Responses to Reviewer 1

We would like to thank the anonymous reviewer 1 for taking the time to proofread and provide insightful comments on the manuscript. We will do our best to address all comments in the revised version so as to improve the overall quality of the manuscript in line with the reviewer's recommendations. Our responses are reported below in blue.

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.

Thank you very much for drawing our attention to the SNICAR model implemented in CESM. We will mention the modelling frameworks of Lawrence et al. (2019) and Flanner and Zender (2005,2006). We propose to change the sentence (lines 80-81 in the original version of the manuscript): *"However, due to their high computational cost, they are not used in ESMs, despite a few rare attempts (Punge et al., 2012)"* by: "*Due to their high computational cost, such detailed snowpack models are rarely incorporated in ESMs, despite a few exceptions. As an example, the SNICAR snow scheme (Flanner and Zender, 2005, 2006) implemented in the CESM model (Lawrence et al., 2019) simulates a variety of key snow processes, including metamorphism. This is a major advance when compared with the approach most often used in ESMs. However, SNICAR considers only 12 snow layers, whereas snow models implemented in regional climate models involve several tens of layers".*

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.

G_{surf} is positive when the net energy flux is directed downwards and negative when it is directed upwards. On the other hand, Hs and H_L are positive when they warm the atmosphere. As a result, when the turbulent heat fluxes are negative (i.e., warm the surface) while their absolute values increase, G_{surf} also increases. This is what is written in Eq. (1). We guess that the confusion comes from the sentence *"G_{surf} is computed negatively when it cools the atmosphere"*. We acknowledge that it is a bit puzzling. We hope that we will make the text clearer by replacing this sentence with *"G_{surf} is positive when it warms the soil"*.

3. 3. Line 210: "δc 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 = δc results in τ_{snow} reduced by a factor of e, while for warm temperatures it may actually become negative.

Although strictly a simplification, we believe this covers, in essence, what occurs in the model. Indeed, snow age is reduced by a factor 1/e at each time step. However, as the ORCHIDEE time step is 30 mn, snow age is almost zero in only a few time steps. For example, if τ_{snow} is set to 40 days before the snow fall event, it will be equal to 10^{-3} days in only 6 time steps (3 hours) as soon as $P_{snow} = \delta_c$. Moreover, we have to stress that τ_{snow} cannot become negative as surface temperature cannot be greater than 0°C over snow covered areas. Consequently, $g_{temp}(T_{surf})$ defined by Eq. (10) is always positive or zero, and so is snow age.

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.

We agree with this remark. Indeed, solar absorption is not accounted for in the ES model. We are aware that this is a crude simplification that may affect the accuracy of the model. However, as long as a more physically-based albedo scheme is not implemented in the model, we do think that this approximation is justified. In fact, solar absorption is highly dependent on snow optical properties which are themselves dependent on snow grain size. Since metamorphism is not explicitly represented in the model, we chose to ignore the absorption of solar energy in the snowpack. However, as specified in our responses to Reviewer 2, we are currently carrying out developments to implement a new snow spectral albedo model accounting for aerosols (light absorbing particles). However, this new model (Krishnakumar et al., 2024) is still under evaluation and is not available for this work. Its implementation in our ORCHIDEE-ICE model is the subject of very near future work.

Ignoring light absorption has impacts on albedo and therefore on the melting of snow and ice. This may be one of the main reasons why we have been compelled to reduce albedo. We will therefore follow the recommendations by stating explicitly in Section 2.1.2 that solar absorption in deep snow layers is not accounted for and by adding a comment on the related potential effect on the albedo.

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).

In equation (27), the temperature of layer *i+1* is a function of the temperature of layer *i* and the two coefficients C_{gr_snow} *and* D_{gr_snow}. These coefficients were themselves calculated using the same numerical scheme as that used to calculate the temperature in the soil and published by Wang et al. (2016, see Appendix A herein). This simplified writing actually hides a complex calculation (through the determination of the coefficients C_{gr_snow} *and* D_{gr_snow}) in which the temperature at the interface between two layers is calculated as a linear interpolation method according to the two nearest nodes (middle of the layers). Diffusion therefore takes place in both directions. This will be clarified in the revised version.

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.

You are right, this was not very clear. In fact, in case of rainfall, we simply increase the function f_{age} by a factor 2 ($f_{age} = f_{age}$ x 2). We agree that this is a completely ad-hoc parameterization. It has been introduced in the model to account for the effect of rain-on-snow events. Such events accelerate metamorphism and densification (Marshall et al., 1999), thereby lowering the albedo (Yang et al., 2023).

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

Indeed, the RMSE refers to the albedo, and there was a typo error. In the revised manuscript we will replace the sentence you are referring to by : *"In doing this, they also succeeded in improving the model-data fit over the whole between the ORCHIDEE albedo and MODIS retrievals by reducing the root-mean-square error (RMSE) by ~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.

