We first would like to thank Reviewer#2 for the thoughtful comments and well complete review which will help to improve our manuscript.

The paper by Delhasse et al. presents a coupling methodology between the regional atmospheric model (forced by the large-scale CESM2 climate model) and the PISM ice-sheet model over the 1991-2200 period. Different levels of complexity for the coupling are compared: A real two-way coupling, a one-way coupling in which only the melt-elevation feedback is accounted for through an offline correction and, finally, the no coupling scheme where MAR is run with a fixed topography of the Greenland ice sheet and PISM is forced by the surface mass balance and the surface temperature computed by MAR. A focus is made on the role of the melt-elevation feedback and the barrier winds on the evolution of the Greenland ice sheet geometry (and the consequences on ice surface velocities), the surface mass balance and its components (runoff, meltwater production solid and liquid precipitation) and the contribution of Greenland ice sheet to sea level rise. Most often the results are generally presented in a fairly clear manner, although some parts of the paper require further explanation or analyses (see Specific Comments). This study is similar to the one conducted by Le chlec'h et al. (2019) that was based on the coupling between MAR (forced by the CMIP5 climate model MIROC5) and another ice-sheet model (GRISLI), despite a slightly different methodology to extend the warming scenario beyond 2100. I find it very instructive to compare two modelling studies done with similar approaches but different models. The discussion provides a detailed comparison explaining the different conclusions of these two studies. Overall, I find the study of Delhasse et al. deserves to be published after a number of revisions have been addressed.

Specific Comments

In several places, I think the paper would be of better quality if some clarifications or additional explanations were made.
1/ First of all, I think that a more extensive description of the PISM model is required. The description provided in the paper is too technical, if not incomprehensible,for a reader who is not familiar with ice sheet dynamics models. Although a number of references are given in which PISM is described, I think it is important to be able to understand the important points of this section without going to look for the references mentioned. Also, I think it is necessary to define a number of notions, such as SIA, SSA, the Mohr-Coulomb criterion, exponent of the sliding law, flow enhancement factor, etc... This list is obviously not exhaustive and some equations (and their meaning) would allow a better understanding of how the model works. For example, in figure 6 you represent the driving stresses. But how are these driving stresses calculated? Since you mention in the description of PISM the aspects related to the dynamics of the ice, it would be interesting to refer to them more in the analysis of the results, or else to keep in the description of the model only the characteristics used for the analysis of the simulations.

Yes, we agree and explain now a bit more the PISM setting and try to communicate to a broader audience. However, this section is very technical because it should also inform other ice sheet modelers about the ice sheet settings we used, such that they can reproduce or improve this experiment. Explaining all equations would go beyond the scope of this study and readers should refer to references describing PISM.

We propose to revide the entire section 2.1.2 about PISM description as follows:

“To represent the dynamics and surface elevation of the Greenland Ice Sheet (GrIS), we utilise the Parallel Ice Sheet Model (PISMv1.2.1), a high-resolution numerical ice-sheet/ice-shelf model (Bueler and Brown, 2009; Winkelmann et al., 2011). In PISM, the geometry, temperature, and basal strength of the ice sheet are incorporated into stress balance equations at each time step to determine the ice velocity.

PISM employs two approximations for shallow ice sheets: the Shallow Ice Approximation (SIA) and the Shallow Shelf Approximation (SSA). The SIA is suitable for slowly flowing ice that deforms under its own weight, assuming a strong connection between the ice base and the bedrock. The softness of the ice, affecting its flow velocity, is modulated by an enhancement factor, which we set to E = 3 in our experiments. Faster flowing ice, such as ice streams, glaciers, and shelves, is approximated using the SSA. PISM combines both approximations into a hybrid stress balance mode (Bueler and Brown, 2009; Aschwanden et al., 2012).

Basal sliding of the ice over the bedrock introduces basal resistance. The speed of basal sliding is determined by the sliding law, typically a power law relating to the basal shear stress and yield stress. In our study, we adopt the Mohr-Coulomb criterion and use an exponent of q = 0.6 for the sliding law.

The model considers basal resistance based on the hypothesis that the ice sheet rests on a till layer. The yield stress represents the strength of this aggregate material at the base of an ice sheet. When yield stress is lower than the driving stress (τd) there is likely to be sliding, and thus faster velocities can be observed. The driving stress in turn is dependent on the ice thickness (H) and surface gradients (Hs) of the ice: $\tau d \propto$ Hs. The thicker and steeper the ice, the higher the driving stress and most probably the ice velocity.

The properties of the till are further approximated by using material properties such as the friction angle. We vary the till friction angle linearly between 5° and 40° with respect to bedrock elevation (between -700m and 700m), following Aschwanden et al. (2016). This variation in friction angle leads to lower friction at lower altitudes and below sea level, resulting in increased surface velocities at the margins of the ice sheet, thus improving the match of flow structure for the glaciers.

