We would like to thank the reviewer 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 Reviewer 2

The paper claims to be focused on assessment of the future of the GrIS through 2150. But in fact, it seems more focused on assessment of a new technique for RCM-Ice Model coupling. Throughout the paper, focuse shifts back and forth between the two. The experiment is to run a future simulation of the GrIS using MAR coupled with GRISLI in three different ways, and then compare/analyze the results from each other.

The coupling method is interesting, but the GrIS is more interesting. I believe the paper would be better if it would keep its focus firmly on the GrIS, while keeping the methods separate. I ultimately want to know, what do we learn about Greenland? Unfortunately, the figures do not really support that. Figures 1-4 do in a way; but the rest of the figures only really tell us about technical differences between coupling technique.

In the revised version, we have largely restructured the manuscript. We now start the result section with a thorough analysis of the GrIS evolution simulated with the most comprehensive method, i.e. the two-way (2W) coupling. The primary focus being now the GrIS evolution, we hope that we have addressed your concerns on this point.

The experiments in the paper show that the different coupling techniques provide different answers. Unfortunately, it is hard to know which answers are closer to the truth, because there are no controls. I came into this believing that the most sophisticated coupler would produce the most melt and also be more accurate; but I had no proof on the accuracy part. This paper has reinforced my prior assumptions, without providing any additional evidence on accuracy. I am therefore hard pressed to say what it has added to my understanding of coupling technique.

All methods, and more generally all models, have their flaws. As stated in the manuscript, both the NF (No Feedback) and PF (parameterized altitude feedbacks) methods (corresponding respectively to NC and 1W in the first version of the manuscript) do not account for the change in atmospheric circulation induced by the change in ice-sheet orography and albedo. The PF method intends to represent the non-linearities of SMB changes with linear corrections based on vertical SMB gradients. Finally, it is fair to say that, compared to the NF and PF methods, the 2W method is the most physically based approach. The two approaches (NF and PF), are inaccurate by construction but have been widely used in the community because of the complexity of including a dynamical ice sheet model in RCMs. Related to your concern on accuracy, we acknowledge the fact that there is only minor constraints to test the validity of our projections: the satellite era covers a relatively short period for which the change in ice sheet topography is small. Thus, although we can state firmly that the 2W method has a stronger physical realism we cannot however guarantee the accuracy of the projections.

I did learn some things about the future Greenland itself, in spite of the figures not really helping with this. I learned:

1. *Expect a steeper slope and stronger katabatic winds, in addition to the expected smaller ice sheet. This will result in colder (not warmer) temperatures near the coast.*
2. *In parts of Greenland, the ELA could be as high as 3000m by the year 2150. I find that idea astounding, at 77 degrees North latitude. Some discussion of this result would be really interesting.*
3. *Expected sea level rise contribution of Greenland in 150 years is 20cm; and the rate of melting will be continuing to rise at that point.*
4. *Ice loss and SMB are highly correllated over the next 150 years; so much so that plots of the two look highly similar. Unfortunately, the paper does not try to quantify the correllation.*

Thanks for mentioning that. In the revised version of the manuscript, these points appear more clearly along with the description of the 2W results in Sec. 4.1. Katabatic winds are discussed in Section 4.2.1.

Concerning the ELA, we did not mention in the original text that it could be situated as high as 3000 m. In the revised manuscript, we added more information about the shift of the ELA towards higher altitudes (see Fig. 3 and section 4.1.1):

"The equilibrium line altitude (ELA, i.e. altitude for which SMB = 0) increases significantly between the beginning and the end of the 2W experiment, as a consequence of increased runoff for areas below 2000 m. As an example, at around 73.5 °N, on the eastern side of the ice sheet, the ELA moves from ~1000 m to ~2500 m (Fig. 3). In other regions, at the end of the 2W experiment, the ELA is generally situated between 1500 and 2000 m high, except in the northern part where it is between 1000 and 1500 m. This shift of ELA towards higher altitudes represents an increase of 24 % of the ablation area between the beginning and the end of the experiment".

Concerning the correlation of SMB with total mass loss, we added more discussion on the role of ice dynamics (see Section 4.1.3). As now shown in this section, ice dynamics act to counteract ice loss from surface melting (see Figs 4 and 5). This was also noticed in previous studies (e.g., Goelzer et al., 2013, Edwards et al., 2014a). In turn, ice dynamics is impacted by changes in ice-sheet geometry (see Fig. 6a).

