We first would like to thank the Reviewer#1 for the thoughtful comments which will help to improve our manuscript.

## Major comments

1. The algorithm used to correct for elevation differences (the offline correction) is a key part of the methodology, and it is extensively mentioned in the results and discussion. I would recommend to provide a more detailed description of this correction in the methods section, and possibly mention how this algorithm was optimised for the Greenland ice sheet. This would help the reader to better understand the main conclusion of this manuscript. For instance, it is not clear now if the surface temperature was corrected with a lapse rate, and if so, using which value ?

It seems that our current description of the offline correction it's not clear and deep enough as the two reviewers pointed out. To address this issue, we propose to add a scheme to illustrate better how this correction is set up, as suggested by Reviewer#2. Furthermore, we propose to revise the explanation as follows:

P5. L. 3-10: "Before any data exchange between the models, data has to be interpolated on the destination grid because the two models were run at two different spatial resolutions (25 vs 4.5 km). The surface elevation simulated by PISM is then interpolated using a four-nearest-neighbour distance-weighted method on the MAR grid at 25 km. For the MAR variables, they are interpolated using the same method on the PISM grid at 4.5 km. However, they are further corrected by considering the difference in altitude between the two grids at the time of interpolation thanks to local vertical SMB/ST gradients. This method is described in (Franco et al., 2012) and is called offline correction hereafter. This method corrects the altitude-dependent variables (such as SMB and ST) by applying a local linear gradient of the variable according to the surface elevation differences between the current MAR grid cell, and the surrounding MAR grid cells (9 grid cells considered here to compute the vertical gradient)."

Become:

"Before any data exchange between the models, data has to be interpolated on the destination grid because the two models were run at two different spatial resolutions (25 vs 4.5 km). The surface elevation simulated by PISM is then interpolated using a four-nearest-neighbour distance-weighted method on the MAR grid at 25 km. The MAR variables are interpolated using the same method on the PISM grid at 4.5 km. However, they are further corrected by considering the difference in altitude between the two grids at the time of interpolation thanks to local vertical SMB/ST gradients. This method is described in (Franco et al., 2012) and is called offline correction hereafter. Firstly, a linear and elevation-dependent gradient (Figure R1) is calculated over the MAR grid by considering the values of the considered variable (SMB at 4.5 km in our example, Figure 1) of the eight surrounding grid cells of the current one. This gradient is specific to each PISM grid cell and is locally determined. An example of this gradient can be found in Figure RS1 in The Supplement. Subsequently, These gradients are utilised to correct the variable when it is interpolated onto the PISM grid. The correction is performed by multiplying the interpolated variable by the difference in surface elevation between the grid cells in MAR and in PISM. This offline correction is specifically employed to correct variables that are influenced by temperature lapse rate with altitude, namely temperature and derived variables."

