

We would like to thank the reviewer J. Fyke for the evaluation of our study and the constructive comments that helped us to improve the manuscript. Please find below the reviewer's comments in black font and the author's response in blue font.

Responses to J. Fyke (Reviewer 1)

Le clec'h et al present a study that assesses the strength of Greenland ice-sheet atmosphere feedbacks over the 21st century using a regional model that is coupled to an ice sheet model. I think this is a novel experiment and valuable study and has the potential to be cited extensively as ice sheets are increasingly incorporated into various climate model architectures. My suggestions for improvement, listed in 'order of appearance', are below. My primary general concerns, which I hope the authors can address adequately, involve some apparent inconsistencies in the coupling/spinup (e.g. use of topography anomalies, and uncertainty on how land surface types change in response to ice retreat, and what happens if the ice sheet wants to expand beyond present-day margins). Finally, please feel free to counter my suggestions if you think I'm in error.

Thank you for your constructive comments. We hope that we addressed your concerns in the following.

P1L1: "the projected Greenland sea level rise contribution is mainly controlled by the interactions between the Greenland ice sheet (GrIS) and the atmosphere": while I tend to agree, relevant models can't yet fully assess the ocean contribution, so I think this statement is overconfident. Please moderate.

We have considerably modified the text in the abstract and this statement has disappeared in the revised version:

"In the context of global warming, a growing attention is paid to the evolution of the Greenland ice sheet (GrIS) and its contribution to sea-level rise. Atmosphere-GrIS interactions, such as the temperature-elevation and the albedo feedbacks have the potential to modify the surface energy balance and thus to impact the GrIS surface mass balance (SMB). In turn, changes in the geometrical features of the ice sheet may alter both the climate and the ice dynamics governing the ice sheet evolution".

P1L2: "in particular through the temperature and surface mass balance – elevation feedback": no, the atmospherically-driven GrIS SLR contribution is controlled by radiative excess/warming. Feedbacks reinforce this effect but is do not control it.

Again, these lines have been modified (please see our previous comment).

P1L2: "fine scale processes"->"fine scale dynamical processes" ?

OK modified.

P1L15: "Furthermore, in 2150, using a fix ice sheet mask, as in the no coupling method, overestimates by 24 % the SLR contribution from SMB compared to the use of the ice sheet mask as simulated in the two-way method" this seems counter to the previous statement that SLR from two-way coupling is 9.3% larger than the uncoupled case. Is the difference due to dynamic

discharge term?

There is no contradiction but we acknowledge that the way this sentence was written was confusing. Actually, this sentence aims at quantifying the overestimation of the SLR projection inferred from changes in SMB only (and not from changes in simulated ice volume) when using a fixed ice sheet component. Therefore they ignore the albedo changes and the SMB-elevation feedbacks. By using such methods, we show that the use of a fixed ice-sheet mask leads to an overestimation of the GrIS contribution to SLR of ~6 % in 2150, and to an overestimation of ~23 % of the SMB (with respect to the use of a time variable ice-sheet mask). These estimations are referred to as SMB_{MSK-NF} (fixed ice-sheet mask) and SMB_{MSK-2W} (time variable ice-sheet mask) and are both based on the SMB-integrated method, traditionally used in RCM-based studies that have no interactive ice-sheet component. Conversely, when considering the two-way and the one-way coupling experiments, we find that the GrIS contribution to sea-level rise (computed from ice volume changes simulated by GRISLI) is 9.3 % higher when GrIS-atmosphere feedbacks are accounted for (i.e. in the two-way coupled method). In the revised version, this has been better presented (see section 4.4) and reformulated in the abstract:

“As a result, the experiment with parameterised SMB-elevation feedback provides a sea-level contribution from GrIS in 2150 only 2.5% lower than the two-way coupled experiment, while the experiment with no feedback is 9.3 % lower. [...]In addition, we quantify that computing the GrIS contribution to sea level rise from SMB changes only over a fixed ice-sheet mask leads to an overestimation of ice loss of at least 6 % compared to the use of a time variable ice-sheet mask”.