For the record, here's what I learned about coupling techniques:

1. *Integrating SMB over a fixed ice mask over time is a poor way to calculate total SLR contribution, due to the changing ice mask.*
2. *The 2w case melts more than the 1w or NC case in the RCP8.5 scenario.*
3. *Full Stokes solvers might yield better results.*

Overall... I think this paper has done some interesting modelling runs, but so far has mostly failed to draw interesting conclusions from those runs, and to focus the reader's attention on those conclusions. I would suggest the authors think through the question "What have we learned about Greenland;" and then re-do the figures and commentary to support that learning, and focus the reader's attention on it. The paper will also need significant disucssion of these Greenland results, in comparison with other papers that have looked at the future of Greenland; for example, Vizcaino et al 2015. Especially interesting would be places where this *paper predicts something DIFFERENT from those other papers, and why? In this way, the reader needs to be drawn to focus on the most interesting things — the surprises! — first, without having to dig for them.*

In the revised version of the paper we did our best to organise the ideas following your suggestion, emphasizing on the fate of the GrIS in our projections with the 2W method. We have also added a thorough discussion with existing literature (see in particular Section 4.4 and Section 5). These sections, as well as the Conclusions (Section 6) have been entirely re-written.

Once the paper has focused primarily on Greenland, I would then think about how to add discussion of a new coupling technique, without taking away from the main scientific focus of the paper. But in the absence of any solid provable way to prove that one coupling technique is better than another, I would avoid making too many claims about the 2w coupling; just that you think it is better, and it certainly melts more ice.
In the parts (bulk) of the paper focused on Greenland, I would use whatever coupling technique you think is most realistic.

Here, we do not agree. The 2W method is definitively more physically based than the two other methods and explicitly represents feedbacks that are lacking in NF and PF. For example, the change in albedo in response to ice sheet retreat exerts a major control on local SMB changes that is completely discarded with the two simple coupling techniques. A simpler approach can provide similar estimates for GrIS melt but not always for physical reasons.

A secondary issue: the paper reports many numbers, and only a few of them have error bars. Where did those error bars come from, and why are error bars not reported for other numbers? Would it be possible to get error bars for other numbers?

Because we only have one scenario for each coupling technique we cannot assess statistically the uncertainty in our projections. The +/- signs that you saw for some numbers in the original manuscript stand for the spatial average of the standard deviation for a given variable. For example, in Sec. XX for deltaH=XX+/-YY, the YY is simply the standard deviation in ice thickness change (i.e. the XX value) from the initial condition for a given temporal snapshot. However, in the revised manuscript we now provide the 5th and the 95th percentile values to indicate the range of a given variable.

p.2l.24: Studies by Vizcaino et al (and also at GISS; see Fischer Nowicki 2014) use elevation classes to develop an SMB. Elevation classes are mathematically equivalent to custom-designed gridcells that follow elevation contours. They are therefore able to offer high resolution in the direction of the slope gradient, while continuing with low resolution perpindicular to the gradient.

We agree. Their technique is a way to downscale the SMB from their coarse GCM grid. The technique of elevation classes to downscale SMB is explained in Vizcaino et al. (2013), not in Vizcaino et al. (2015) that was cited in the first version of the manuscript. Following your suggestion, we also mentioned the study by Vizcaino et al. (2013) in the revised manuscript in the Introduction section:

"To circumvent the low resolution, some authors have used the method of elevation classes and are therefore able to offer high resolution in the direction of the slope gradient (e.g. Vizcaino et al., 2013)".

p.8 l.25: I have traditionally used different labels for the different coupling strategies described. Your "NC", I have traditionally called "1-way coupling." Your "2w", I would call "serial 1w coupling". Your "1w coupling," I would call "corrected 1w coupling." Given the differences in terminology, it's probably best to describe what each of your schemes is (which you do), but don't assume that others would use the same names. BTW, none of the coupling schemes here conserve energy, in the sense that two-way couplers (say) between the ocean and atmosphere typically do conserve energy. Therefore, I would be reluctant to call any of them true "two-way coupling."

