General comments

This paper consists in essence of two parts. First, it provides a model description, documenting the Explicit Snow (ES) scheme implemented in the version of the ORCHIDEE land surface model used in the IPCL-CM Earth System Model for IPCC AR6, and the changes made to apply the ES scheme to glaciers and ice sheets, also including an increase in its vertical resolution. Second, the enhanced version of the model, named ORCHIDEE-ICE, is applied to the computation of snow mass balance (SMB) and its components over the Greenland ice sheet, in experiments forced with input data from the regional MAR model. With default parameter settings, the model clearly overestimates the SMB compared to MAR, and the improvement of vertical resolution reduces this bias only slightly. Good agreement with the MAR results for the SMB components is achieved by tuning the snow albedo parameterization, but at the cost of underestimated surface albedo over Greenland.

Overall, this paper paves the way towards including ice-sheet surfaces in the land surface module of the IPSL-CM ESM. At the same time, it provides a cautionary story, as satisfactory simulation of SMB is only achieved when the snow surface albedo is biased low.

I think this paper is a useful contribution to the topic, and it is generally well written. Therefore I recommend publication of the paper in The Cryosphere subject to the relatively minor corrections/clarifications listed below.

Specific comments

1. lines 78–80: I think it would be worth mentioning, as an example of a relatively sophisticated GCM snow scheme, the Community Land Model (Lawrence et al. 2019) which includes the SNICAR scheme (Flanner and Zender 2005; 2006) and is employed at least in CESM and NorESM. This modelling system simulates prognostically snow density, grain size, liquid water and absorbing aerosols in a multi-layer snowpack, and computes snow albedo and absorption of solar radiation within the snowpack based on snow grain size and aerosol concentrations. Recently, SNICAR has been extented to also compute the albedo of glacier ice (Whicker et al. 2022), although (to my knowledge) it has not yet been coupled with a glacier model.

REFERENCES:

Flanner, M. G., and C. S. Zender (2005), Snowpack radiative heating: Influence on Tibetan Plateau climate, Geophys. Res. Lett., 32, L06501, doi:10.1029/2004GL022076.

Flanner, M. G., and C. S. Zender (2006), Linking snowpack microphysics and albedo evolution, J. Geophys. Res., 111, D12208, doi:10.1029/2005JD006834.

Lawrence, D. M., Fisher, R. A., Koven, C. D., Oleson, K. W., Swenson, S. C., Bonan, G., et al. (2019). The Community Land Model version 5: Description of new features, benchmarking, and impact of forcing uncertainty. Journal of Advances in Modeling Earth Systems, 11, 4245–4287. https://doi.org/10.1029/2018MS001583

Whicker, C. A., Flanner, M. G., Dang, C., Zender, C. S., Cook, J. M., and Gardner, A. S.: SNICAR-ADv4: a physically based radiative transfer model to represent the spectral albedo of glacier ice, The Cryosphere, 16, 1197–1220, https://doi.org/10.5194/tc-16-1197-2022, 2022.

2. lines 162–163. I'm puzzled about the sign convention here. Firstly, what is actually meant by saying that "G_{surf}" is computed negatively"? That G_{surf} is negative when the net energy flux is directed downwards, and positive when it is directed upwards? Second, judging by Eqs. (3) and (4), H_S and H_L are positive upwards. So if H_S and H_L increase, more energy is directed away from the surface... which means that according to Eq. (1), G_{surf} becomes more negative, which implies (according to line 163) that there is more warming of the surface?! This does not sound physically correct. Please, check and explain carefully the sign convention used in Eqs. (1–4). A graph showing which terms are positive upwards or downwards could be helpful.

3. Line 210: "δ_c is the critical value of solid precipitation necessary for resetting the snow age to zero". This is only roughly true. For very cold temperatures, setting $P = \delta_c$ results in τ_{snow} reduced by a factor of e, while for warm temperatures it may actually become negative.

4. Section 2.1.2: What is assumed about the vertical distribution of solar radiation absorbed by snow? And in ice (Section 2.2.2)? I'm getting the impression that all energy is deposited on the surface, and it is then distributed in snow only through diffusion, but it would be helpful to state this explicitly.

5. line 303: (Eq. 21): Does this indeed mean that the temperature of a snow layer only depends on the temperature of the layer above, and not on the temperature of the layer below? I would expect heat diffusion to work in both directions. The same question goes for ice (Eq. 27).

6. lines 394–395: “we also enhanced snow ageing by a factor of two in case of a rainfall event”. This sounds rather ad-hoc, and furthermore, it is not clear what it actually means. I think you should report, with an equation, how rainfall impacts snow age (τ_{snow}) in the model.

7. lines 444–445: “Reducing the root-mean-square error (RMSE) by $\sim 22\%$.” Which quantity are you referring to? The RMSE in albedo? Also, Rauolt et al. (2023) reported this number to be “over 25%”.

8. line 455: Which parameter(s) were the target in the optimization? Surface albedo or something else (SMB, runoff?). This seems to be said on line 605, but it should be reported already here.

9. lines 534–535: This should be the other way round: “$\sim 11\%$ lower and $\sim 35\%$ higher”.

10. lines 585–586. This sentence is not very clear. Does the implementation of ice layers make runoff smaller or larger?

11. lines 690: I would suggest to conclude this paragraph by saying explicitly (e.g.) that “Thus, the improved SMB simulation in OPT-12L is achieved through compensation of errors”.

12. In Table 3, consider also showing areal-mean biases.

13. line 705: “Metamorphism, dust . . . are ignored”. In fact, snow metamorphism and dust are included in the model, albeit in a crude manner, through the snow aging parameterization.

14. I strongly appreciate the existence of Appendix A. However, to make it easier to use, I recommend ordering the list alphabetically. Say, first the quantities with Greek letters, followed by the quantities in the ordinary (i.e., Latin) letters in alphabetic order.

Technical and language corrections

1. line 310: should ”required to” be ”available to”, ”used to”, ”consumed in” etc.?

2. line 341: The latter ”thickness” should be ”thicknesses”.

3. line 343: something missing before $\left(\delta_i, \frac{z_{snow}}{12}\right)$. “Min”?

4: Equations (24), (31) and (32): Consider changing the notation so that the melt terms (currently S_{melt} and I_{melt}) are replaced with e.g. M_{snow} and M_{ice}, to be consistent with the other terms in Eq. (32), which have phase indicated in the subindex.

5. As a follow-up comment on Eq. (32), according to Appendix A, the terms on the right hand side have different units: either ”m” (for P_{snow} and P_{rain}), ”kg $m^{-2}s^{-1}$” (for S_{snow}), and ”kg m^{-2}” for S_{melt} and I_{melt}. Everything cannot be correct.

6. lines 411: “De Ridder and Schayes, 1997” is missing from the reference list.

7. line 516: Add “(e)” at the end of the figure caption.

8. Some of the titles on top of Figs. 2–6 have the word ”differences” and some don’t. Please harmonize them.

9. Appendix A: For δ_c and P_{snow}, units are given as ”m”. This is ambiguous. Are they water-equivalent values or not? If yes, ”kg m^{-2}” would be better. For consistency, also for P_{rain}.