P2L 4: “The atmospheric conditions control the variability” -> “Atmospheric conditions control variability and change”

This section has been completely re-written to provide clarifications on surface melting and snowfall drivers before dealing with atmosphere-GrIS feedbacks:

“The evolution of the Greenland ice sheet (GrIS) is governed by variations of ice dynamics and surface mass balance (SMB), the latter being defined as the difference between snow accumulation, further transformed into ice, and ablation processes (i.e. surface melting and sublimation). While surface melting strongly depends on the surface energy balance, snowfall is primarily controlled by atmospheric conditions (wind, humidity content, cloudiness...). However, various feedbacks between the atmosphere and the GrIS may lead to SMB variations that can therefore directly affect the GrIS total mass by impacting its surface characteristics, such as ice extent and thickness, with potential consequences on ice dynamics (e.g., due to change in surface slopes).”

P2L7: “SMB directly affect the GrIS total ice mass by impacting its characteristics such as thickness, ice volume and ice extent” - this can occur both directly and via impacts on ice dynamics. Explicitly state the latter (dynamics) for clarity.

Again this part has been drastically reformulated with clarity in mind (see previous comment).

P2L9: there are more foundational references regarding the dynamical GrIS impact on atmospheric flow. Suggest to use these in addition/instead. As just one arbitrary example: <http://onlinelibrary.wiley.com/doi/10.1034/j.1600-0870.1996.00014.x/abstract>

Thank you for the reference. We have added the following:

“These changes may in turn alter both local and global climate. As an example, changes in near-

surface temperature and surface energy balance may occur in response to changes in orography (temperature-elevation feedback) or in ice-covered area (albedo feedback; see Vizcaino et al., 2008, 2015; Lunt et al. 2004). On the other hand, topography changes may alter the atmospheric circulation patterns (Doyle and Shapiro, 1999, Petersen et al. 2003, Moore and Renfrew, 2005) causing changes in heat and humidity transports.”

P2L11: “different processes and feedbacks”->“different processes and feedbacks that regulate transient ice sheet change”

Thanks for the suggestion, we have modified the text accordingly.

P2L16: “The climate models usually represent” -> “For example, CMIP5 climate models unanimously represented”

We modified as: *“For example, the CMIP5 climate models unanimously represent the ice sheet component with a fixed and constant topography, even under a warm transient climate forcing”.*

P2L24: Suggest citing recent Lofverstrom et al. discussion study on resolution dependence of ice sheet conditions in GCMs: <https://www.the-cryosphere-discuss.net/tc-2017-235/>

The suggested reference has been added as well as the following paragraph:

“Using the AGCM NCAR-CAM3 run at different spatial resolutions (T21 to T85) and coupled to the SICOPOLIS ice-sheet model, Lofverström and Liakka (2017) investigated how the atmospheric model resolution influences the simulated ice sheets at the Last Glacial Maximum. They found that the North American and the Eurasian ice sheets were properly reproduced with the only T85 run. According to the authors, this is likely due to the inability of the atmospheric model to properly capture the temperature and precipitation fields (used to compute the SMB) at lower horizontal resolutions, as a consequence of the poorly resolved planetary waves and smooth topography”.

P2L35: “the authors only consider a strict linear relationship between topography and SMB changes” - please note more clearly either here or in next paragraph why this a handicap to these methods, leading to why your approach is better

We have added the following:

“However, in both parameterisations by Franco et al. (2012) and Edwards et al. (2014b), the authors only consider a strict linear relationship between topography and SMB changes. Although changes in temperature can be derived from a linear vertical lapse rate, other processes governing the SMB such as those related to energy balance, precipitation or atmospheric circulation do not follow a linear relationship with the altitude. While this approach may be valid at the local scale for small elevation changes, it may lead to a misrepresentation of the SMB-elevation feedbacks for substantial changes in altitude, especially at the ice-sheet margins.”

