improvement of other factors. For example, improvement of model snowfall will also affect simulated SMB and could, theoretically, alter the snow albedo parameters that produce the best ice thickness simulation. These points lead to the following suggestion: Simulated snow albedo from the different schemes should be evaluated against observed albedo. The authors provide a very rough comparison with ranges of albedo measurements on p.7 line 8, but I think a spatial evaluation against observations from a space-borne sensor like MODIS would be much more useful. Although it is interesting and important to know how the different albedo schemes affect simulated SMB and ice thickness, it is also critical, in my opinion, to know how the albedo scheme performs in terms of albedo.

(2) Related to (1): The determination of optimal albedo parameters seems flawed because, although these parameters produce the lowest RMSD in ice thickness (figure 8), they clearly do not produce the best simulation of SMB (in comparison with RACMO), as shown in Table 2. Thus, I am concerned that these albedo parameters produce the best simulation of ice thickness for the wrong reasons.

(3) The authors note (p. 6,21) that "Since the simulated climate is freely evolving after initialization, a direct comparison with observed time series is not meaningful." Could land-only simulations be performed, i.e., with fixed atmosphere conditions? Such simulations would enable a comparison of the albedo schemes that is much less affected by weather/climate variability, and would enable a meaningful comparison with observed

time series.

Minor comments:

p.3,1: "its interaction" -> "their interactions"

p.4,33: What is the justification for choosing -2K as the threshold for applying "wet" snow conditions? Are the results at all sensitive to this temperature choice?

Sections 2.2.1 and 2.2.2: The Stockholm and Copenhagen schemes seem unrealistic because they, respectively, neglect dry snow aging and prescribe sharp, instantaneous changes to albedo under wet conditions, e.g., when snow temperature exceeds -2K. I believe these experiments are still useful because they represent bounds, but I suggest acknowledging a bit more clearly that they should not be viewed as realistic albedo schemes and are instead used for boundary-case sensitivity studies. I would also note that abrupt binary changes in physical parameters are often undesirable in climate models because they can lead to instabilities.

p.6 31: "JJA mean pattern" - pattern of what? Please specify.

p.7,25: Why use the skin temperature to determine conditions for snow melting to occur? Is snow temperature not prognosed in the model?

p.8,24: "A side effect of the use of a time-evolving albedo scheme is an increase of interannual variability in melt..." - Yes, but this should be a more realistic feature! (I mention this because "side effect" often connotes an adverse unintended consequence).

p.9,28: "tje"

p.10,16: How are the (relatively short) SMB fields applied in the 25-ky runs? Are the multi-year SMB timeseries (only 25 years in length, I think) simply repeated? Or are they averaged and repeated annually?

p.10,17: "A uniform lapse rate of -7.4 K km-1 is applied." - Why is this lapse rate used? It seems steeper than values commonly used in other downscaling studies.

p.11,1: "downscaling the locally strongly negative SMB values are" - Please fix for clarity.

p.13,2-4: "The height - mass balance..." - This sentence (particularly the reference to "given back to the climate model") is somewhat unclear to me.

Figure 6: I suggest showing RMSD statistics for the SMB in this figure, as shown in figure 8 for ice thickness.

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