To match the present-day extent of the ice sheet, we impose a strong negative surface mass balance (SMB) at the margins of the Greenland present-day ice mask. This setup allows only for ice retreat in our experiments. We also enforce a minimum thickness of 50 m for floating ice at the calving front and utilize the von Mises calving law, which is suitable for glaciers in Greenland (Morlighem et al., 2016). All other parameters are set to default values (University of Alaska Fairbanks, 2019). Our simulations do not consider bedrock deformation or changes in ice-ocean interaction, as we maintain constant submarine melt rates.”

2/ The GRIP record does not extend to 125 ka as the signal was perturbed at the bottom of the ice core. While, I think that it shouldn't affect much your results, this should be mentioned. Also, the original publication should be provided (Johnsen et al, 1992) instead of Johnson et al. (2019).

Yes, we cite the dataset here, as it includes several data sources. For clarity, we write now (P4, L24): "The historical time series (Johnson et al., 2019) ***includes the temperature*** derived from Oxygen Isotope Records from the Greenland Ice Core 25 Project (GRIP, Johnson et al., 1992)"

3/ The initialisation method of PISM and the coupled model could be better explained with a scheme. For example, you mention that during the first step of your initialisation PISM is forced with the 2D temperature anomalies of the last glacial cycle. Are these temperatures correspond to the internal temperatures? If so, these anomalies are not a forcing but an initial state for the next step of the initialisation. Other clarifications are needed: Explain why do you need to equilibrate the vertical temperature profile. This is not clear in the manuscript. How these anomalies are computed? What is the impact of using anomalies instead of absolute values?

For a glacial spinup it is assumed that the state of the ice sheet before a glacial cycle is equal to the one at present day, which means that ice topography and surface temperatures are as well. However, different surface topographies lead to different surface temperatures (which we achieve during our coupled spinup runs.) This is why it is common practice to use temperature anomalies over the last glacial cycle, because the assumption of equal glacial states before and after the glacial cycle only holds when using anomalies. The temperature anomalies are transferred to the surface, which gradually modifies the ice interior temperatures over time. The process of surface temperatures penetrating down into the ice column takes several thousand years due to the thickness of the ice, which can be several kilometers. The internal temperature profile of the ice in turn, determines its softness and deformability, thus affecting the flow velocity of the ice.

To summarize, in our glacial spinup process, we initialize the ice sheet model with temperature anomalies to account for different surface topographies. These anomalies gradually modify the ice interior temperatures over time, influencing the flow behavior of the ice.

We propose to complete section 2.1.3 as follows (since P4, L. 19):

"PISM is forced by yearly ST and SMB from MAR forced by CESM2. To achieve a stable spinup state, we forced PISM with the MAR mean fields (ST and SMB) over 1961 – 1990, when the GrIS was close to balance (Fettweis et al., 2017). However, for a realistic thermodynamics representation of the ice sheet, the temperature evolution of the last glacial cycle has to be considered"

Becomes:

"PISM is forced by yearly ST and SMB from MAR forced by CESM2. To achieve a stable spinup state, we forced PISM with the MAR mean fields (ST and SMB) over 1961 – 1990, when the GrIS was close to balance (Fettweis et al., 2017). However, for a realistic thermodynamics representation of the ice sheet, the temperature evolution of the last glacial cycle has to be considered, ***because the surface temperature slowly propagates down the ice column and determines the vertical ice profile of the ice sheet. The ice profile determines the ice softness and deformability, thus affecting the flow velocity of the ice.***

For a glacial spinup, it is assumed that the state of the ice sheet before a glacial cycle is equal to the one at present day, which means that ice topography and surface temperatures are as well. However, different surface topographies lead to different surface temperatures (which we achieve during our coupled spinup runs.) This is why it is common practice to use temperature anomalies over the last glacial cycle, because the assumption of equal glacial states before and after the glacial cycle only holds when using anomalies."

We also propose to add Figure R1 in Section 2.3.1 (Initialisation of the coupling) to illustrate both the PISM spinup and the coupling spinup:

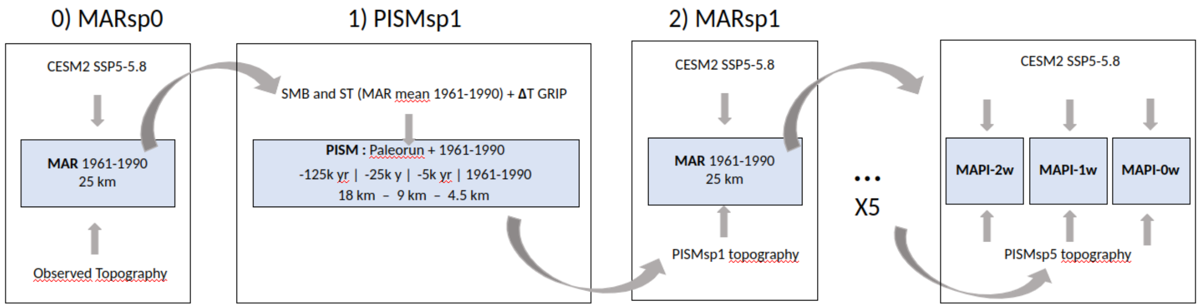

Figure R1. Steps of the coupling initialisation. Each MAR step corresponds to a 30-year long run over the reference period (1961-1990). And each PISM step consists of a new initialisation cycle of PISM as described in Section 2.1.3.