P2L9: “The second fundamental requirement is to represent the ice sheet topography changes in the atmospheric model by using an ISM instead of the fixed geometry usually used” This sentence is tautological since by definition a fixed geometry will not capture topography changes. Reword sentence.

The sentence has been reworded as: *“The second fundamental requirement to describe the interactions between atmosphere and GrlS is to represent the ice sheet topography changes in the*

atmospheric model by using an ISM (instead of the fixed geometry typically used) to take into account the effects of ice dynamics on the ice sheet topography changes”.

Throughout text: “developped” -> “developed”

OK, modified.

P4L7: 16 km high, from surface? Sea level?

This part of the text has been changed in “The MAR horizontal resolution is 25 km x 25 km covering the Greenland region (6600 grid points), from 60 °W to 20 °W and from 58 °N to 81 °N, and 24 vertical levels to describe the atmospheric column in sigma-pressure coordinates (Gallée and Schayes, 1994)”.

P4L12: “hydrological cycle” -> “atmospheric hydrological cycle”?

Yes, modified.

How does Crocus differ/integrate with SISVAT? Please clarify. In the case where the ice sheet expands or contracts, how is under-snow (or snow free) ice sheet surface exchanged for bare land surface (or vice versa)?

Crocus is a 1D snow model, while SISVAT is the surface model embedded in MAR. In SISVAT, each grid cell is assumed to be covered by at least 0.001% of two major surface types, namely tundra and snow (including ice sheet). Tundra is considered by SISVAT as a vegetation zone with an albedo ranging from 0.1 to 0.2 as a function of surface water and plant type. On the contrary, the Crocus snow model is used to compute the albedo of ice covered areas. In the 2W method, the percentage of tundra/snow evolves following the ice-sheet model advance and retreat. We now provide more information about the MAR model in Sec. 2.1 and we hope that the interplay between Crocus and SISVAT appears now clearer:

“MAR is a regional atmospheric model fully coupled with the land surface model SISVAT (Soil Ice Snow Vegetation Atmosphere Transfer model, see Gallée and Duynkerke, 1997) which includes the detailed one-dimensional snow model Crocus (Brun et al., 1992) which simulates fluxes of mass and energy between snow layers and reproduces snow grain properties and their effect on surface albedo [...]. Each grid cell is assumed to be covered by at least 0.001 % of tundra and snow. At each time step SISVAT computes the albedo of each surface type and the characteristics of the snowpack which are weighted and averaged as a function of the snow and vegetation coverage in each grid point, and then exchanged with MAR.”

In addition, we have included more details on the 2W coupling methodology (in Sec. 3.3):

“At the end of a MAR model year, MAR is paused and GRISLI is forced by the downscaled SMB and ST fields with the method of Franco et al. (2012) as in PF (Eq. 7). Then, GRISLI computes a new GRIS topography and a new ice extent at 5 km which are aggregated at the yearly time scale onto the 25 km MAR grid. The aggregated ice extent is used to update the fraction of tundra relative to ice/snow covered surface type for the subsequent MAR run. To account for the differences between MAR and GRISLI topographies, the surface elevation which is aggregated onto MAR is computed from GRISLI surface elevation anomalies added to the present-day observed topography (Eq. 7). It is then used as the updated surface elevation in MAR. As previously mentioned, topography changes are negligible before 2020. Hence, changes in ice-sheet geometry are fed to MAR only

after this date. Compared to the NF and PF approaches, this two-way coupled method is the most accurate to represent the GrIS-atmosphere feedbacks”.

P4L20: “The topography of the GrIS as well as the surface types (ocean, tundra and permanent ice) are provided by Bamber et al. (2013)” -> clarify this is for the NC is experiment (presumably)

We made this clarification in the text:

“Except for the experiment presented later in this study in which MAR is coupled to an ice-sheet model, the topography of the GrIS as well as the surface types (ocean, tundra and permanent ice) are taken from the Bamber et al. (2013) dataset aggregated on the 25 km grid.”

