Responses to Reviewer #2

The authors present results of evaluation tests of a new tile within the ecLAND module of the IFS-code of the ECMWF. I have read this paper with interest - the paper is generally well written - but needs improvement in the depth of the evaluation. The authors now convincingly show that the code update is an improvement. However, it would be good if the authors show also the performance against existing models, to see if the new tile/module performs similar as those models, or that there is still room for further improvement.

> This paper focuses on describing the improvements to ecLand, which is part of the IFS/ERA6 system, widely used as a global dataset. While an additional comparison to other models would be of interest, it is beyond the context of this manuscript. Our aim here is not to compare ecLand against other models, but rather to improve the representation of snow and glaciers within the coupled system for Numerical Weather Prediction (NWP) and reanalyses applications and therefore show an improvement to the overall model performance. We acknowledge that there remains room for improvement from a snow and ice perspective. However, because ecLand is part of an operational forecasting model, any significant changes to augment the realism of the snow/ice schemes may degrade NWP skills. For this reason, comparisons with other models, though potentially informative, would not add substantial value given the specific goals pursued here.

The essential improvements are those in the evaluation of the skin temperature, and especially of those of the subsurface temperature and to add an evaluation of in situ surface mass balance. I would genuinely like to see the technical improvements that I think are essential for better representation of the SEB and SMB without much coding - at the other hand I am afraid this request/wish will be in vain.

> As indicated above, the fact that ecLand is part of an operational model precludes such significant changes to the model at any one time. While we agree with the reviewer that the SEB and SMB could be improved, the impact on the NWP model, as well as future reanalyses would potentially be unforeseeable and must be carefully evaluated.

Comments

Introduction: glaciers and ice sheets are two different things, so by using "glaciers", ice sheets are sometimes forgotten. Therefore, I think it is better to replace "glaciers" of lines 24, 32, 39, 106 to "glaciers and ice sheets", "glaciers" of lines 30, 94, 97, 100, 107 to "glaciated surfaces". L 108 is a special case, I leave it to the authors to make it more general. And check other instances when I have missed the word "glacier" in this list.

> Thanks. We have amended the text throughout to make this aspect more precise.

L 34: Greenland Ice Sheet - so with capitals.

Thanks, done.

L 38: Don't forget that for ice sheet, Earth System Models (like CESM2, UKESM) perform increasingly well (if the ESM developers try to model the SMB well), so regional climate models are not the only alternative of using observations only. These things are discussed later in the introduction too, so this formulation is already somewhat inconsequent.

Thanks, we have amended the text to better reflect this aspect.

L 43-54: I don't think this sidestep to ice dynamics is relevant for this manuscript nor introduction. For example, the initial-condition problem of ice sheet models is completely different to those of atmospheric models. I propose to remove this paragraph.

In hindsight, we agree with the reviewer's comment and have removed this paragraph in the revised manuscript.

L 48: I think this reference if outdated, take a newer one if you decide to retain this paragraph.

This paragraph has been removed following reviewer's previous suggestion.

L 65: A regional climate model is not an Earth System model. So, HARMONIE-AROME improvement are not an example of ESM improvements.

Thanks, we have rephrased this sentence as follows: *"Efforts to improve the representation of ice sheets and glaciers in NWP models have been relatively limited compared to the advances in climate models. Mottram et al. (2017) improved the representation of melting events in the HARMONIE-AROME regional model for NWP applications, by including an upper threshold on the surface temperature of the ice surface (i.e. melting point), using the remaining energy to melt the snowpack."*

L 74-82: Given that the land-ice parameterization of ecLand is simple and uses very few layers compared to the advanced schemes in, particularly, polar adapted Regional Climate Models, I would like that the authors formulate very specifically what their new parameterization should do well, and what not necessarily. For example, is that the SEB (including albedo evolution) for the typical range of a weather forecast (14-21 days), on seasonal timescales; does it include the surface mass balance (including refreezing effects)?