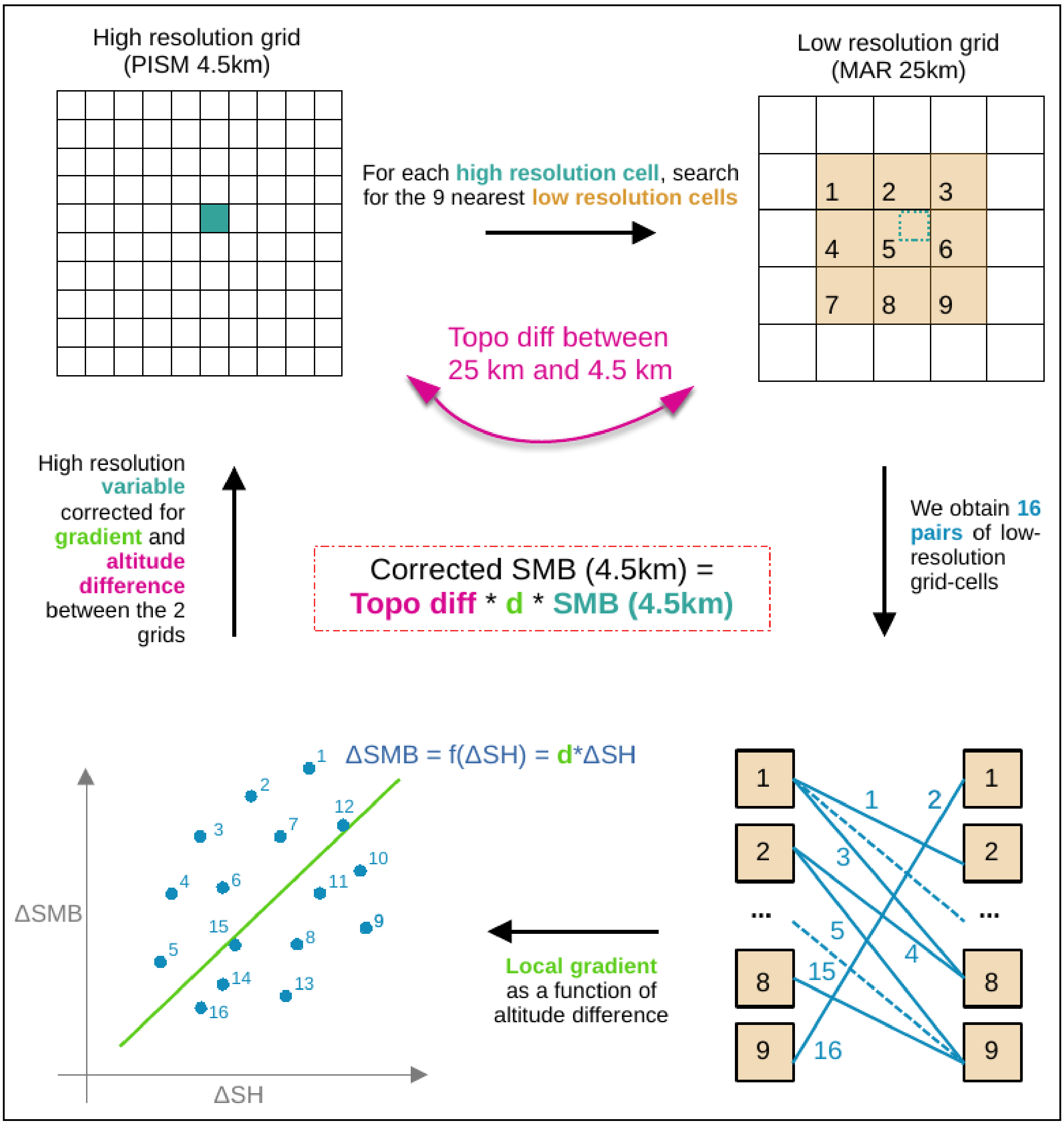


**Figure R1.** Steps of the offline correction as described in Franco et al. (2013). After interpolation of a variable (SMB, surface mass balance, in this figure) from a low to higher resolution grid, this variable is corrected to consider the influence of the temperature lapse rate with altitude. The correction is based on a local gradient (d) calculated by considering SMB differences (ΔSMB) between 9 nearest grid cells in the neighbourhood of the current one in the source grid in function of the surface elevation difference (ΔSH). Modified from Wyard (2014).