Following your advice in agreement with the two other reviewers, we renamed the coupling experiments. The experiment with no feedback representation is now called NF (for no feedbacks). The experiment which represents the elevation feedbacks by correcting the MAR outputs is called PF (for parameterized feedbacks). The two-way experiment name remains identical (2W). Since the 2W method does not account for the ocean and since it is based on topography anomalies, we removed all the occurrences of "full two-way coupling" and "fully coupled" replaced them by "two-way coupling".

p.9 l.7: Why is the 2w scheme more expensive? I see that you have to run the GCM and ice model together, rather than separately. But is any more expense actually involved?

In case you do not have an existing MAR simulation, it is true that the 2W is not drastically more expensive than the two other methods since the only difference is the additional time needed by the ice sheet model (negligible compared to the atmospheric model). However, the major advantage of the NF and PF methods is that we can use existing MAR simulations. In this case, we can run multiple sensitivity experiments since only the ice sheet model is run. We have clarified this point in the text:

"[The PF] method offers the possibility to account artificially for the elevation feedbacks when using existing RCM simulations in which the topography is kept constant. As such, it is also transferable to any ice sheet model".

p.9 l.21: Fig. 1 does not support the text. Now I see Fig. 1 is reporting anomalies; but I think it would be more interesting (and no less informative) if it would report actualy Temperature.

Following your suggestions to focus on what happens to GrIS, we start the Result section with a description of the results obtained with the 2W experiment. Therefore, Figure 1 has been changes. It now displays (in absolute values, not anomalies) the evolution of SMB and its components integrated over the whole ice sheet. The spatial distribution of the surface temperature anomaly (2140-2150 vs. 2000-2010) is now given in Fig. 2d.

p. 10 l.2: Cause-and-effect is backwards. Actually, the lower SMB is the CAUSE of the ELA shift.

We totally agree with the reviewer. More precisely, the ELA shift is mainly due to increased runoff (see Fig. 2c). This has been clarified:

“The equilibrium line altitude (ELA, i.e. altitude for which SMB = 0) increases significantly between the beginning and the end of the 2W experiment, as a consequence of increased runoff for areas below 2000 m”.

p. 10 section 4.1.2: This is the one section of the paper with error bars. How were those error bars computed, it didn’t say? Unfortunately, some of the values reported are not statistically significant; and many others are barely. A more clear way to report the reports in this section would be something like "we saw no statistically significant change in the GRISLI ice sheet in the years 2000-2050." This conclusion is already pretty apparent in the figure: the "interesting stuff" happens further out in time, especially with the more advanced coupling.

As stated earlier in response to one of your comment, the values given in the section 4.1.2 of the original manuscript were the spatial averages of the standard deviation. The idea behind these numbers was to have an idea on how geographically different is the variable of interest. However, we agree that these numbers, averaged over the entire ice sheet do not illustrate statistically significant changes. In the revised manuscript, the results are most often discussed as a function of the altitudinal locations. Therefore it does no longer make sense to provide quantitative results averaged over the entire ice sheet. Instead, we often used the 5th and the 95th percentile values, as previously mentioned.

p.10 l.12-24: This looks like an explanation for the increased slope; but I’m not following it.

The increased slopes are simply due to larger and negative SMB changes at the margin relative to the interior. Changes in surface slopes have consequences on ice dynamics with increased slopes leading to increased velocities. We have made substantial text modifications in this paragraph that now reads:

”The changes in local ice dynamics between the first and the last 10 years of the 2W experiment are also related to changes in surface slope and ice thickness, particularly at the margins. To investigate the ice dynamics changes at the local scale, we used the examples of the Jakobshavn (western coast) and the Kangerlussuaq (eastern coast) glaciers for which the fine scale structures of the ice velocity, obtained after the GRISLI initialisation procedure, are relatively well reproduced compared to the observations (Figs 7ab).

For the Jakobshavn glacier, and for altitudes above 1500 m, the vertically-averaged ice velocities increase by more than 15 m yr^{-1} (i.e. +10 %) as a result of increasing surface slopes, and slow down by more than 200 m yr^{-1} (i.e. +29 %) for altitudes below 1000 m due to the decreasing ice thickness (Fig. 7c). For altitudes above 500 m, the vertically-averaged velocity is mainly driven by the SIA velocity (Figs. 7c-e). On the contrary, below 500 m, basal sliding velocities are large due to low basal drag coefficient (see Fig. 3 in Le clec’h et al., 2018) and the SSA velocity component dominates the ice flow (Figs 7c and 7g). However, while basal drag is lower in locations below 500 m, the ice flow is limited by the strongly reduced ice thickness (Fig. 4).