We thank the reviewer for this comment. We have clarified the scope and purpose of the new scheme in the introduction. Also, we have added a table in the Methodology section to highlight the main changes and process improvements in the new scheme compared to the old one (see response to Reviewer #1 main comment).

L 94: As far as I can recall, this simple snow scheme has been used also in earlier IFS cycle. If so, replace by "In CY49R1 (the NWP ....2024) and preceding versions since version <first version with this code>, glaciers and ice sheets are ...."

Glaciated surfaces have been simulated using the same formulation for many cycles (decades, possibly) of the IFS. We have clarified this sentence adding “preceding versions” as suggested by the reviewer.

L 135: This method of confining the skin temperature to the melting point is already long in the IFS code even before Arduini 2019. Still, I remain to the opinion that this workaround is a poor solution, given that one can easily solve this issue in a mathematically sound and numerical simple way. We have used previous IFS versions (e.g. CY33R1) and in those versions considerable errors arose. Specifically, the time step after observing melt, the initial skin temperature guess was below the freezing point, the large conductivity number was not used, and the actual skin temperature became again well above the melting point. Even if this problem does not arise in this version, the method of Arduini leads to inconsistent skin temperatures if the uppermost snow or ice layer is not at the melting point.

Our solution was and is the following: When one observes that the skin temperature of a snow/ice/glacier tile is above the melting point, we keep the skin conductivity as is, but (simply) apply that one thus knows that the skin temperatures of that tile is at the melting point. If those tiles cover the whole grid box, one only need recalculate the fluxes. If these melting surface cover only a part of the grid box, we solve the SEB again, applying that we know the skin temperature of these melting snow/ice tiles, while the other tiles remain unknown as in the normal linearized SEB solve method. The authors can have more extensive documentation of our approach if they wish to have.

What the reviewer is referring to in the first part of their comment is a different method, not the one described in Arduini et al. 2019. As far as we know, the method has been documented and tested in coupled simulations in Arduini et al 2019, but we are happy to include any other reference, pointing to this specific implementation for offline and coupled simulations, that we are not aware of. The method described in the second part of their comment is very similar to what we have implemented in IFS, with the difference that the surface temperature over snow/ice tiles is constrained by setting the temperature of the underlying surface to the melting point and using a large skin conductivity. For clarity, the reviewer can have a look at the offline implementation in ecLand, around the LREPEAT switch: https://github.com/ecmwf-ifs/ecland/blob/main/src/surf/offline/driver/vdfdifh1s.F90

More generally, when indicating an IFS cycle or reference, we refer to the cycle in which codes get operational and used for Numerical Weather Prediction, rather than research branches in which code was added, but not active, under a logical switch.

Section2.2.2:

To me, this decision to not create separate prognostic variables for glaciated variables is a very, very poor compromise. For the glacial ice it is acceptable - sea ice is not active over land - but for snow I really do not see the urgency to mess up your snow physics to safe 4x4 prognostic variables (layer mass/thickness, layer density, layer temperature, layer water content). Snow surfaces, and particularly the snow albedo, is a classic example of a non-linear process, so the aggregate of two implies that now both are wrong.

We have no longer the computers of the 70s for which fast memory was a severe limiting factor and is the fancy type system of IFS not specifically set up to allow for adding new variables without having to adjust the code from top-to-bottom? It sounds like that the authors were allowed to play around and improve the representation of glaciated surfaces; under the condition they won't bother the rest of the ECMWF-IFS community in any way. Don't get me wrong, I applaud the efforts of the authors to improve the representation of glaciated surfaces, but this compromise is very typical of the general and decades-long neglect of glaciers and ice sheet surfaces by the ECMWF.

Ideally, this poor compromise is rectified, and glacial snow is separated from land snow (and if you are doing that, please also separate snow over low vegetation and the snow below high vegetation into two independent snow layers sets, as that mix-up is equally bad). But I also do understand this strong suggestion (separate variables) is infeasible to effectuate (now), and I am aware this rant leads to zero change on the code or paper. However, hopefully it encourages the authors (or their successors) to fight even harder for a proper representation of glaciated surfaces in the IFC code. In all cases, I would like to see a longer motivation in the rebuttal why this poor compromise has been taken.