P5L10: “we have repeated the MIROC5 year 2095 (representative of the years 2090s) for 50 additional years” - this repetition is certainly not representative of this time period due to lack of continued change, and also lack of internal variability. While I don’t think this is a fatal flaw of the study, the authors should clearly note this caveat here and later during discussion of results, so readers clearly realize the effects of this artificial ‘extension’ (probably, fairly strongly reduced overall change, making the results presented here conservative).

We acknowledge the fact that, in our approach, we discard the role of interannual variability within the GCM after the year 2100. This could indeed results in conservative estimates due to non-linearities of SMB (in particular ablation). However, the GCM imprint of the year 2095 may also increase regional changes in term of GrIS response. We present these limitations in the revised version of the manuscript in the discussion section:

In section 3, we mentioned that the use of a constant forcing from 2100 to 2150 *“implies that both climate changes and large-scale inter-annual variability are neglected beyond 2100”.*

In the Discussion section: *“A second question concerns the impact of a constant MIROC5 climate used to force MAR beyond 2100. As outlined in section 3, this results in discarding the continued change that the climate will likely undergo beyond 2100 suggesting that our SLR projections are underestimated. The second consequence is that inter-annual variability is neglected after 2100. This can lead to conservative estimates of Greenland melting contribution to sea level rise in the future due to non-linearities of the SMB. On the other hand, the imprint of the 2095 MIROC5 climate may amplify regional changes of the GrIS response”*

P6L11: Why is the annual mean bottom snowpack temperature not used as the boundary condition for the ISM instead?

It would be indeed possible, but probably would have very low impact on the ice temperature profile simulated by GRISLI. Indeed, the annual temperature at the bottom of the snowpack is very similar to the annual mean surface temperature. Because GRISLI has a yearly time step, it can not see annual temperature variability simulated by Crocus. Therefore, we have used the annual mean temperature as a boundary condition for the ISM.

P6L19: also just due to the long timescale of ice sheet responses?

Yes, we agree with this comment. It seems more appropriate to only deal with the long time-scale response of the ice sheet. Our motivation has been reformulated as: *“Due to the long time scale response of the ice sheet to a given climate forcing, a proper initialisation of the model is required before performing forward experiments”.*

Moreover, our spin-up procedure also includes the calibration of unknown parameters (basal drag coefficient) and our inversion procedure can be seen as a way to correct model deficiencies. That being said, we have clarified and simplified the presentation of the spin-up procedure (see Section 2.2.2) as we now directly refer to the paper published in the discussion forum of the Geoscientific Model Development journal. In the revised manuscript, this paper refers to as : Le clec'h et al. (2018).

P7L1: what is meant by 'vertical fields'? Please clarify.

We meant temperature and ice velocity profiles from Gillet-Chaulet et al., 2012. It is now specified in the revised manuscript.

Spin-up procedure: How does this procedure deal with ice growth outside the observed ice sheet extent? Figure 2 suggests this ice is simply removed? If so, how does this effective strong artificial sink of ice impact all subsequent sensitivity experiments? Please explain the impacts of this clearly in the text, if this is the case.

You are right, in our framework we apply an artificial strong negative SMB outside the observed present-day ice sheet mask. We do not think that it is a major flaw in our methodology as our spin-up procedure aims at reducing the mismatch between observed and simulated ice thickness. Assuming that MAR produces a realistic SMB on the ice sheet and because the simulated ice thickness is close to observations, we can hypothesise that our simulated ice flow is realistic. As such, in theory, the ice sheet should not grow outside the observed present-day ice sheet imprint. The artificial strong negative SMB outside the present-day ice sheet mask can be seen as a way to correct both the atmospheric model bias (e.g. positive / not enough negative SMB over the tundra) and the spin-up procedure bias (too strong ice export towards the margin). This has been explained at the end of the spin-up description (section 2.2.2).

P8L21: why not simply start the coupling at 2005 (i.e. the end point of the 1976-2005 initialization/spin-up period)?