4/ a) The offline correction method is a key component of the overall paper as is drives the melt-elevation feedback, but the description of the method is very short. Although, I think I understand the basic principles of the method, I found that more details would have been welcome, and possibly a scheme to better illustrate the method.

b) Similarly, I think that the analysis of the results and the comment of Figure 8 are unclear (as well as the conclusions drawn from this figure) are unclear. For example, the authors mention "The dependence is no longer linear" (P12, L20). This just means that the temperature gradients are not the same in the two experiments, if I understood correctly. In other words, the altitude correction is not the same in both experiments. Again, this part of the analysis would be better understood if the authors had given more details about the correction method.

c) Also, I am not sure I agree with the first conclusion of this analysis, namely, "the linear correction is no longer valid in the ice sheet margins". I don't think the analysis leads to this conclusion if the altitude correction is different in the two experiments. Perhaps it would have been better to plot the regressions for each experiment independently (not as anomalies) and to examine the slopes of the regression lines separately.

d) Also, at the ice sheet margins, the behavior is not the same for altitude differences below ~350 m and above. This could be mentioned.

With this comment, we well realised that the description of this offline correction really needs to be revised. We answer these comments in 4 points (a to d) to be clear:

4.a) As asked by the first reviewer too, we propose to revise the description of the offline correction and add an illustration, to better illustrate what's done.

Here are corrections in the text including two more figures (this is the same answer as in review#1, first major comment):

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 R2) 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 used 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."

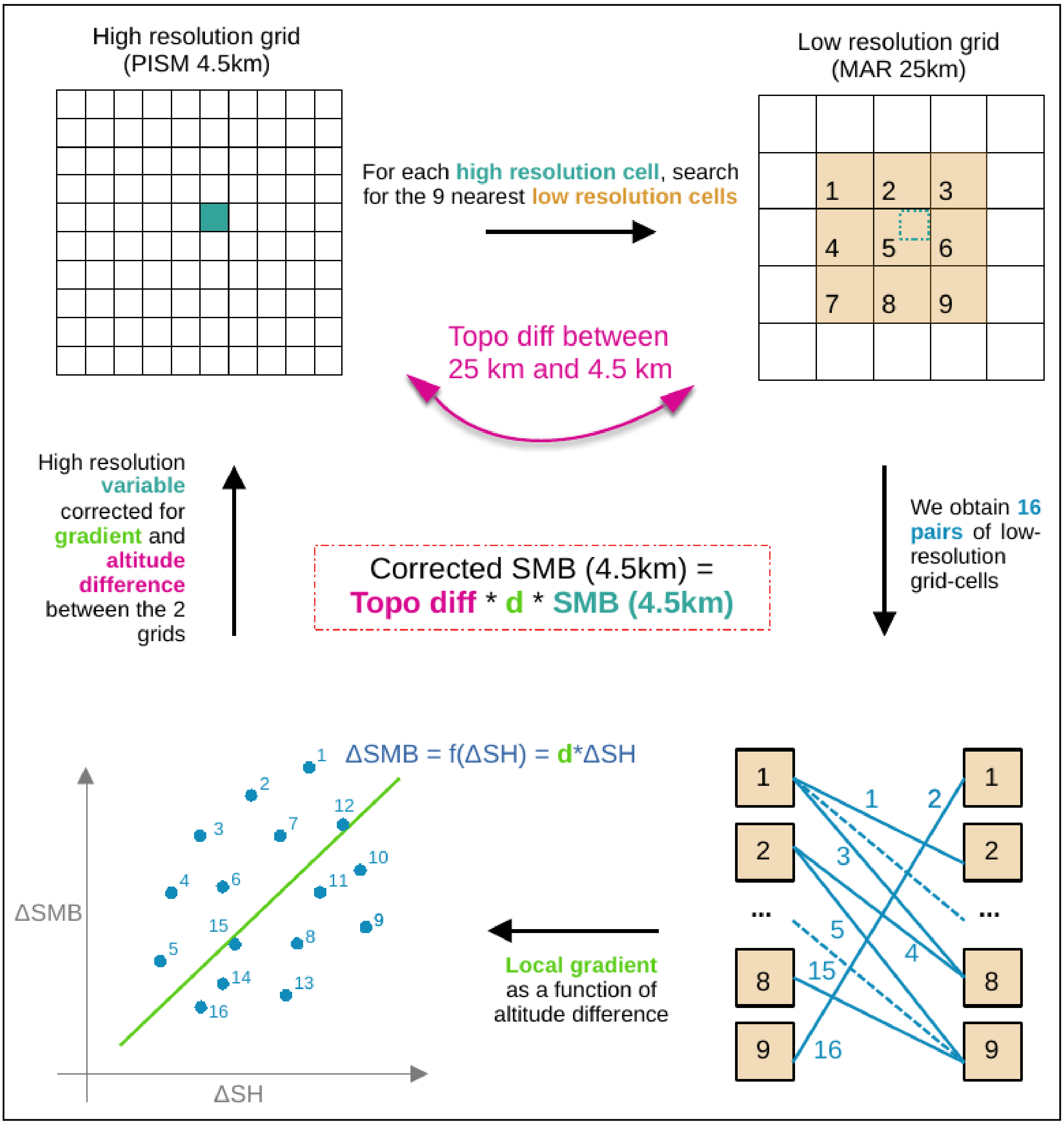

Figure R2. Steps of the offline correction as described in Franco et al. (2013). After interpolation of a variable (SMB, surface mass balance, in this figure) from a low to higher resolution grid, this variable is corrected to consider the influence of the temperature lapse rate with altitude. The correction 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).