We thank the reviewer for this detailed and interesting comment. The decision of not including additional prognostic variables was motivated by the fact that the scientific improvements were foreseen limited, relative to the substantial technical effort required to implement and validate them correctly. We expand on these two elements in the following.

Firstly, in response to the comment that "the aggregate of two implies that now both are wrong", we acknowledge that having two different prognostic variables is, in principle, the best solution. However, in the present work we are looking at the difference with the current model formulation. In this respect, the evaluation of albedo did not give any indication of degradation for the heterogeneous grid-points around the coast of Greenland. The evaluation of river discharge, an integrated measure of snowmelt over a river basin, was also showing improvements in most of the considered basins in the Northern Hemisphere. The latter is quite significant, as an erroneous snow albedo, both over "land" and land-ice tiles, would very likely lead to an erroneous snowmelt and therefore a degradation in skills in the simulation of the river discharge.

Secondly, although weather forecasting is not the primary focus of this study, it is central to ECMWF applications. From that perspective, the new scheme yields approximately an 8% improvement in 2-metre temperature forecasts errors around the Antarctic coast during summer, suggesting an enhanced representation of the surface energy balance. In addition to that, for weather prediction applications we believe that model complexity should always be balanced between the different earth system components. Therefore, it is not immediate that additional complexity, like five additional prognostic variables, would lead to better results.

We agree with the reviewer that computational resources have advanced significantly since the 1970s, and memory limitations are now a less critical constraint. However, the complexity of modern code bases has also increased substantially, such that introducing a new component requires many changes in several components, from initialisation to data assimilation and archiving, to avoid undesired effects. From a technical and scientific perspective there are several challenges that should be considered that we believe require dedicated future work:

- in an operational system for NWP, the additional prognostic fields should be correctly initialised to avoid spin-up issues in the subsequent forecast or in a hindcast setup to calibrate seasonal and sub-seasonal predictions.
- Usage of additional prognostic variables for snow tiles should be evaluated in the 4D Variational Assimilation (4D-Var) system, in particular for the observation operators making use of the snow variables (e.g. microwave observation operators).
- The new variables would require additional GRIB2 codes that must be proposed to WMO and approved, before the fields can be disseminated.

We can reassure the reviewer that our intention is to push the boundaries of snow/glacier modelling within the IFS. However, as within all operational centres, such changes need to be implemented gradually.

L 168: Please specify what Tsn is. It could be the temperature of the uppermost snow layer or the glaciated surface skin temperature - I don't know now.
I presume the authors are aware that this snow albedo (Eq. 3) includes two major simplifications. Firstly, the snow albedo is not a function of temperature (or density-per sé), but of snow grain size (and to some extend to the solar zenith angle). Secondly, the snow albedo is very strongly dependent on the wavelength, almost always 1 for UV to yellow; strongly varying for "red" and zero-ish for near-infra red - see e.g. Gardner and Sharp, GRL, 2010, or Van Dalum et al, The Cryosphere, 2020). So, using one albedo value number is similarly bad as running ecRAD with only one G-band. Again, I know it is not realistic to expect the authors to use a state-of-the-art grain-size based snow albedo scheme, but it is good to mention in the manuscript that this albedo is not regarded as

state-of-the-art and specify its limitations with a reference to Gardner and Sharp, GRL, 2010 or a similar paper.

> We thank the reviewer for this comment. We have amended the text to clarify the limitations of the implemented snow albedo scheme with the relevant references. The update of the snow albedo scheme is in our future plans and will require dedicated work. Following what we have said in the previous response, we believe that complexity *per se* might be detrimental in a coupled model due to compensating biases between different processes. Therefore, implementation of more complex schemes based on snow grain size (or a proxy of it) and using several spectral bands will require a careful implementation and retuning of other model components.