Sure it would have been possible. However, as stated in the manuscript, the results would have been similar as the SMB changes through 2005-2020 does not produce any significant topography changes in GRISLI.

P9L13: The use of topography anomalies is concerning since it implies the SMB/ST field received by GRISLI is inconsistent with GRISLI's height (for example, the ELA on the GRISLI grid would exist at a different elevation than if the GRISLI elevation was directly used). Can the authors comment on why this approach does not introduce problems with their experimental design? As it stands, this is not justified adequately. An alternate approach that would have avoided this problem would have been to use the spun-up GRISLI topography as the 'fixed' topography instead of the Bamber topography.

The issue here is that GRISLI tends to produce steeper slopes than what is observed. This has important consequences for the climate simulated by MAR due to, in particular to katabatic winds. This is why we made the choice to maintain the realism of the simulated present-day climate (computed on the Bamber et al. (2013) topography) and the consistency between the climate simulated by MAR and the climate used to force GRISLI, downscaled at the 5 km resolution using the method developed by Franco et al. (2012).

In Section 3.2, we mentioned that *“Due to the topography differences between MAR and GRISLI, this approach has been chosen to avoid large inconsistencies between the SMB and ST fields computed by MAR and the ones corrected to account for the GRISLI topography”*.

We also discussed the impact of the anomaly method in Section 5:

“A second limitation is related to the 2000-yr relaxation GRISLI experiment, run at the end of the spin-up procedure to reduce the model drift in terms of ice volume, that produces residual differences with the observed topography (Bamber et al. 2013) used in the MAR simulations. This has important consequences on the MAR simulated climate. In particular, the steeper slopes existing in the GRISLI topography (i.e. S_{ctrl}) tend to produce unrealistic katabatic winds. Therefore, we choose to use an anomaly method of the surface elevation onto which the SMB and ST fields are downscaled at the 5 km resolution grid (Eq. 7). The objective of this approach was first to maintain the realism of the simulated present-day climate computed on the observed topography (Bamber et al. 2013) and, secondly, to avoid inconsistencies between the climate simulated by MAR and that used to force GRISLI. However, this implies that the forcing climate is not fully consistent with the GRISLI topography. This should be taken into consideration in a future work to improve the quality of our results”.

Figure 2 and other figures: 5 years is likely not long enough to generate robust climatologies. Suggest using at least 10 years instead.

We have followed your suggestions and used 10 years to compute climatologies.

P11L10: the finding of very strong marginal cooling due to increased katabatics is very interesting and pertinent, and deserves a further explaining. It would be very useful the authors plotted overlaid near-surface wind anomaly vectors plus ST changes in ‘zoomed-in’ plot of a good illustrative portion of the margin.

We provided further explanations to justify the role of katabatic winds in the marginal cooling (see section 4.2.1):

“Over the ice sheet, the steeper surface slopes simulated in 2W in 2150 (discussed in Sec. 4.1.2) lead to a slight increase in katabatic winds (Fig. 9). However, at the ice sheet margin, i.e. where the ice mask in MAR is below 100%, there is a substantial decrease in surface winds. This is because the change in surface elevation as seen by the atmospheric model is computed from the aggregated changes in GRISLI at 5 km. As such, a non-zero fraction of tundra, which presents no change in surface elevation, results in smaller elevation changes compared to grid cell in the same region with permanent ice cover only. This induces artificially lower surface slopes at the margin with respect to the interior and a decrease in surface winds in these regions. Altogether, the slight increase in katabatic winds over the ice sheet and their reduction at the margin lead to a cold air convergence towards the ice sheet edge (Figs. 8b and 9 and Fig. S8-S9)”.

To support these explanations, we added a new figure (Figure 9) displaying the 2W near-surface wind vectors at the end of the 2W experiment as well as the wind strength anomaly between 2W for NF. A zoom-in plot showing near-surface wind anomaly vectors overlaid to ST changes is provided in the Supplementary Materials as Figure S7.