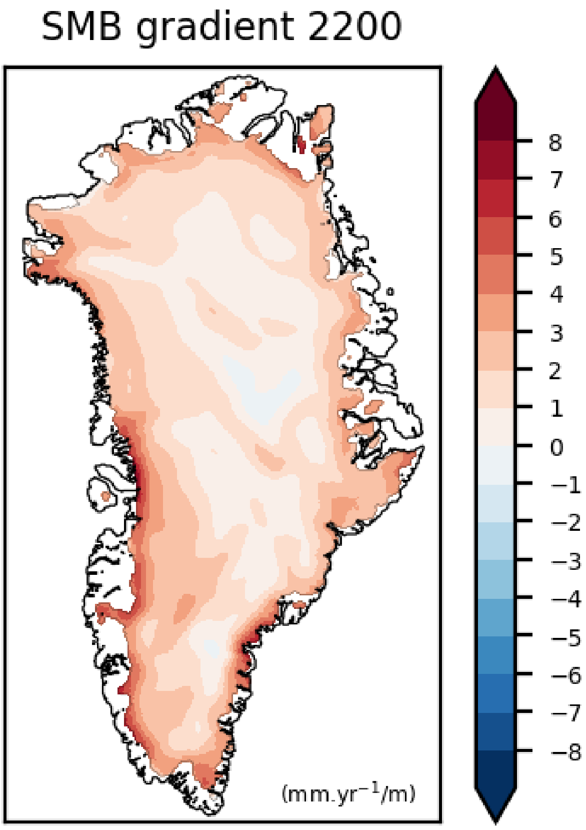


**Figure RS1.** Surface mass balance (SMB) gradients used to correct SMB as modelled by MAR (25 km) when interpolated on PISM grid (4.5 km) in 2200 by the MAPI-1w run (MAR-PISM uncoupled). Gradients (mm.yr-1/m) are multiplied by the difference in surface elevation to correct the rough SMB.

2. 25km horizontal resolution seems rather coarse to represent the narrow ablation zone in the most part of the Greenland ice sheet. The observed SMB can vary by a factor 2 within such a distance (e.g. on the K-transect, Van de Wal et al 2012), or even contain the entire ablation zone (e.g. on the Q-transect, Hermann et al 2018). While I acknowledge that the aim of this study is not to accurately model the SMB, it is likely that such a relatively coarse resolution strongly deteriorates the modelled wind and temperature patterns near the edges, therefore significantly changing the turbulent heat fluxes, and therefore surface melt. I would recommend to mention this in the discussion, and possibly refer to some studies which have shown that RCMs are still not yet accurately modelling turbulent heat fluxes near the edges of the ice sheet (e.g Fausto et al. 2016), or are sensitive the horizontal resolution (e.g. Franco 2012, van de Berg et al. 2020).

Thanks for your comment, it's an excellent remark that we will add to our discussion, as suggested. The reason why such "coarse spatial resolution" (25km) is used is that it's a compromise to well represent the SMB for all the ice sheet, and the MAR-computation time. Especially in a coupled mode, MAR computation time is very large. To optimize experiment time, we then used the optimal 25 km of horizontal resolution, knowing that SMB over all the ice sheet is not significantly improved by using a finer resolution.

3. One of the main results of the research is found in p12 L12 "ME depends on the sensible heat flux (SHF) related to air temperature and wind speed which are also overestimated on the margins by MAPI-1w (Fig. 7d and e)". I would recommend mentioning this very interesting result in both the abstract and the conclusion, since this is a key mechanism explaining the lower lapse rates in the coupled experiment.

As recommended, we will add this conclusion in both the abstract and conclusion. As discussed in the next comment, this could be followed by the assumption of the wind barrier that explains these differences in fluxes.

4. In the discussion, a very interesting link is suggested between the lower melt rates in the 2W coupled experiment and the mitigation of barrier winds due to drastic surface lowering. While this is a plausible explanation for the changes, this would require further analysis to properly quantify. For instance the increase in surface slope is also expected to affect the katabatic forcing in the momentum budget. Therefore I would recommend to either perform a more detailed analysis of modelled wind patterns, e.g. by investigating the entire momentum budget (van Angelen et al 2016) between the 1W and 2W experiments, or to mention in the text that changes in barrier winds are just one (of the possibly many) possible effects of surface lowering. In the conclusion (p18 L5) the reduction in barrier winds is now stated as a fact yet it has not been demonstrated in this study.

We agree that to confirm this assumption (changes in barrier wind responsible for changes in melt rate) it should be better to realise a complete wind budget. If it should be really relevant, it is also a bite outside the main goal of the paper, which is to present the coupling between MAR and PISM and an explanation of why both methods of representing the melt-elevation feedback give different results. As this comparison highlighted a new feedback link with the wind regime and which mitigates the melt-elevation feedback, it was important to try to explain it. A wind budget requires also more data than we have, meaning that we need to run again the model to obtain more detailed output concerning wind components, and at different levels. Given all these parameters, we prefer to keep this explanation with barrier wind as the main assumption to explain why we have less melt at the margins in the fully-coupled MAR version. A wind budget should be an excellent exercise to propose as a perspective of this work and will be of course explained in the discussion.

We could also add that the horizontal resolution, as discussed in the former comment is probably not the best one to realize this exercise. A finer resolution should more suitable to represent this kind of phenomenon, as well as flux budget.