Section 2.4: It doesn't become completely clear to me how the experiments are carried out. This holds for both the point-scale as 2D global simulations. I understood from the description that the land model is rerun, but the atmospheric model not. Whether I got it right or not, explain in more detail which parts of the code have been rerun and which not. Furthermore, please specify which fields/fluxes are updated/adjusted in the experiments, and which fields/fluxes were kept constant. I conclude from the paper that the SEB has been recomputed, but I don't understand how that is done as that is far from trivial to do afterwards online within the IFS framework (e.g. one needs the derivatives of fluxes to close the SEB and I would be seriously surprised as these derivatives are available from the ERA5 simulation.).

> We thank the reviewer for this comment. The land component of the IFS, ecLand, is fully externalised and can be run “offline” forced with atmospheric fluxes and state variables as boundary conditions with a certain forcing frequency. In this configuration, the forcing variables are liquid and solid precipitation, downwelling longwave and shortwave radiation; wind speed, temperature and specific humidity at the lowest model level; surface pressure. The forcing variables are kept constant if the forcing frequency is greater than the model’s time step. For these experiments, the forcing frequency is 1 hour, and the model’s time step is 30 minutes. With these inputs the SEB is solved using the implicit method described in Best et al. (2004), providing the required fluxes to run the land-surface components. The caveat, as in all land-surface only simulations, is that there is no feedback to the atmosphere due to changes in the land variables (e.g. snow). We have amended the text adding more details on the simulation setup in Sect. 2.4 and summarised experimental settings in Table S1 (as supplementary material).

Figure 1 & 8: please use a map projection that shows Greenland with the right width to length ratio, so either Lambertian, Polar Stereographic or rotated lat-lon.

> Thanks, done.

L 175: From the PROMICE dataset, the authors use the skin temperature. However, this temperature is not measured. I presume that the authors use the temperature derived from the upwelling longwave radiation. Please specify that explicitly here.

> Thanks, the temperature is derived from the upwelling longwave radiation. We have amended the text to clarify this aspect.

L 220: Make this description of the CLIM data more specific. Fgl is thus not 1 - otherwise that would have been stated - but still it would be sensible to use the tiled skin temperature for Figure 2-4. State which values have been used - grid-box average skin temperature or tile skin temperature. From line 275 I conclude that grid box averaged skin temperatures are used, it is better to adjust this - as the paper is about evaluating the glacier tile and the observations are on the ice sheet. So, why is not the tiled skin temperature analyzed? This parameter can be exported, so practical reasons are not impeding this.

> We guess the reviewer is referring to lines 223-225, describing the “E5-CLIM” experiments, not line 220 describing the “E5” experiments. Regarding the “E5-CLIM” experiments, the purpose of the experiment was to evaluate the schemes in a sort of “operational” setting when the global model is run using a fractional glacier mask. With this purpose, it makes sense to evaluate the grid-box average skin temperature, as this is what would be passed to the atmospheric model in coupled simulations. We have clarified this aspect in the revised version of the manuscript.

I like the analysis in Figures 2-4 - it is a good method to show what the new module can and cannot. The drawback is that it is hard to compare how well this new module performs compared to existing glacier surface descriptions, as I haven't seen it in other papers. Therefore, assuming that the PROMICE skin temperature is derived from the upwelling longwave radiation, the golden standard is the modelled skin temperature with a SEB model (e.g. https://doi.org/10.1017/jog.2024.68). It would be good add the performance of such a dataset, to compare it against the CTL-OBS and GLA-OBS results. Similarly, the "E5" and "E5-CLIM" could be compared of the performance of a polar adapted RCM like MAR (https://doi.org/10.5194/tc-14-957-2020) or RACMO (https://doi.org/10.5194/tc-12-811-2018 - although this dataset is outdated). I am quite sure the required data for such an analysis is available for the authors to be used.