Similar to above point: it would be excellent to see a quiver plot of wind anomalies over the entire

ice sheet, given their importance. Also would it be possible to visualize the increased mixing in the boundary layer, leading to warming in the 2-W coupled case?

A similar plot as Figure 9 in the main text is also given in the Supplementary Materials (see Fig. S9).

P11L23: do authors mean “Following the increase of the ST”?

Yes, this is what we meant. However, due to modifications in the structure of the revised manuscript, this part of the text has been removed.

P11L25: “, there is a decrease of 112 Gt yr⁻¹ of ice ” -> “112 Gt/yr extra ice ablates“

The sentence has been changed in: *“This process is faster in 2W than in NF and PF. In 2150, the ablation zone is 14 % (resp. 11.7 %) larger in 2W than in NF (resp. PF) causing 112 Gt yr⁻¹ of extra ice ablation in 2W (w.r.t NF)“.*

P11L30: “14 % larger in 2-W” - can an estimate be made of the uncertainty in this value (and others) due to interannual variability? Put another way, can the authors confirm that the changes they see are significant in the face of background noise in ablation area (for example)?

As specified in sections 2 and 5, the use of a constant climate forcing for MAR after 2100 (here the MIROC5 climate simulated for year 2095) implies that the inter-annual variability is neglected beyond 2100. As such, the relative changes in ablation areas after 2100 mentioned in the text are necessarily statistically significant, at least within the framework of our experimental setup. However, we acknowledge that a better approach would be to perform similar simulations with a prolonged RCP8.5 scenario (not available at the time of this study).

*P12L5: “lower surface temperature over these regions” - suggest reinforcing to readers once more here that this is *relative* to the NC experiment.*

Thanks for this remark. We paid attention to clarify the text when dealing with relative changes.

P12L8/9: what does the +/- indicate here?

This is the mean value +/- the root mean square error over the region. In the revised manuscript, the mean values do not longer appear with the +/- root mean square error. We chose to present the mean value results with the 5th and 95th percentiles when necessary.

P12L13: “become ice or snow-free or snow free, exhibiting bare ice ” this is confusing. What happens if the entire GRISLI ice column disappears? Does tundra emerge?

This part of the text has been modified and changes in ice-sheet extent are now only discussed in Section 4.4.

The point which which is addressed here has been clarified (see section 2.1):

“[In MAR], each grid cell is assumed to be covered by at least 0.001% of tundra and snow. At each time step SISVAT computes the albedo of each surface type and the characteristics of the snowpack which are weighted and averaged as a function of the snow and vegetation coverage in each grid point, and then exchanged with MAR”.

In both the NF and the PF experiments, the ice-sheet mask, as seen by GRISLI is not updated in the atmospheric model and MAR sees the present-day observed ice-sheet mask throughout the simulation. In the 2W experiment, the ice extent computed by GRISLI is then aggregated to MAR to update the fraction of tundra relative to ice/snow covered surface type for the subsequent MAR run. As a result, if the entire ice column disappears, MAR sees in each grid cell a fraction of tundra of 99.999 % and modifies the albedo accordingly.

P12L25: Previous studies have highlighted a strong decrease in ice discharge across outlet glacier grounding lines as a consequence of increased surface melting. E.g. Gillet-Chaulet 2012, Goelzer 2013 and others. Is this same effect seen here?

At the end of the 2W experiment (2140-2150), there is a decrease of surface velocities compared to the 2000-2010 mean period (Figs. 6a, 7c), suggesting that ice discharge across outlet glaciers is reduced. Moreover, the negative anomaly of ice flux divergence (Fig. 5b) shows an upstream ice accumulation (i.e. ice accumulates faster than it discharges through outlet glaciers). These results strongly suggest a decrease of ice discharge across outlet glaciers, similarly to what was found by Gillet-Chaulet et al. (2012) and Goelzer et al. (2013).

P12L25: Is it completely correct to say the entire SLR contribution is caused by the 'melting contribution'?