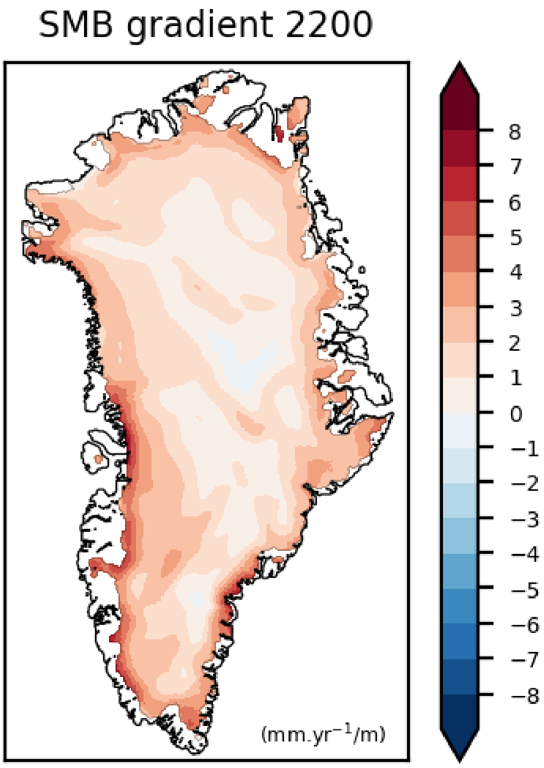

Figure RS1. Surface mass balance (SMB) gradients used to correct SMB as modelled by MAR (25 km) when interpolated on PISM grid (4.5 km) in 2200 by the MAPI-1w run (MAR-PISM uncoupled). Gradients (mm.yr-1/m) are multiplied by the difference in surface elevation to correct the rough SMB.

4.b) Similarly, I think that the analysis of the results and the comment of Figure 8 are unclear (as well as the conclusions drawn from this figure) are unclear. For example, the authors mention "The dependence is no longer linear" (P12, L20). This just means that the temperature gradients are not the same in the two experiments, if I understood correctly. In other words, the altitude correction is not the same in both experiments. Again, this part of the analysis would be better understood if the authors had given more details about the correction method.

Figure 8 highlights the spatial modification of the dependence between altitude and temperature, and consequently between altitude and melt. This is possible because we can compare a simulation where the topography has remained fixed (MAPI-1W) with an identical simulation where only the topography changes (MAPI-2w). So here we compare the results on the MAR grid of these two simulations, before applying the offline correction, each year at the same place. We are not comparing the different gradients used by the correction. Before running the simulations, we expected to find similar dependencies between the evolution over time of topography and temperature over the entire ice sheet. Thus the main expectation was that the gradients found here, by comparing the results over time, would give gradients similar to those calculated by the correction spatially.

Figure 8a.) highlights that in the center of the ice sheet (in blue), when the topography is modified in MAR, melt and temperature evolve linearly with these changes in topography, compared with the results obtained when the altitude remains fixed. In the center we can therefore say that 99% of the temperature changes (94% for melt, figure 8b.) can be explained by the change in surface elevation (R^2 = 0.99 and 0.94 rsp.), with a lapse rate of ~ -0.4°C/100m (regression slope) when we compare the annual temperatures obtained with a fixed topography and with a variable topography over time.

However, when we compare the same results for temperature and melt on the inner margins of the ice sheet (relations in green in Figure 8), we notice that only 61% (R^2 = 0.61) of the changes in temperature are explained by changes in surface elevation (69% for melt). We all agree that the term non-linearity is perhaps a little strong. But the key message here is that the relationship no longer reflects such a strong dependency (R^2 are lower). This proves that other factor(s) explain(s) these temperature changes. We have interpreted this as a factor that mitigates the dependence of temperature on altitude, and therefore the melt-elevation feedback.

The point about the offline correction is that it is based on the theory that changes in surface elevation lead to changes in temperature following temperature lapse rates, and this has a direct influence on melt, SMB and other temperature-dependent variables.

To avoid any confusion in Figure 8 with the correction, we propose to add, in the main text, values of the correction gradients used at these specific locations for both temperature and melt (Table R1). Note firstly that these gradients, for the interior of the ice sheet, are relatively close to those calculated in our example (regression slopes). Secondly, at the margin of the ice sheet, the gradient used in the correction is similar to that inside the ice sheet. However, thanks to our comparison in Figure 8, we know that by modifying the topography, the temperature and melt gradients in the margins are mitigated. It is therefore clear that the correction applied to the simulation with a fixed topography will give higher melt rates than expected at the edges of the ice sheet to the simulation with varying topography where all the processes and feedback resulting from elevation changes are considered. The correction we apply therefore does not take into account the process(es) that attenuate the melt-elevation feedback. This/these process(es) mitigate, as a consequence, the dependence relationship between the temperature and altitude by around 10 to 20%.