> We thank the reviewer for this comment and the suggested references. Figures 2 to 4 already compares the surface temperature resulting from the SEB to the one diagnosed from the observed upwelling longwave radiation. We agree that a comparison with other models could provide additional insights for the reader. However, such an analysis lies beyond the scope of the present study, which is specifically focused on evaluating the new module against the current implementation and showing the improvement for a globally used model. A fair and meaningful comparison with a Regional Climate Model, as suggested, would require a dedicated experimental design and a separate study, which would be interesting to consider in the future.

L 252-260: The interpretation of Figure 3 is complicated without analysing the SEB and the T2m. An T2m temperature analysis could indicate if the atmospheric conditions of the E5 and E5-CLIM simulations are colder/warmer than those in the observations. Similarly the SEB analysis (against the PROMICE data) could indicate why the conditions are colder than observed. These figures don't need to be added to the manuscript, but such an analysis add depth to paragraph, which is now not much more than "Hmmm, our model is warmer/colder than observed." In that respect, the 3K warm winter bias of both OBS is remarkable, as for this experiment the 2m is "correct". In short, figure out why the new and old model deviate, and report that in the revised manuscript.

> We thank the reviewer for this comment. We have looked at the Surface energy balance components to diagnose how the other energy fluxes respond to the changes in the ice and snow parameterisations. With regards to Figure 3, the differences in the wintertime bias between the OBS and ERA5 experiments are mainly attributable to downwelling longwave radiation ("LWdown"), which is systematically higher in the observations than in ERA5 (see below for the accumulation sites). The weaker LWdown forcing in ERA5 leads to lower surface temperatures in the E5 experiments, which in turn explains why CTL-ERA5 and GLA-ERA5 exhibit a smaller bias compared to CTL-OBS and GLA-OBS. Importantly, lower surface temperatures increase the surface–air temperature gradient, thereby enhancing the sensible heat flux ("Qh") in the ERA5 experiments. It is well known in the literature that turbulent mixing tends to be overestimated in ecLand/IFS, which helps to explain why the E5 experiments still display a positive wintertime bias despite the underestimated LWdown relative to the observations.
>
> We have included this discussion in the revised manuscript, see Sect. 3.1.1 and have amended the text to provide more physical understanding on the causes of the differences in Figure 3 and 4 between CTL and GLA; we have added the Figures of the mean annual climatology and diurnal cycle of fluxes as supplementary material of the revised manuscript.

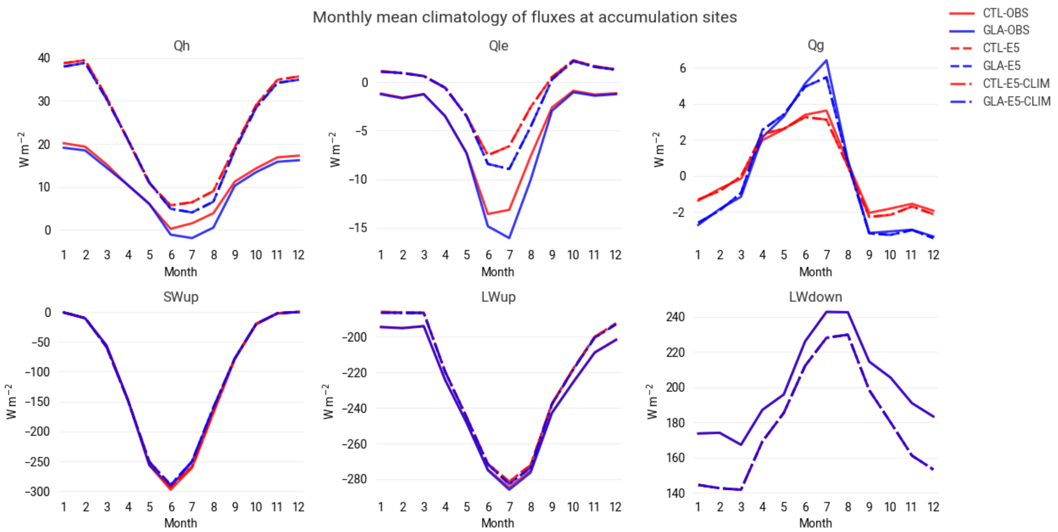


Figure 4: add in the caption that summer months are evaluated.