The Kangerlussuaq glacier is located in regions where the bedrock is characterised by a succession of valleys surrounded by mountains merging in a canyon where the deepest part is located 100 km away from the coast (Morlighem et al., 2017). The ice flow of the Kangerlussuaq is therefore divided in different branches with increasing ice velocities towards *the ice sheet margin and becoming even larger when merging in the canyon (Fig. 7b). As for the Jakobshavn glacier, the ice flow accelerates at the end of the 2W experiment as a consequence of the increase in surface slope for high altitudes (~2000-2500 m, see Fig. 4). Conversely, a strong decrease of the ice flow is found in most of margin regions (Fig. 7d) directly related to the ice thinning (Fig. 4). Contrary to the case of the Jakobshavn glacier that presents large basal sliding velocities only below 500 m, the Kangerlussuaq shows low basal drag coefficients in the entire glacier (see Fig. 3 in Le clec'h et al. 2018) and thus the ice flow is mainly governed by the SSA component (Fig. 7h)".*

p. 10: In general, please report ice loss in dual units: both Gt, and mm of sea level rise. If this were done consistently, then section 4.2.3 would barely be needed. Section 4.2: Now, the paper stops telling us about Greenland, and analyzes minute differences between the coupling techniques Not so interesting.

Concerning the units, we adopted the following conventions: Integrated SMB values (over the whole ice sheet) are given in Gt yr^{-1}, while spatially-discretized SMB values are given in m yr^{-1} for consistency reasons with units of ice thickness variations from which our GrIS contributions to sea-level rise are inferred. As such SLR units are in cm.

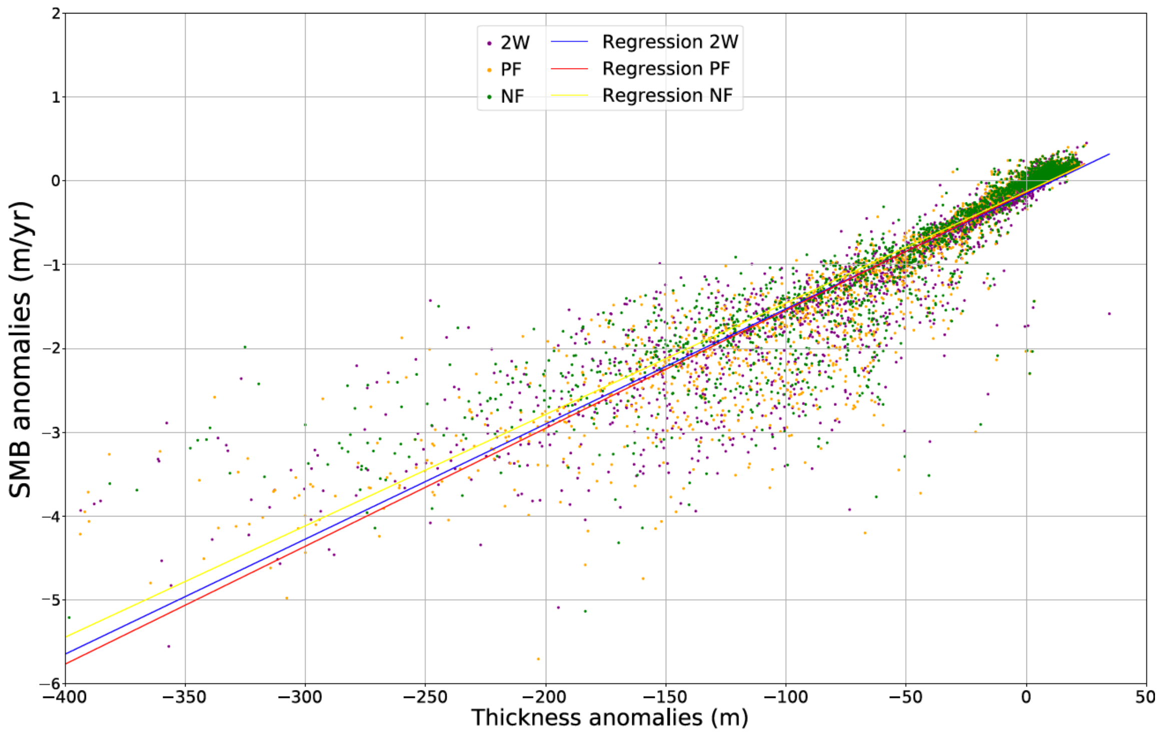
Section 4.4 in the revised manuscript replaces the former Section 4.2.3. This new section has been largely modified. In particular, we added an extended discussion to compare the different experiments and the impact of feedbacks.

p.11 l.20: The word "probably" is used. This indicates a hypothesis; how can that hypothesis be tested?