In our model, there is only a very few number of grid points in contact with the ocean. Therefore, calving is negligible and melting remains the dominant contribution to sea-level rise. However, to avoid confusions, we removed all expressions such as the Greenland melting contribution to SLR in the revised manuscript and simply use the Greenland ice sheet contribution instead.

P12L25: Can the authors quantify the reduction in marine margin extent in 2-W?

As explained in our previous response, the number of grid points in contact with ocean is negligible in our model. This is likely due to the too coarse GRISLI resolution (5 km) that prevents from properly resolving the complex topographic features of marine terminating glaciers. As a result, it is not possible to quantify accurately the marine margin extent. To illustrate the limitations induced by the coarse ice-sheet model resolution, we added the following paragraph in the Discussion section:

"Regarding the ice-sheet model, a 5 km horizontal resolution does not permit to capture the complex ice flow patterns of smallest outlet glaciers, whose characteristic length scale can be less than 1 km (Aschwanden et al., 2016) and to quantify accurately the ice discharge at the marine front. This may have large implications in the sea-level rise estimates. Using a 3D ice-sheet model with prescribed outlet glacier retreat, Goelzer et al. (2013) found an additional SLR contribution from outlet glaciers of 0.8 to 1.8 cm in 2100 and 1.3 to 3.8 cm in 2200, with the influence of their dynamics on SLR projections decreasing with time and with the increasing importance of the atmospheric forcing. This is in line with the fact that ice dynamics act to counteract ice loss from surface melting (see Section 4.2), as previously outlined by several authors (Edwards et al., 2014b, Goelzer et al., 2013, Huybrechts and de Wolde, 1999). However, despite the possible decreasing influence of marine terminating glaciers, at the centennial time scale, it seems to be preferable to evaluate more accurately the impact of ice dynamics and to better capture the complex geometry of fjords surrounding the marine-terminating glaciers".

P13L1: *“This higher integrated SMB, obtained when using no updated ice sheet mask” - do the authors mean “lower”..? This sentence seems to directly contradict the previous sentence. If I’m mistaken here, a clearer description of the processes here is needed.*

Yes, you’re right. In the revised manuscript (Section 4.4), we tried to better explain the issues related to the integrated-SMB method. We hope that the text has been clarified enough:

“A widely used method to estimate the projected GrIS to global sea-level rise is to compute the GrIS mass loss as the time-integral of the SMB computed by an atmospheric model over a fixed ice-sheet mask (Fettweis et al., 2013, Meyssignac et al., 2017, Church et al., 2013). In the present study, we go a step further since the ice mass variations related to SMB changes are computed over a changing ice-sheet mask as simulated by GRISLI. However, in both the NF and the PF experiments, the atmospheric model does not account for the variations in the ice-sheet extent simulated in GRISLI and the ice-sheet mask, taken from the observations (Bamber et al., 2013) is kept constant throughout the simulation. Taking the changes in ice-sheet mask into account may have strong impacts on the computed GrIS contribution to sea-level rise. To illustrate the influence of the ice sheet mask, we used the SMB outputs from the NF experiment at the MAR resolution and applied the integrated SMB method over the fixed observed ice-sheet mask (SMB_{MSK-NF}) and over the updated 2W mask (SMB_{MSK-2W}). Results reported in Table 2 indicate differences in SMB values exceeding 23 % in 2150. In the same way, compared to a time variable ice-sheet mask, the use of a fixed ice-sheet mask overestimates the sea-level rise by ~6 % in 2150. Though a bit lower, this number is far from being negligible compared to the errors made when the SMB-elevation feedbacks are not taken into account (i.e. 7.6 %) and when all the feedbacks are ignored (i.e. 9.3 %). This strongly suggests that realistic SLR projections cannot neglect the evolution of the ice-sheet extent, only accounted for through the use of an ice-sheet model”.

General: The authors should consider quantifying actual feedback factors associated with the inclusion of elevation feedbacks (see Roe 2009, Reviews of Geophysics). This would be a good benchmark number to produce, for other works to compare to.

