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 3

The two-way coupling between a regional climate model and an ice sheet model is an important development that marks a clear step forward to improve the projections of the future contribution of the Greenland ice sheet to sea-level change. The manuscript compares results of the two-way coupling to former methods of representing the interactions between ice sheet and atmosphere and comes to important conclusions concerning the errors implicit to those simpler approaches. The manuscript is of clear interest to the readers of The Cryosphere but still needs to be substantially improved before being acceptable for publication. I recommend major revisions along the comments outlined below.

General comments

The language of the manuscript needs substantial improvement, because many formulations give rise to misinterpretations of the scientific content. While many mistakes could clearly be avoided with a better command of the English language, a large number of typographical errors and mistakes in the referencing suggest that the authors could have made a better effort to deliver a readable manuscript for the review process.

We apologize for the language of the submitted manuscript and the large number of typographical errors. In the revised version, we did our best to avoid English mistakes and typographical errors. We hope that the new version has become more readable.

The text itself reveals that the models are not actually fully coupled (use of anomaly method) and also gives indications why a full coupling is so much more difficult to achieve. I suggest to adjust the title and modify other occurrences of "fully coupled" in the text to "two-way" coupled to take this into account. A discussion item on this point and next steps that need to follow to work towards truly full coupling between RCMs and ISMs should be included.

Following your recommendation, we changed the title of the manuscript by replacing "fully coupled" with "coupled". Now, the title is "Assessment of the Greenland ice sheet atmosphere feedbacks for the next century with a regional atmospheric model coupled to an ice sheet model". For consistency reasons, we also removed the other occurrences of "fully coupled" in the text. The "fully coupled" experiment (in the first version of the manuscript) is now simply referred to as the "two-way" experiment (2W). In the discussion section (i.e. section 5), we also discussed the limits of this experiment with respect to a real fully coupling method between RCM and ISM.

The ice sheet initialisation procedure is somewhat non-standard and therefore requires a much better explanation. As it is heavily based on former work that is in part not well documented, an additional effort is required to describe the method in a way reproducible for other modellers.

Since the submission of the present paper, the ice sheet initialisation procedure used in this study has been published in *Geoscientific Model Development Discussions* (see https://doi.org/10.5194/gmd-2017-322). The GMDD paper describes in detail the different steps of the method, its sensitivity to various parameters as well as its limitations. We have therefore simplified the description of the ISM initialisation method in the present paper (Section 2.2.2) by only providing the basic principles of the procedure, and we propose to the readers willing to focus on the spin-up procedure to focus on the GMDD paper. However, in our response to reviewers, we tried to reply as clearly as possible to all questions related to the spin-up procedure.

Finally, the evaluation of the method appears to be based on an experiment that is not closely related to the model state actually used for the projections, which may be possible to resolve with an additional control experiment.

We apologize for this misunderstanding. The evaluation of the initialisation procedure is actually based on a 2000-year forward control experiment with constant forcing. In the revised paper, we have clarified the points related to this control experiment. Below, we also provide a detailed answer to the comments associated with this experiment.

The thermodynamic aspect of the model is not well represented, arguably because it plays a minor role for the present work. Nevertheless, substantial computing time is spent during initialisation to equilibrate the temperature and the role of bottom and surface boundary conditions is mentioned. Therefore, the model description in 2.2 requires at least a short description of this model component.

Following your recommendation, a short description of the thermodynamic aspect of the ice-sheet model has been added in section 2.2 with the following paragraph:

"*Basal melting occurs when the basal temperature is at the pressure melting point. The ice temperature plays a crucial role in the dynamics of the ice sheet because it also affects the viscosity, and thus the ice flow in the entire ice column (Ritz et al., 1997, 2001). In turn, heat released by internal ice deformation and basal dragging over the bedrock modifies the temperature. The temperature field is computed by solving a time-dependent heat equation both in the ice and in the bedrock accounting for advection and vertical diffusion processes. At the surface, the boundary condition is provided by the prescribed surface temperature. At the base of the ice sheet, the boundary condition is given either by the geothermal heat flux or by the temperature melting point at the ice-bed interface.*"

The experiment names are not specific enough and should be improved. For my understanding, what is presented as the method "no coupling" is in fact a one-way coupling, where the ice sheet is responding to changes computed by the RCM (with "no feedback"). 2-W is correctly described, but 1-W is somewhere between one-way and two-way coupling because it parameterises the feedback. Maybe you could use "no feedback", "parameterised feedback" and "two-way" instead.