We observe a strong snowfall increase in the northeastern part of the ice sheet, mainly occurring in autumn (see Fig. S2d), explaining the SMB increase in this region.

p.12 section 4.2.2: Ice thickness and SMB maps are highly correllated throughout this paper. For that reason, section 4.2.2 says pretty much the same thing as section 4.2.1. It would be better to (a) talk about the correllation explicitly, even quantify it, and then (b) keep ice thickness and SMB together in one section every time it is discussed in the results.

We agree with you. Ice thickness and SMB maps show that both are highly correlated as shown with Figures 2a and 4 in the revised manuscript and with the plot displaying the SMB anomalies vs the ice thickness anomalies between the last ten years (2140-2150) and the first ten years of the 2W experiment (see below). The values of the regression coefficients also emphasizes the high correlation between both variables (e.g. for 2W: R^2 = 0.92).



Caption: Surface mass balance anomalies vs the ice thickness anomalies simulated in the 2W experiment. The anomalies are taken between the 2140-2150 and the 2000-2010 mean periods. Solid lines represent the linear regression lines for the 2W (blue, R^2 = 0.92), PF (red, R^2 = 0.93) and NF (yellow, R^2 = 0.92) experiments.

The high correlation between ice thickness anomaly and SMB anomaly shows that climate change due to the imposed RCP forcing is the major control on the Greenland ice sheet geometry change. However, we find important to keep two sections presenting on one hand the changes in SMB and, on the other hand, the changes in ice thickness because it allows to better constrain the role of ice dynamics. Indeed, in our revised version, we show that ice dynamics counteracts the SMB signal (see Section 4.1.3, Fig. 5 and the following paragraph):

"To quantify the role of ice dynamics on the GrIS geometry (Fig. 4), we plotted the ice flux divergence integrated over 150 years (2000-2150, see Fig. 5b). In particular, over the central plateau, the cumulated SMB (Fig. 5a) reaches about +50 m, 40 m of which are transported away by the ice dynamics (Fig. 5b). As a result, the ice thickness anomaly is reduced to only ~10 m in this region (Fig. 4). An opposite behaviour is found near the western coast, where the ice melting is partly compensated by ice convergence, resulting in a less negative ice thickness anomaly than that related to the SMB forcing. This shows that ice dynamics act to counteract ice loss from surface melting, as previously noticed by several authors (Huybrechts and de Wolde, 1999, Goelzer et al., 2013, Edwards et al., 2014b). As a consequence, it appears to be essential to account for ice dynamics to estimate accurately the mass balance of the whole ice sheet".

p.12 l.30: I appreciate that doing wrong calculations will give the wrong answer. I'm glad that you are not doing that. But is this worth half a section to explain? It seems you are going out of your way because someone else did something fishy.

Our study is the first one to provide the GrIS melting projection that makes use of a RCM coupled to an ice sheet model. This means that all the previous studies based on RCMs, did not consider the change in ice sheet mask. We therefore think that this section is particularly relevant for the ice-sheet surface mass balance community. To emphasize the importance of the results, this section has been re-written:

"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".

p. 13 l.2: the last sentence of this paragraph is the most important. Don't "bury the leded"... put it up at the front.

We agree with you. In the revised manuscript, an entire paragraph is devoted to the role of the katabatic wind feedback as simulated in our model. We also added the new Figure 9 to support our findings (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 Figs. S8-S9). Another consequence of the katabatic winds increase due to increased surface slopes in the GrIS interior, is to enhance the atmospheric exchanges along the slope of the ice sheet. The area with lower atmospheric pressure generated by the stronger katabatic winds is filled in by the warmer air coming from higher atmospheric levels in the boundary layer. The warming of the upper part of the boundary layer combined with the lower surface elevation, explains the ST increases in the interior of the GrIS".

p.14 l.15: I don't believe this argument on ice-ocean feedback. We know that tidewater glaciers retreat VERY quickly once they become imbalanced. How many tidewater glaciers will be left for us to simulate in the year 2050, 2100 or 2150? And what about going beyond that — when the REALLY interesting things start to happen? I just don't believe that ocean coupling is very important for GrIS.