The targets of the optimization are the SMB and its components (mainly runoff). This is achieved through the adjustment of albedo parameters in the range reported in Table 1 so as to lower the albedo, increase the runoff and decrease the SMB compared to the STD-3L experiment. To clarify this, we will change the sentence to which you refer to specify that the model performance is evaluated in terms of SMB and its components:

"Therefore, using the new ORCHIDEE-ICE model version, we adopted a manual tuning approach (i.e., trial and error method) to adjust the albedo parameters (OPT-12L experiment). This procedure consists of 1/ changing the parameter values, the new values being taken from the range reported in Table 1, 2/ running the model with the new parameter values, 3/ evaluating the model performance ***(in terms of SMB and its components)*** *using statistical criteria (e.g., RMSE* ***between MAR and ORCHIDEE-ICE****) and 4/ repeating steps 1/ to 3/ until an acceptable calibration is obtained".*

9. lines 534–535: This should be the other way round: "~ 11% lower and ~ 35% higher".

Yes, indeed. This will be corrected in the revised version.

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

We mean with lines 585-586 that accounting for ice layers makes the runoff larger. The sentence will be modified to make it clearer. However, we must also add that in the southern part of GrIS, the albedo computed in ASIM-12L is slightly higher compared to MAR and even MODIS, with the consequence of limiting the temperature and thus the runoff.

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”.

We will mention the compensation of errors in the revised version. However, we do think that albedo reduction can compensate for missing processes such as the lack of an explicit representation of metamorphism which decreases the albedo, the non-inclusion of the penetration of incident solar energy into the snowpack and the fact snow-atmosphere feedbacks are ignored.

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

We will include in the revised manuscript the new Table 3 with areal mean biases (see below):

Table 3 (revised): Albedo RMSE values (column 1) and areal mean bias (column2) for the MAR, STD-3L, STD-12L, ASIM-12L and OPT-12L experiments compared to MODIS. Columns 3 and 4: same as columns 1 and 2 respectively for the ORCHIDEE-ICE experiments compared to MAR. All values are averaged over the 2000-2017 period.

Experiments	RMSE (w.r.t MODIS)	Areal mean bias (w.r.t MODIS)	RMSE (w.r.t MAR)	Areal mean bias (w.r.t MAR)
MAR	0.076	- 0.005		
STD-3L	0.098	- 0.047	0.055	- 0.042
STD-12L	0.097	- 0.051	0.058	-0.047
OPTinit-12L	0.072	0.001	0.051	0.006
OPT-12L	0.111	- 0.008	0.092	- 0.047

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.

Yes, we agree. However, it would be more correct to say the effect of metamorphism on the albedo is accounted for through snow ageing parameterization. This will be specified in the revised version.

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.

Table 1 will be rearranged to make it easier to use. We will follow the alphabetical order as you suggest, but the parameters of the same parameterisation (e.g., snow viscosity, thermal conductivity will be listed jointly.

Technical and language corrections

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

This will be changed to “available to”

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

This will be corrected

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

Yes, you are right. “Min” will be added to the equation

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.

The terms will be replaced in the equations and in Table 1 accordingly

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.

Yes, this is a good remark. We have reported the units in Table 1 as they appear in the netcdf output files. Appropriate unit conversion has been done for the model results analysis. However, for more consistency, we will homogenize them in the main text, the equations and in Table 1.

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

The reference will be added to the reference list (see the complete references) at the end of our responses.

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

This was an omission. Thank you for this remark.

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

Figures 2 to 4 (original manuscript) represent the raw distributions (absolute values) of runoff, sublimation and refreezing. Conversely, Figures 5b to 5e display the SMB differences between ORCHIDEE and MAR, while Figure 5a is for the raw SMB distribution of MAR. We acknowledge that the titles of the figures may be confusing although everything is explained in the figure caption. To avoid any further confusion, we will split the figure into two parts: Raw distribution for the MAR SMB on one hand and SMB differences. The problem also arises for Figures 6 and 8 as Fig. 6a and Fig. 8a display the MAR and MODIS albedo respectively while the other panels represent the differences between the simulated albedo and the MAR albedo (Figs 6b-6e) or between the simulated albedo (MAR and ORCHIDEE-ICE) and the MODIS albedo (Figs 8b-8f).

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}.

Yes, these are water equivalent values. All the units will be homogenized in Table 1 and in the main text (see also our response to your comment numbered 5).

References:

De Ridder, K, and Schayes G: The IAGL land surface model: Journal of Applied Meteorology, 36, 167-182, doi:10.1086/451461, 1997.

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Marshall, H.P., Conway, H., Rasmussen, L.A.: Snow densification during rain, Cold Regions Science and Technology, 30, 35-41, doi: 10.1016/S0165-232X(99)00011-7, 1999.

Wang, F., Cheruy, F., and Dufresne, J.-L.: The improvement of soil thermodynamics and its effects on land surface meteorology in the IPSL climate model, Geosci. Model Dev., 9, 363–381, https://doi.org/10.5194/gmd-9-363-2016, 2016.

Yang, Z., Chen, R., Liu, Y., Zhao, Y., Liu, Z., & Liu, J. (2023). The impact of rain-on-snow events on the snowmelt process: A field study, Hydrological Processes, 37(11), e15019; doi: 10.1002/hyp.15019, 2023.