Reviewer 2:

Triggered by the somewhat provokative abstract, I've read the manuscript by Koepnick et al.. To my regret, I adviced the editor that this research does not meet the standards to be publishable in Climate of the Past. Below I'll motivate my advice.

We thank the reviewer for the helpful suggestions. We have made significant revisions to our calculation of the surface mass balance following these suggestions and revised the text accordingly. Specifically, we now account for the ground heat flux, are careful to calculate the SMB using monthly rather annual-average analysis, and have addressed all other comments. These corrections, ground heat flux in particular, had a significant effect on our results, for which we are grateful, and we believe the manuscript has improved considerably.

The final conclusion of the authors is "that General Circulation Models (GCMs) still struggle to reliably calculate the SMB", and this is the correct conclusion for the results presented in the manuscript, but not for ESMs (GCMs is an outdated term) in general. I'm sorry to say that the authors have based their analysis on a model simulation that is not state of the art for modelling the SMB with an ESM. If the authors wanted to know, they could take a look at the recent research by Miren Vizcaíno, for example, to see how well an ESM can model the SMB of an ice sheet, or read the numerous papers by various research groups running coupled atmosphere-ocean-ice sheet models on their extensive efforts to get realistic SMBs within their coupled model environments. In the ITraCE simulation, Brady et al obviously did not. Otherwise they would have calculated the SMB on the fly, but they didn't.

Thank you for your comments. It seems that the issue was not the iTraCE simulation not being state-of-the-art, but rather our neglect of the ground heat flux in our offline calculation. Our conclusions have changed after we corrected our calculation method, and uur calculated SMB is now more consistent with ICE-6G mass rate of change, especially for the early deglaciation period. We followed the work of Miren Vizcaíno, we cite the relevant papers, and exchanged emails with her during the revision to make sure we understood the details of her approach. We note that Vizcaino et al. have used a similar configuration of CESM1 to study the SMB of the GIS. While the reviewer is correct that iCESM1.3 does not include a SMB calculation, we now calculate the SMB offline in a way we feel is satisfactory.

So the conclusion of the manuscript is obvious, namely that if a model doesn't aim to model SMB, it won't get the SMB right. I don't see the need to publish this. It is common sense that if a model is not set up to model a particular property right, there is no chance that by some magic that property will be modelled correctly.

We note that after revising our calculation of the SMB and including the ground heat flux, it is no longer in disagreement with ICE-6G. We also note explicitly in the manuscript the novelty of this study,

> Our goal here is to study the role of SMB during the last deglaciation,

> introducing two novel elements to the analysis: First, we calculate the SMB of the LIS continuously in time during the entire deglaciation. Second, we compare the area-integrated SMB to ice mass loss rate deduced from independent geophysical constraints as represented by the ice sheet reconstruction in ICE-6G (Peltier et al., 2015).

The authors give the impression that they have estimated the SMB from the output, and I am willing to believe that the authors think they have estimated the SMB to the best of their ability. The latter, if true, is rather worrying because the manuscript gives the impression that the authors do not know what is essential to model the SMB correctly. This impression arises from the pointless discussion on whether or not to use the geothermal heat flux as an estimate of the ground heat flux of a glaciated surface (the ground heat flux is dominated by other processes),

Thank you for your comments. We now include ground heat flux and calculate the monthly SMB using monthly iTraCE output.

the funny unit error in Figure 1 (if you're talking about kg, you don't need to specify that it's in water equivalents. You do if you're using volume or thickness),

We fixed this typo, thank you for catching that.

the fact that the authors present year-averaged, ice-sheet-averaged fluxes in Figure 2,

we now use annual averages that include only months when melting occurs, thank you for this important comment as well.

and, lastly, the failure to realise that before you start playing around with small changes in albedo, you need to show that albedo makes sense at all. Given that there is negative SMB all the way up to the ice divide, I'd say it doesn't.