The discussion, conclusion and abstract will be adapted following this comment and the answer.

## Minor comments

1. p2 L25 The statement that the offline correction works well as long as SMB is mainly dominated

by elevation could be reformulated. In principle there is no reason to believe that the SMB is a linear function of elevation, yet this is what is observed in the field.

To clarify this sentence we propose to add that SMB is linked to the surface elevation through the temperature lapse rate.

“Using an offline correction works well as long as SMB (particularly melt) is mainly influenced by the surface elevation.”

Will be replaced by:

“Using an offline correction works well as long as SMB (particularly melt) is mainly influenced by the surface elevation **through the temperature lapse rate**.”

2. p2 L34 ”What becomes GrIS” should be reformulated

“First, the aim is (1) to analyse what becomes GrIS in 2200 with this new coupling following an extreme scenario.”

will be replaced by:

“First, the aim is (1) to analyse **the evolution of the GrIS by 2200** with this new coupling following the influence of an extreme scenario.”

3. p3 L16 ”good performance”. Providing some numbers would be useful to better describe the uncertainties in modelled SMB by regional climate models. It would also help to mention that the evaluation of MAR by the authors (Delhasse et al, 2020) was using a higher horizontal resolution in MAR (15 km). See also Major comment #2.

In Delhasse et al. (2020) SMB from MAR is not directly compared to SMB-observation, but near-surface climate variables, determinating SMB in MAR are. What we can add here is that MAR forced by reanalyses better perform to represent near-surface temperature that ECMWF-reanalyses themselves compared to in-situ observation. We can also specify that the resolution was finer in the evaluation paper.

4. p6 It would be useful to extend Figure 1 with the initialisation steps to better understand section 2.3.1.

This remark was already formulated by the editor at the first read. We propose so to integrate into Figure 1 the extension realised for the editor's comment:

5. It is not clear why the surface mass balance is sometimes referred to as ”(surface) mass balance” (e.g. p7 L29) or ”surface mass balance”. Using the same would improve readability.

Thanks for this remark, it seems that it was not very clear. We just mean mass balance and surface mass balance. But we will change and clarify with both abbreviations: MB and SMB.

6. p8 I believe there is something wrong with the notation of mass loss in L2 :"-50 Gt.10−3". Should it be 50 10.3 Gt ? Also in L4.

It's not a mistake, but we should specify this is the value of total mass balance, which is negative.

7. p1 L5 What is meant exactly by "as well" ? Do the authors refer to the performance of degree-day models in ice sheet models ? Please be more specific.

To clarify, we propose to change the sentence as follows:

"[…] atmospheric models, which can represent the surface mass balance, usually using a fixed surface elevation, and the ice sheet models, which represent the surface elevation evolution but do not represent the surface mass balance as well as atmospheric models."

Will be rephrased as:

"[…] atmospheric models, which can represent the surface mass balance **(Positive degree day, or regional climate models for instance)**, usually using a fixed surface elevation, and the ice sheet models, which represent the surface elevation evolution. **These last ones do not represent the surface mass balance explicitly as well as atmospheric models.**"

8. p8 The unit of the y-ax in fig 8b is missing. Adding the variables of each ax would also increase readability.

Thanks for these remarks, we will modify them in consequence.

9. p14 L3. Why is only the north-south wind component investigated, and not the wind vector or even the entire vertical profile of modelled wind speeds ? The latter would give a clear indication of changing boundary layer structure and therefore surface fluxes.

In order to demonstrate and illustrate the changes in barrier winds, we have only proposed the north-south component of the wind, which shows that speeds in this direction are less pronounced at the margin of the ice sheet in the coupled mode in MAR. The east-west component, as well as the mean wind speeds, have of course been analysed, but are not illustrated here because the changes observed do not allow us to justify the differences in terms of temperature, and therefore in terms of melting and SMB, between the results from the coupled and uncoupled simulations. These illustrations can of course be added as supplementary material.

## Technical comments

- o1 L10 "to" − > "for"
- p1 L12 "avoid"
- p5 L8: (Franco et al, 2012)
- p5 "2.3.1 Inisialisation"

• p17 L16 ”Do”

Thanks for all these technical comments, they were all considered in the revised manuscript.

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