Thanks, we have clarified that June-July-August (JJA) are evaluated.

Concerning Figure 3-4, to which extend are the difference due to elevation difference between the observational site and the height of the grid box? Elevation biases induce temperature biases which have nothing to do with poor functioning parameterizations. Remove the effect of an elevation bias (if present).

We have computed the lapse-rate correction at each site to separate errors due to elevation differences from those due to forcing uncertainties. The average lapse rate for the “low”, “upper” and “accumulation” sites is 0.29 °C, 0.15 °C, -0.13 °C, respectively. For this reason, we have chosen not to modify the plots, but we now explicitly mention these results in the revised text for clarity.

Concerning Figure 4, again I would like to challenge authors to dive a bit more deeper into the 'why' the model is deviating from the observations. Very little physical explanations are given, and the manuscript (and your understanding of the model performance) will be improved if this indepth analysis is made. Again, the avenue to get this insight is through analysing the SEB, and again these figures don't have to be added to the manuscript (possibly supplementary materials), but allows for a more physical explanation why the model is deviating. I would guess that the underestimated cycle for the high locations is due to too high effective thermal capacity of the snow layer (being 50 cm thick), while alternatively missed nighttime refreezing (normally dampening cooling of the surface during the night) is the cause of deviations for the lower and upper ablation sites.

We have responded to this point in the previous comment regarding “L 252-260". As responded in that comment, we have included a discussion on the changes to the surface energy fluxes to have a better physical understanding of the differences in the revised manuscript in Sect. 3.1.1.

Section 3.1.2: Snow temperature

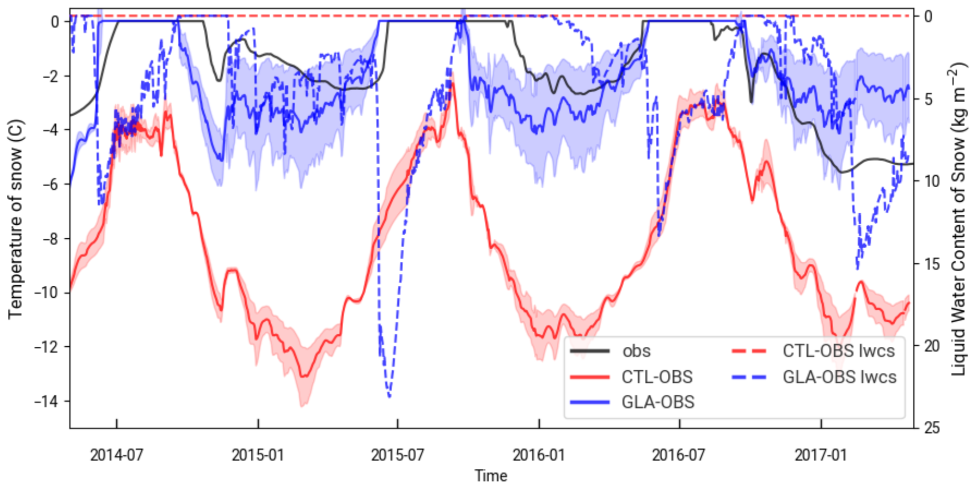
Snow temperature is a very good indicator of the performance of the model, and it is a very good idea to analyse and discuss this here. However, measuring snow temperature is in some sense trivial but using it is very complicated. With thermistor strings you can easily measure the snow temperature on a give location of the snow, either below or initially above the surface, depending how the string is installed. Given this installation, the temperature sensor moves with the snow pack in which is is burried, or stays at a given height above or below the measurement frame (like an Automated Weather Station) it is attached to. In all cases, the actual snow surface is moving away and towards the sensor all the time, so a sensor is never all the time at, say, 1 m depth below the surface. Compared to the data available online at the PROMICE website, the shown observational curve equals to those of "sensor 1" - although the online dataset has considerable datagaps in the summers of 2016 and 2018. The Fausto paper indeed state that this sensor is/was at 1 meter depth, but winter accumulation and summer snow melt are both well over 1 meter at TAS-A, so it could be - well, has been - anything like 0 to 2 meters. Given that sensor 2 has positive values in the summer of 2016 (after which the snow temperature sensors are reinstalled, visible in the shift in all readings), that happened for sensor 1 as well.