We eliminated the albedo sensitivity discussion in the revised manuscript following these suggestions and we feel it is not needed given the revised SMB results.

The authors also seem unaware of the complexity of modelling the SMB of an ice sheet IF you have a proper (online) estimate of meltwater runoff and hence SMB. Ablation zones are generally narrow and steep, especially for land-terminating ice edges

Thank you for mentioning the sensitivity of the narrow ablation zones to model resolution. While we cannot correct the relatively coarse iTraCE resolution, we now acknowledge and discuss this important issue as follows,

> The resolution of the climate model experiment we analyzed is relatively coarse, to enable their long time-integrations. It may not accurately resolve the relatively narrow ablation zones at the ice sheet margins, where steep SMB gradients can occur due to intense localized melting and runoff. This can lead to an underestimation of mass loss, as critical

> areas of negative SMB are smoothed out and fail to be fully resolved. Additionally, the coarse atmospheric model resolution may inaccurately represent the interaction between ablation zones and atmospheric processes, such as the advection of warm air and moisture, further biasing SMB estimates. These limitations emphasize the need for higher-resolution models to better resolve the complex dynamics of ablation zones and their contribution to overall ice sheet mass balance.

(ok, the dying glaciers we have around the world contradict this statement, but yes, these glaciers are dying). When working with lower resolution input data, as is the case here, these ablation zones are usually missed, leading to an overestimated integrated SMB. So if you find an SMB that is still too low, then something is really wrong.

With the reviewer's recommended changes, the SMB is now no longer too negative. We hope this means that the resolution of the narrow ablation zones, while still an issue which we acknowledge, still allow us to obtain useful results.

Finally, the authors' assumption that the SMB should at least not be more negative than the long-term mass loss is inadequate.

Given the corrected results, this is no longer an issue. We thank the reviewer again for the helpful guidance.

I assume the authors are aware that the Greenland Ice Sheet, although losing mass, still has a positive SMB of about 30% of its accumulation input.

Yes, thank you.

Yes, the Laurentide Ice Sheet was land-terminating at its southern margin, but marine-terminating everywhere else.

Agreed, we explain now how we carefully deal with grid cells that are part ice, part land (Section 2.5).

Yes, ICE-6G does not provide a mass budget for the Laurentide Ice Sheet, but I don't see what's complicated about investigating the existing modelling literature for the typical mass budget of ice sheet models representing this period, by personal communication if the papers don't reveal it. I'm sorry, but this is poor research practice IMHO.

We have now cited and discussed all existing modeling literature of the simulated SMB of the LIS during the last deglaciation. This includes papers by van den Broeke et al. (2009); Khan et al. (2015); Fyke et al. (2018); Kapsch et al. (2021); Ullman et al. (2015); Bradley et al. (2024); Rutt et al. (2009). Given these, we feel that the novel aspects of our study as identified in our introduction and listed above are valid and would like to think this paper represents a useful contribution. We hope the reviewer agrees given the revision.

So what can be done to improve this manuscript?

In iTraCE, the surface energy balance is not derived for glacier surfaces, or at least not correctly. So simply reconstructing the melt from the surface energy balance (Eq. 1) does not work, as the authors rightly conclude. I don't see the added value of improving the analysis of the results presented here, to conclude again that if a model doesn't try to model SMB, it doesn't model SMB correctly.

As mentioned above, we would put the blame on our previously neglecting the ground heat flux rather than on iTraCE. We now carefully follow papers such as Vizcaíno et al. (2014) and feel the offline SMB calculation provides reliable and useful results, much more consistent with the mass loss rate deduced from ICE-6G.

In my opinion, the authors need to start from scratch. If the authors want to continue to use the iTraCE simulations to estimate the SMB, they need to develop a model or method to estimate the SMB that acknowledges the shortcomings of the GCM data. There are several studies by palaeo-ice sheet modellers using GCM data that have done this before. However, the result of such research is a new paper, not a revised version of this paper.