The gradients computed for the offline interpolatio from the fixed MAR grid towards PISM grid (MAPI-1w) in the same specific locations as Figure 8 are given in Table R1. Given a 200m difference in surface elevation between the fixed MAR grid and the PISM grid in 2200, this will lead to a correction of 990mm (-4.95 mm x -200m) inside the ice sheet. Similarly, we obtain a correction of 1052.5mm (-4.2 mm x -250m) at the margins. However, following our comparison over time of MAR coupled and running with fixed topography (Figure 8), the gradient in the margin should be much lesser important compared to inside the ice sheet; -4.95 inside VS -4.21 mm/m at the margins as calculated by the correction, and -3.30 VS -1.34 mm/m by comparing the two simulations. So as the gradient does not consider mitigation of the temperature lapse rates, nor the consequent mitigation of the melt-elevation feedback, the melt correction applied is too important and artificially increases the melt rate. The correction in the margins is too strong and results in underestimating the SMB in these areas.

Calculated on MAPI-1W	Temperature gradients (°C/m)	Melt gradients (mm/m)
INSIDE (49.24 °W, 67.07°N)	-0.0069	-4.95
MARGIN (48.83°W, 67.08°N)	-0.0065	-4.21

Table R1. Temperature and mel local gradients as considered by the offline correction in 2200 for the MAPI-1w simulation at two locations, one inside and one on the margin of the ice sheet. These locations are the same as in Figure 8 of the manuscript.

To better explain this figure in the main text, we propose to adapt as following:

P12, L14 - P13, L5:

"The underestimation of SMB in MAPI-1w is due to an overestimation of the melt-elevation feedback by the offline correction when interpolating MAPI-1w towards the PISM grid compared to the explicit consideration of this feedback in MAPI-2w. This correction is based on the linear temperature dependence with the surface elevation to account for the melt-elevation feedback that alters the SMB and related variables. The correction applies local linear gradients according to these altitude differences. We compare, on the MAR grid, the yearly evolution of the altitude differences between the two experiments (coupled and uncoupled) with the evolution of the temperature differences inside the ice sheet and on the margin (Fig. 8a). We notice that on the margin, the dependence is no longer linear (analyse for different other grid cells have been carried out but are not shown here). Inside the ice sheet, the temperature-elevation relation, evaluated as -0.4 °C/100m, remains linear. In our example, modifications of the topography in the 2-way coupling experiment have modified this linear relationship to -0.1 °C/100m of the temperature increase with the surface elevation lowering along the margins. The same relationship is illustrated for melt differences (Fig. 8b), confirming the modification in linear dependence with changes in surface elevation. This highlights two main elements: (1) the linear-offline correction of SMB is no longer valid in the ice sheet margins; (2) the non-linear relationship between temperature and altitude driving the melt-elevation feedback leads to mitigation of this feedback along the ice sheet margins."

Become:

"The underestimation of SMB in MAPI-1w is due to an overestimation of the melt-elevation feedback by the offline correction when interpolating MAPI-1w towards the PISM grid compared to the explicit consideration of this feedback in MAPI-2w. This correction is based on the linear temperature dependence with the surface elevation to account for the melt-elevation feedback that alters the SMB and related variables. The correction applies local linear gradients according to these altitude differences. We compare, on the MAR grid, the yearly evolution of the altitude differences between the two experiments (coupled and uncoupled) with the evolution of the temperature differences inside the ice sheet and on the margin (Fig. 8a). **We notice that on the margin, differences in altitude between the two MAR-grid (ΔSH) explain only 61% (69% for melt) of the changes in temperature differences (ΔT2m and ΔME respectively), compared with the interior of the ice-sheet where these relationships are much more dependent, with R^2 of 0.99 and 0.94 respectively.** In our example **(Fig. 8a),** modifications of the topography in the 2-way coupling experiment have modified this linear relationship **from -0.4 °C/100m inside** to -0.1 °C/100m of the temperature increase with the surface elevation lowering along the margins. The same relationships are illustrated for melt differences (Fig. 8b), confirming the modification in linear dependence with changes in surface elevation. **We will now compare these gradients, obtained by comparing the MAR simulations with and without changes to the topography over time, with the gradients used by the offline correction. These are calculated locally, i.e. taking into account the differences in altitude and in the variable considered with the surrounding grid-cells. For the example of the temperature, we find gradients of -0.69 and -0.65 °C/100m in 2200 respectively for the same locations as in Fig. 8 inside the ice sheet and on the margins. Although in absolute values these gradients are different from those obtained by comparing the two simulations over time, the difference between the two regions is smaller. The gradient applied to the margin of the ice sheet follows a similar dependency to that of the interior of the ice sheet. This explains the exaggeration of temperature and temperature-dependent variables (melt, SMB, etc.) on the margins by the correction, given the use of a gradient that is too large and does not reflect processes leading to the mitigation of the temperature altitude dependence, and consequently, melt-elevation feedback. All these comparisons** highlight two main elements: (1) the linear-offline correction of SMB is no longer valid in the ice sheet margins; (2) **the changes in the linear** relationship between temperature and altitude driving the melt-elevation feedback lead to mitigation of this feedback along the ice sheet margins."