I really would like to see that the authors can retain this analysis, but that does require that they reconstruct the actual depth below the surface of "sensor 1" using the observed surface height - and not only for TAS A but for all stations used in Figure 5. After reconstructing the actual depth of the sensors, the adjacent model temperature can be extracted from the model data and compared to the observations.

If this is not possible, another way to assess the subsurface temperature is to replicate the evalution as provided by e.g. https://tc.copernicus.org/articles/15/1823/2021/.

And if this analysis is retained and improved, then it would be great if the results could be compared with subsurface temperatures from what is considered advanced surface models for either (or both) SEB models and or polar regional climate models, similar as requested for Figure 2.

> We have followed the suggestion and used the sensor height measurements relative to the snow surface to diagnose the actual depth of the temperature sensors. This depth can vary substantially due to snow accumulation, reaching up to 3 m during the winter months. Accounting for this variability significantly improves the agreement of the GLA experiment with the observations (see below). Previously, the model tended to be too cold in winter because it considered temperatures from a snow layer closer to the surface, which is more strongly influenced by the colder atmosphere above. We have included the new figure in the revised manuscript (new Fig. 6, see below) and updated the text accordingly in Sect. 3.1.2. Even though interesting, we believe the comparison with SEB models regional climate models is beyond the scope of the current work and should be explored in future work.



L 287 & Figure 5: State explicitly that this Taylor diagram and bias on annual data.

Thanks, done.

Section 3.1.3 Trend in melting occurrences. Personally, I am more interested in the model performance than in the trend, as the latter has been documented in many papers already. So focus more on the statistics - in how many cases are the melting days well predicted, missed and are there many false alarms (and how does this compare to other models) - and remove the trendline.

A statistical comparison using a skill score (e.g., the threat score) is already presented in Figure 9 of the original manuscript. The purpose of Figure 7 is to evaluate how the current and new modules reproduce trends in melting occurrences, which is an important feature to assess for a model which is intended to be used for reanalysis products (e.g. future ERA-Land datasets). We have revised the text in Sect. 3.1.3 to clarify this scope.

Figure 8: please replace panels b and d by panels with the biases compared to the observations. A reader should be able to eyeball the improvement compared to CTL, but I don't want to eyeball how far off the GLA is from the observations, which is the current situation. And please be aware of the issues with the MODIS albedo if the solar zenith angle is larger than 70 degrees - which also may explain the positive bias in North Greenland in panel 8c.

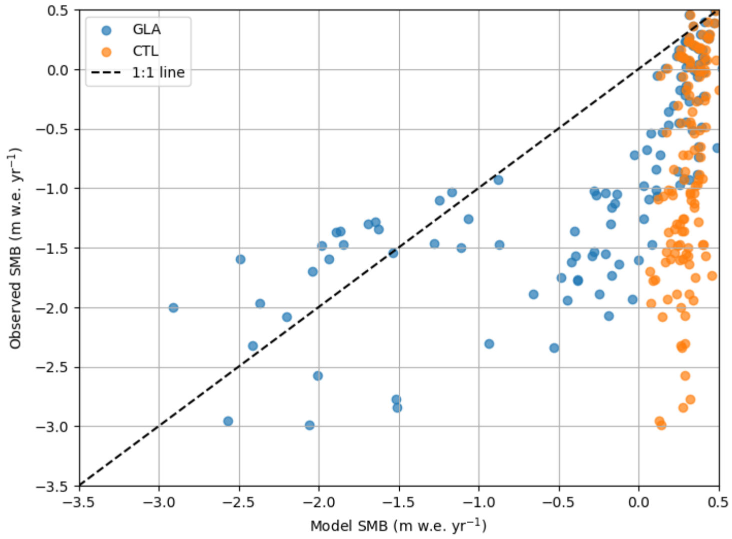
The purpose of our analysis is to highlight the improvement or degradation of the new model relative to the control experiment. For this reason, we consider it more appropriate to present the absolute bias difference, as this illustrates the regions where the new scheme improves or degrades albedo and melting occurrences. We also appreciate the comment regarding MODIS albedo uncertainty and have now included this point in the manuscript.