We agree with you concerning the high probability of having a decreasing influence of outlet glaciers in the future as a result of increased melting in margin areas. We have outlined this in the discussion section (see section 5). However, it remains difficult to accurately evaluate the time scale at which the influence of outlet glaciers on the whole Greenland ice sheet will be negligible. At the centennial time scale, it is therefore highly desirable to have a good representation of tide water glaciers because they have important consequences on inland ice dynamics. A strong change in ice dynamics could in turn strongly modify the SMB signal and the projected sea-level rise contribution. This process cannot represented in our model because of the too coarse GRISLI resolution. As an example, 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, as mentioned in Section 5. In addition, ocean may exert a strong influence on ice dynamics through the intrusion of warm waters in the fjord system that can accelerate the destabilization of marine terminating glaciers and the subsequent ice discharge. This leads to a release of freshwater flux in the ocean, modifying oceanic circulation, sea-surface temperatures and sea-ice cover and the exchanges at the atmosphere-ocean interface, resulting in fine in SMB changes (due to changes in external forcings). These ideas have been developed in Section 5:

"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".

*** Any idea what happens beyond the year 2150? I know it's outside the scope of this paper. But this paper opens up more tantalizing questions by simulating a non-steadystate process just a little bit of the way — to a point where the changes are continuing to accelerate. What does this simulation look like in 500 years? 1000 years? 5000 years? How important are the feedbacks on that timescale?*

Unfortunately, running the MAR model over such long time scales is out of reach for the time being because of the considerable computational resources it would require. However, from the ice-sheet perspective, we can reasonably expect:

a/ an amplification of the SMB-elevation feedbacks, as suggested by the results presented in this paper (see Section 4.4);

b/ a smaller ice-sheet extent (possibly combined with a larger ablation area) with therefore a growing influence of the albedo effect amplifying warming and surface melting (see Section 4.4);

c/ increased surface slopes favouring thereby (see Section 4.1.3):

> i/ the convergence of cold air in margin areas through the effect of katabatic winds, acting therefore against warming;
>
> ii/ the increase of surface ice velocities in the interior regions.

d/ decreased ice thickness leading to a reduction of ice velocities (see Section 4.1.3)

e/ inland retreat of outlet glaciers resulting in their limited influence on ice dynamics (see Section 5);

f/ Multiplication of melt ponds at the surface of the ice sheet, possibly even in high altitude areas leading to:

> i/ surface albedo reduction;
>
> ii/ increased lubrification and basal sliding.

Of course all the processes listed above should be investigated with a coupled climate-ice-sheet model to investigate their relative influence at different time scales. In addition, atmosphere-ocean-ice sheet feedbacks should also be considered (see our response to your previous comment).

Fig 6A: Why is there a vertical-stripe pattern in western Greenland? That makes me suspicious of the model. Please explain...

This pattern is due to the interpolation method between the coarse MAR grid and the finer GRISLI grid

*** Figures: Please make sure of the following in figures:*

a) Avoid the rainbow color scale in most cases (Fig 4). There are better choices.

We kept the rainbow scales, but we paid attention to the choice of the colour to better illustrate our purpose.

b) If you do use the rainbow, avoid splitting green at zero (Fig 4A). One figure has green for both positive and negative numbers; not cool.

We agree. This has been changed.

c) Avoid a color scale that's read on one end and violet on the other; because then the smallest and largest values look almost the same.

Once again, we agree with you. Colour scales have been changes accordingly.

d) When using color scales with red on one end and blue on the other, make sure that red always corresponds to places that are melting / getting warmer / losing mass; and blue corresponds to the opposite. Reverse the color scale if needed, in order to keep this consistent.
All the colour sales are now consistent: Blue colours correspond to a decrease of the displayed variable, and red colours represent the opposite.

e) The figures in this paper all use different color scales and conventions, for no apparent reason. It looks like they don't belong together. Please make them more uniform, unless there's a good reason for the difference.
We followed your recommendation.

f) Please put a title on top of every plot, in font large enough to read. Make sure that every plot has units on every axis (either the color scale, or the x-y axis. Most fonts on most figures need to be larger.
We have put a title on most figures and the fonts are now larger.