4.c) Also, I am not sure I agree with the first conclusion of this analysis, namely, "the linear correction is no longer valid in the ice sheet margins". I don't think the analysis leads to this conclusion if the altitude correction is different in the two experiments. Perhaps it would have been better to plot the regressions for each experiment independently (not as anomalies) and to examine the slopes of the regression lines separately.

Figure 8 does not present the corrections applied in the two experiments, but the dependencies that exist over time between changes in altitude and temperature, and between altitude and melt, as explained in comment b) above. However, as detailed above, we propose to extend our comment on this figure and add a comparison with actual gradients used by the offline correction.

4.d) Also, at the ice sheet margins, the behavior is not the same for altitude differences below ~350 m and above. This could be mentioned.

That's right, beyond a difference of 300-350m it seems that the relationship is modified. We can point this out in our results. Without reaching values like those inside the ice sheet, it seems that beyond these differences in altitude, the relationship seems to get closer to the expected one. This could perhaps mean that we are dealing with something temporary, or that after a certain drop in altitude, the well-known dependence between altitude and temperature takes over again in terms of influence. This would have to be verified at different locations in the zones affected by mitigation of melt elevation feedback before any conclusions could be drawn.

5/ The role of barrier winds seems to be a key element to explain the differences in the melt-elevation feedback between MAPI-2W and MAPI-1W. Could it be mentioned more explicitly in the abstract?

As explained in the 1st review, mitigation of barrier winds is one plausible hypothesis to explain why we have here a mitigation of the melt elevation feedback by directly using an evolving topography in MAR. To confirm this hypothesis, it should be better to process a complete wind budget of the two simulations and highlight differences. As it's not really the first aim of this paper, we prefer to keep this as a hypothesis. This is why we prefer to not emphasise this explanation directly in the abstract.

6/ Overall, the paper is written in understandable English. However, I think that the quality of the manuscript would be greatly improved if it were proofread by a native English speaker.

We will make sure to improve the English written expression of the manuscript when we revise it.

Minor comments

P1, L3: While MAR is able to diagnose the ice sheet surface mass balance thanks to the implementation of snow/ice layers, this is not the case for many atmospheric models. The statement "atmospheric models which can represent the SMB" should be tempered.

Thanks for your comment, we propose to adjust the sentence as follows:

"This process is called the melt-elevation feedback that can be considered by using two types of models: 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. "
Become

"This process is called the melt-elevation feedback that can be considered by using two types of models: atmospheric models, which can represent the surface mass balance **for some of them, especially polar-oriented regional climate models. But they usually use a fixed surface elevation. And the other side,** ice sheet models which represent the surface elevation evolution but do not represent the surface mass balance as well as atmospheric models. "

P2, L31: "As the coupling is dependent on the used ISM à What do you mean? I guess that you mean that the results of your coupled simulations are model-dependent? Please, clarify.

We specify here that the coupling is model-dependent, specifically concerning the ISM-used, due to the divergence of the results in similar conditions of different models over Greenland (ISMIP Greenland 5 & 6). A coupling is less dependent of the RCM used, as SMB resulting from different RCM are quite similar over the recent period over Greenland. By saying that the coupling is dependent on the used ISM, we refer to and sum up the explanation given in P2, L14-17.

Section 2.13: It seems to be that the abbreviation ST has not been defined before. Also, explain why PISM needs to be forced with ST.

We now explain why we need a forcing of surface temperature. (see answer to major comments above. We introduce "ST" in section 2.1.1.

P4, L-25: The GRIP record was perturbed at the bottom and did not extent to -125 ka. This should affect your results so much but should be mentioned.

Yes, the dataset includes GRIP data but also had other sources so that we could simulate temperatures until -125 ka. See answers to major comments.

P6: L13: MARref forced with CESM2 is also run with PISMsp5 topography (see previous sentence). This sentence is a bit confusing. Please rephrase. Also change PSIMsp5 in PISMsp5.

Yes exactly, both MAR simulations (with CESM2 and ERA5) are running with the same topography from PISMsp5. To be clear, we propose to adjust our sentences as follows:

"The PISMsp5 topography, the last step of the initialisation process, will be the initial state of the different simulations compared here and is used to run the MAR reference simulation over the reference period (MARref). As our projections could not be evaluated, we compared performances of MARref forced with CESM2 to MAR using the PSIMsp5 topography and forced with the observed climate, i.e. the reanalysis ERA5 here (Hersbach et al., 2020). "

Become:

"The PISMsp5 topography, the last step of the initialisation process, will be the initial state of the different simulations compared here and is used to run the MAR reference simulation over the reference period (MARref). As our projections could not be evaluated, **we evaluated the performances of MARref over the present. To do so, we compared MAR results over the current period (1961-1990), with the initialised topography (PISMsp5) forced on one hand, by the ESM used for projections (CESM2) and on the other hand ERA5 reanalysis (Hersbach et al., 2020), considered as observations and well representing current climate.**"

At different places in the manuscript there is confusion between interpolation and aggregation. Outputs coming from a higher resolution model are *aggregated* on a coarser model grid. Variables computed with a lower resolution model are *interpolated* on a finer model grid.