We agree, have started from scratch, the results have changed significantly. We are embarrassed about having neglected the ground flux previously (having been influenced by some related SMB literature) and we feel the revised manuscript can indeed be considered a new paper...

However, if the editor decides that this manuscript should be given a second chance, I would suggest that

- The authors first discuss the (summer) surface energy balance (including summer albedo and near-surface temperature) and evaluate whether the modelled fluxes are realistic. Modern Greenland and Antarctica can provide some clues as to what might be expected, taking into account orographic and (Milankovisch-driven) isolation differences. In this evaluation, all energy fluxes (including LHF) are expressed in W/m2 and the 'flux convention' is used, i.e. fluxes are positive when directed towards the surface. [And remove the geothermal heat flux, don't embarrass yourself].

Thank you for these suggestions. we

- switched to a monthly surface energy balance and surface mass balance calculation.
- removed geothermal heat flux
- included the ground heat flux
- switched to a convention where all fluxes are positive toward the surface.

- Next, the modelled SMB is analysed.

done.

- The authors make a realistic estimate of mass loss to the ocean as function of the time, so that a proper "observational" integrated SMB time series is used.

Hopefully our revised calculated SMB represents a more realistic mass loss estimate. If not, we are admittedly not completely clear on what this item suggests.

- The authors compare the modeled SMB patterns with SMB patterns used in other studies, generated by other methods.

Thank you for this comment. We have expanded the discussion of the comparison to existing estimates, including the following text,

> The magnitude and spatial distribution shown in Fig. 4 are similar to Fig. 4 of Kapsch et al. (2021) for the time slices of 15 and 14 ka, based on their simulation of the SMB using a surface albedo evolution model and during the last deglaciation using the Earth system model of the Max Planck Institute (Mauritsen et al., 2019). The negative SMB/net melting (runoff) intrudes into the center of the ice sheet more than in Kapsch et al. (2021). This may be due to the coarser resolution of iTraCE (2° rather than $< 1°$). Although the spatial distribution of SMB for the LIS was provided by Kapsch et al. (2021) for a few time slices, they only showed the full area-integrated SMB over time for the GrIS, while our focus here is the LIS.

- When the authors then try to correct the SMB by increasing the albedo, both the new summer albedo fields and the new SMB patterns are shown, at least for some key moments during the transient simulation. The authors should be aware that increasing the albedo decreases the surface temperature, which increases SHF, LHF and LWup (so, e.g. LWup becomes less negative). On instantaneous data, one can make some first order estimates of how large these feedbacks are to arrive at relatively correct updated melt estimates. However, doing this on monthly data is questionable - but ignoring these feedbacks would be even more questionable. So any result of such an analysis should be accompanied by a proper idea of the uncertainties involved.

We agree and have removed this section. It is no longer needed given that the SMB agrees with ICE-6G now.

- The authors remove Figure 2 and its subsequent analysis as it stands. You can look at papers such as https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2020GL090653 to see how you can work out what processes are driving the melt. I know there is more than one way to partition the energy sources of the melt, but annual, ice-sheet averages are not among the methods that give meaningful results.

Following this suggestion, Figure 2 has been modified to using monthly data and averaging only over summer months. In any case, this figure (inspired by Figure 6 in https://doi.org/10.5194/cp-20-211-2024). We also added the following to the caveats section in Ln. 414–416

> We also calculated melt on a monthly time-scale, which may not resolve transient melt events. However, Wang et al. (2021) showed that large melt events contribute little to total surface mass loss for the present-day GrIS.

- The authors revise the wording of their conclusions, acknowledging that models that are not set up to model the SMB cannot be expected to model the SMB correctly. This research doesn't say anything about the performance of GCMs (well, use ESMs, not GCMs) that do try to model the SMB correctly.

The paper has been revised throughout, as suggested. As we acknowledged above, the issue was not so much that the iTraCE model was not set up to model the SMB, but our mistake of not including the ground flux. We take the opportunity to thank the reviewer again for pointing this out.