We agree that a formalised way to quantify the elevation feedback would be very interesting, in particular for inter-comparison exercises. However, the definition of such a metric has yet to be done. For now, we only compare our SLR projections with and without the elevation feedback to other papers available in the literature a similar approach has been followed (e.g. Vizcaino et al., 2015; Calov et al., 2018).

P14L11: *“As for the ISM, increasing the grid resolution of MAR” - do you mean “as for the regional climate model”..?*

No, we think that an increase in both ISM and RCM resolutions could better constrain the SLR contribution from Greenland ice sheet. These aspects have been detailed in the Discussion section (in the revised manuscript).

P14L35: *“underestimated by simulating.” Unclear.*

“By simulating” should be removed. This error was probably due to an improper “copy-paste”

P15L1: *“surface albedo and strength of katabatic winds.” -> “surface albedo and strength of katabatic winds, with a demonstrably strong return influence on SMB”*

The Discussion section has been entirely re-written and this sentence has been removed from the

original text.

P15L27: “optimal resolution of the ice sheet and the atmospheric model, for ISM-RCM coupling.” While an interesting-sounding statement, I find it also a bit vague: by optimal, do the authors mean something like “of high enough respective resolutions to resolve both important atmospheric and important ice sheet dynamical processes”?

The point here is to find “high enough ISM and RCM resolutions to resolve both important atmospheric and important ice sheet dynamical processes”, while keeping a reasonable computational time. In the revised manuscript, the sentence has been modified by: *“However, a compromise must be reached between the additional computing resources and the required degree of accuracy of sea-level projections”.*

P15L30: “The next step of this study. . .” as described, this is extremely ambitious, with many challenges that outstrip the effort to implement atmospheric coupling. If it is truly a planned next step; great! But if not, I’d suggest not claiming to plan to do this.

In the revised manuscript (Section 5), we rather gave a few examples to illustrate the importance of having a description of the ocean-atmosphere-GrIS coupled system describing the coupled ocean (see paragraph below), but we followed your recommendation and avoided expressions such as “the next step of this study”:

“There is a growing number of evidence for attributing the acceleration of outlet glaciers to the intrusion of warm waters from adjacent oceans in the fjord systems or in the cavity of floating ice tongues (e.g. Straneo et al., 2012; Johnson et al., 2011, Rignot et al., 2015) that can destabilise the glacier front and/or favour the ice-shelf breakup, decreasing thereby the buttressing effect and increasing the ice calving. In turn, the released freshwater flux in ocean may impact sea-surface temperatures, oceanic circulation and sea-ice cover. Moreover, atmosphere-ocean feedbacks also have an impact on the GrIS. As an example, Fettweis et al. (2013) showed that the disappearance of Arctic sea ice in summer induced by ocean warming enhances surface melting in northern Greenland through a decrease of surface albedo and the subsequent atmospheric warming. Thus, the absence of the oceanic component in our modelling setup appears as a limiting factor, although, the direct impact of ocean via sub-shelf melt at the ice sheet margin will likely be limited in the future as a result of inland retreat of GrIS”.

Note however, that MAR has already been coupled to a regional configuration of the oceanic model NEMO (e.g. Jourdain et al., 2011), but applied to the Ross Sea sector in Antarctica. We can therefore reasonably envisage that in the coming years, we will be able to develop a coupled atmosphere-ocean-ice-sheet model.

Jourdain, N. C., Mathiot, P., Gallée, H., Barnier, B. : Influence of coupling on atmosphere, sea ice and ocean regional models in the Ross Sea sector, Antarctica, *Clim. Dynam.*, 36, 1523-1543, doi: 10.1007/s00382-010-0889-9, 2011.

General: while the writing is 100% understandable and clear, a final proof-read by a native English speaker would be useful as a final stage, if possible, to clear up remaining small grammar issues.

We are aware of the fact that many English mistakes and syntax errors appeared in the submitted manuscript. We made a huge effort to improve English writing.