Yes true, we didn't make the distinction, but we will correct that, thanks.

Section 2.4: You should add a comment explaining why you deal sometimes with surface mass balance and sometimes with mass balance. Also, explain (maybe before section 2.4) the difference between both.

The surface mass balance (SMB) is one of the components of the total mass balance of the ice sheet, call here mass balance (MB). The SMB is obtained by using the MAR model and summarises the gains and losses of ice mass at the surface, whereas the total mass balance is the result of computing both SMB and the dynamic of the ice sheet. MB is then the PISM result. In our main text, depending on which component we refer to, we talk about SMB or MB.

To address your request, we propose to specify again which model computes which part of the total mass balance in section 2.2, when we describe the coupling.

P4. L30-31 and P5. L1-2: "The coupling between both models has been performed by exchanging yearly outputs (SMB and ST from MAR, and ice thickness from PISM) on the 1st January of each year for 1991 – 2200 as described in Le clec'h et al. (2019). For MAR, this induces updating the surface elevation and ice extent of the ice sheet at the beginning of each year with PISM results from the previous year, whereas SMB and ST are used as forcing fields for PISM."

"The coupling between both models has been performed by exchanging yearly outputs (SMB and ST from MAR, and ice thickness from PISM) on the 1st January of each year for 1991 – 2200 as described in Le clec'h et al. (2019). For MAR, this induces updating the surface elevation and ice extent of the ice sheet at the beginning of each year with PISM results from the previous year, whereas SMB and ST are used as forcing fields for PISM. **The coupling aims to produce estimations of total MB of the GrIS by simulating dynamical components directly with PISM and using the SMB component as simulated by MAR as forcing for PISM.**"

P8, L4-L5: The sentence is a bit confusing as the climate is not stabilized just before 2100.

We propose to rephrase and complete this part as follows:

"Since there is an acceleration of the mass loss just before 2100, even with a stabilised climate, mass loss is not stabilised in 2200."

Become:

“Since there is an acceleration of **the warming and consequently of the mass loss before 2100, even by stabilising the climate and then the warming after 2100, the mass loss is still increasing until 2200**.”

P9, L10: “synoptic features of the large-scale CESM2 forcing” à Could it be illustrated with a figure (in the Supplement part for example)?

Yes, it’s a good suggestion, we’ll add this illustration in the supplements of the revised manuscript.

P11, L7: underestimated à underestimates

P12, L1-2: This sentence is not clear. Je ne comprends pas pourquoi les résultats de MAPI-1W sont contraires à ceux de MAR. I think that the sentence should be rephrased.

As it seems this sentence is confusing, we propose to extend the explanation as follows:

“The MB overestimation by MAPI-1w is contrary to the result of the MAR outputs, where MAPI-2w gave higher melt rates than MAPI-1w (Fig. 4 solid vs dashed lines).”

Becomes:

“The MB overestimation by MAPI-1w could be contrary to the intermediate results from MAR of both MAPI-1w and -2w simulations. **If we look at these results (raw MAR outputs)before interpolation and forcing of PISM, fully-coupled melt rate outputs are higher than in the one-way coupled simulation (Fig. 4 dashed lines). After interpolation, meaning the MAR results interpolated on PISM-grid and which actually forced PISM (Fig. 4 solid lines), MAPI-1w gave higher melt rates than MAPI-2w.**”

P12,L2: Figure 1a indicates that the melt-elevation feedback is taken into account via an offline correction. This seems to be in contradiction with the text.

To be consistent, when we interpolated MAR outputs on the PISM grid in both 1-way and 2-way simulations, we applied the offline correction. The difference is that in the 2-way coupled experiment, as the topography is updated each year in MAR, corrections during this interpolation are negligible compared to corrections applied in 1-way simulation as differences in surface elevation are very short and only due to spatial resolution. The melt-elevation feedback in the full coupling is actually well considered by directly modifying surface elevation in MAR as all SMB components are calculated in changing surface elevation.

P12, L3 : become à becomes

P12, L17: To which altitude difference do you refer. This is not clear and justifies a more in-depth explanation of the offline correction method.

We refer here to the difference in altitude between the two grids, MAR and PISM. Concerning the in-deep explanation of the offline correction method, we refer to the answer to the major comment 4.

P13,L6: Remind the link between wind, T2m and SHF, as it appears to be a key mechanism to explain the difference in the melt-elevation feedback in your simulations.