L 338: I'm fine if you retain the reference to Zorzetto, but please be aware such parametrizations are already used for over a decade in polar adapted RCM like RACMO and MAR, the latter through the snow model CROCUS. Acknowledge that with appropriate references.

We have added the recent reference to Zorzetto as it was a recent example of a global model using a more physical and complex albedo parameterisation. For more completeness, we have added a reference to CROCUS in the revised manuscript.

Section 3.2.2. This analysis is useful, but a common practice in other papers is to compare modelled SMB against the in-situ observations compilation from Machguth (J Glac 62, 2016, currently being updated by DMI) like done in Noël et al, 2019, Sci Adv. (Supplementary figure 3) or a subset of that like van https://tc.copernicus.org/articles/15/1823/2021/.

We agree with the reviewer that a comparison with in situ observations of SMB would be valuable. However, this would require a higher horizontal resolution, and consequently a more detailed glacier/ice-sheet mask, as most of the in situ observations compiled by Machguth are near the ice-sheet margin. In addition, the altitude difference between the observation location and the model grid point could further affect the SMB. A preliminary analysis with a subset of the observations compiled by Machguth and used by van Dalum et al. (2021) shows qualitatively similar results to Figure 10b in the manuscript: the SMB in the CTL experiment being always positive (i.e. no mass loss), whereas the SMB in the GLA experiment shows better agreement with the observed values (see below). As the manuscript already contains several figures and comparison with various datasets, we chose not to include this additional analysis here but will consider it as part of future work.



L 367: rephrase, ice sheets are not glaciers.

Thanks, done.

L 373: Please explain briefly, in the methods section, the Kling-Gupta Efficiency. Now every reader not familiar to it, is forced to dive up Gupta et al.

We have amended the text including more details on the Kling-Gupta Efficiency and its common usage in hydrology.

Figure 11, panel a. Not the KGE is shown, but the difference or change in KGE. Adjust the caption accordingly.

Thanks, done.

Section 3.3: KGE and glaciers are not my expertise, but when KGE is a common measure to evaluate river discharge, there are other model evaluation studies that have given KGE scores - so cite a few other studies and their scores to give the reader a clue if the KGE scores GLA and CTL are good or poor.

A common strategy is to compare the performance of the model with the observed mean flow. Knoben et al. (2019) have shown that a KGE >~ -0.41 indicates that a model improves on the mean flow benchmark. This corresponds to a Nash-Suttcliffe Efficiency (NSE) value of zero. However, in this study we are more interested in the difference of the KGE between CTL and GLA to highlight if the new parameterisation improves or degrades the hydrological skills of the current modelling system. We have included the reference value of -0.41 in the revised manuscript, when discussing the KGE, as well as the KGE values of GLA and CTL in Figure 11 among with the values of the components of the KGE.

References

Best, Martin J., et al. "A proposed structure for coupling tiled surfaces with the planetary boundary layer." *Journal of hydrometeorology* 5.6 (2004): 1271-1278.

Knoben, W. J. M., Freer, J. E., and Woods, R. A.: Technical note: Inherent benchmark or not? Comparing Nash–Sutcliffe and Kling–Gupta efficiency scores, Hydrol. Earth Syst. Sci., 23, 4323–4331, https://doi.org/10.5194/hess-23-4323-2019, 2019.