We propose to better explain it as follows:

P13, L6 - P14, L2: "The mitigation of the melt-elevation feedback in the MAR-coupled simulation is explained by the modification of the local atmospheric circulation on the margins around the GrIS. The evolution of the topography in the coupled simulation (for instance, Fig. 9e) causes a decrease in the melt increase with the elevation lowering. As meltwater production depends on the near-surface temperature and the wind through the sensible heat flux, we compare the vertical temperature and wind speed patterns above both simulation topographies along a transect crossing the ice sheet. The example illustrated in Fig. 9 highlights [...]"

Becomes:

"The mitigation of the melt-elevation feedback in the MAR-coupled simulation is explained by the modification of the local atmospheric circulation on the margins around the GrIS. The evolution of the topography in the coupled simulation (for instance, Fig. 9e) causes a decrease in the melt increase with the elevation lowering. **The production of meltwater is the result of a positive energy balance at the surface. More specifically, it is the sensible heat flux (SHF) that interests us here, as it is directly proportional to surface temperature and wind speed. And it seems that these two parameters are influenced by changes in surface topography. Thus we** compare the vertical temperature and wind speed patterns above both simulation topographies along a transect crossing the ice sheet. The example illustrated in Fig. 9 highlights [...]"

P13, L6-7: This should be illustrated with a figure.

Actually, this sentence was supposed to summarise the entire following paragraph, and the figure chosen to illustrate all of this is the described one all along this paragraph: Figure 9.

P13, L7-8: Fig9e represents a cross section of the topography and not an evolution of the topography. Please, rephrase. The mitigation of the melt-elevation feedback with elevation lowering is better illustrated with Figs 8a and S4a.

The evolution of surface topography is well illustrated with these cross sections as the fixed topography of MAPI-1w corresponds to the starting topography of the 2way simulation. As we also plot the cross-section of the fully coupled simulation topography in 2200, we consider that we illustrate these changes in surface elevation. Of course, this is also illustrated by Figure 3, with changes all over GrIS, and Fig S4a, which illustrates the differences in surface elevation between the two PISM simulations (from

MAPI-1w and -2w). However, in Fig. S4a, differences could be due to, on one hand, the differences in SMB apply to PISM, or, on the other hand, to the model divergence itself. As it could be confusing, we won't add this reference at this place. Furthermore, the goal to illustrate surface changes with the cross-section is to have an idea of the shape modifications too, because the slope is very important when you study wind components.

P14, L3: at the ice sheet margins / on the ice sheet margins

P14, L4: I don't understand what you mean by "inside the ice sheet" in parenthesis

We just specify here that the grid-cell is in the margin, but still considered inside the ice sheet, so modifications of fluxes and temperature will be important to consider changes in melt.

P17, L16: "Do not consider" à Please rephrase. I suggest something like "We must not consider" or another equivalent formulation.

The sentence would mean that if we miss the consideration of this feedback, this will result in an underestimation of SLR.

We propose to rephrase as follows:

"Missing this feedback will result in underestimating the projected sea level rise contribution of 10.5%"

Typo and technical comments:

Sections 2.1.3 and 2.3.1: Inisialisation à Initialisation

P1, L5: Remove "as well as atmospheric models"

P1, L10: corrected to à corrected for

P1, L11: extrapolated à interpolated

P1, L12: avoid à avoids or "prevents from a too expensive coupling"

P2, L26: Remove "as forcing"

P2, L35 "assess the offline method" should be changed in "assess the ability of the offline method…"

P3, L1 "feedback" à feedbacks

P3, L1: Replace "as well as "by " "and which"

P4, L26-27: Provide the resolutions in km, not in meters (as in the other parts of the manuscript).

P5, L5: “For the MAR variables, they are interpolated” à “The MAR variables are interpolated…”

P6: L13: change PSIMsp5 in PISMsp5.

At different places in the manuscript there is confusion between interpolation and aggregation. Outputs coming from a higher resolution model are *aggregated* on a coarser model grid. Variables computed with a lower resolution model are *interpolated* on a finer model grid.

P7, L23: I feel that 10% is not so negligible.

P7, L29: Components refer to SMB. Replace “their components” with “its components”

P8, L4: Replace -200 Gt.10-3 by -200 10-3 Gt (same thing for L2)

P8, L12: Remove meltwater.

P11, L7: underestimated à underestimates

P12, L3 : become à becomes

P14, L3: at the ice sheet margins / on the ice sheet margins

P18, L14: sensibility à sensitivity

P18, L19: oppositely à in opposite ways ?

Figures

Figure 1a : See comment, P12, L2

Fig. 2: The grey band is not visible. Maybe you could choose a darker colour.

Fig 3: Green and red contours cannot be easily distinguished

Fig. 4 caption: (RU, in green) à (RU, in orange). Precise in the figure caption that solid lines correspond to the coupled experiment

Fig.9: Indicate what do the y-axes and x-axes represent (Figs.9a, 9c, 9e). The line thickness of the black lines in Figs.9b and 9d could be slightly increased.

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

References

Wyard, C: Évaluation de la pertinence du couplage MAR-GRISLI sur le Groenland. Mémoire de master en sciences géographiques, orientation climatologie, à finalité approfondie, Liège, Université de Liège, inédit, 96 p. 2014.