We followed your recommendation and changed the name of the experiments referred to as NC and 1W in the first version of the manuscript. These experiments are now referred to as NF (for "No Feedback") and PF (for "Parameterised Feedbacks"). NF corresponds to the experiment in which GRISLI is forced by the MAR climate, and PF corresponds to the experiment in which both SMB and ST fields simulated by MAR are corrected to account for topography changes simulated by GRISLI. In our revised manuscript the name of the two-way coupling method (2W) has remained unchanged.

The most important question in the comparison of results after 100 years and after 150 years is left open: why does the behaviour of 2-W suddenly change around 2010. For this it may be instructive to also look more detailed at around 2060, where a similar shift is possibly visible.
In the first version of the manuscript we insisted on the behaviour of the 2W experiment after year 2100. However, a closer examination of the results clearly shows that the evolution of the Greenland ice sheet to sea-level rise diverge from one experiment to the other as soon as 2025-2030, namely only a few years after GrIS-atmosphere feedbacks are accounted for in the 2W experiment and in the parameterized feedback experiments (SMB-elevation feedbacks in this case). As a result, while the effect of the feedbacks on sea-level rise becomes significant by the end of the 21^{st} century, it starts to operate much earlier and is amplified over time. To illustrate this point, we added a –zoom-panel in the new Figure 12 displaying the evolution of the anomalies (2W-NF, PF-NF and 2W – PF) of the GrIS contribution to SLR.

Otherwise, I find the comparison redundant because the bottom line in most cases is 'like after 100 years, only stronger'.
We fully agree with this remark. In the revised version of the manuscript, we mostly present the results at the end of the simulations (i.e. 2150) and we only discuss the temporal evolution, including the results by 2100, only in Sect. 4.4.

The integration of SMB anomalies already discussed in the manuscript could be added as an additional experiment, possible even two, if masking would be additionally taken into account. This would facilitate the comparison and place the discussion of the effect of masking on firmer ground.
Estimates of sea-level rise from the time integral of SMB anomalies were already discussed in the submitted manuscript. In the revised version they are referred to as SMB_{MSK-NF} and SMB_{MSK-2W}. Both are based on the SMB outputs from the NF experiment (at the MAR resolution), but the time integral of SMB anomalies is made either on the fixed present-day ice-sheet mask (SMB_{MSK-NF}) or on the time variable ice-sheet mask simulated in the 2W experiment. The results are discussed at the end of section 4.4. However, since these estimates are inferred from diagnostics of already performed experiments (i.e. NF and 2W), we think it is not appropriate to present them as additional experiments.

Title I would argue that the models are not "fully", but rather "two-way" coupled because an intermediate down-scaling step is necessary and, more importantly, an anomaly method is used.
To avoid any confusion, we changed the title in:

“Assessment of the Greenland ice sheet - atmosphere feedbacks for the next century with a regional atmospheric model coupled to an ice sheet model”.

*P1.L1 Better "the projected sea-level contribution from the *Greenland ice sheet*". Also mention a typical time scale here to make clear this is about the centennial time-scale.*

The first sentence now reads as:

“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 at the centennial time scale”.

In the main text we use *“the GrIS contribution to sea-level rise”* or, following your suggestion, *“the projected sea-level rise contribution from the Greenland ice sheet”*.

*P1.L2 Be more precise about the mechanisms and feedback(s). The next sentence ("these feedback*s*") suggests that "temperature and surface mass balance – elevation feedback" refers to at least two feedbacks. What are these precisely? "surface mass balance – elevation feedback" is clear, but what is the role of temperature? Note also that melting is clearly related to temperature increase, but the SMB is ultimately controlled by the energy balance.*

The abstract has been extensively modified. The sentence you mentioned has been changes in:

”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”.

P1.L5 A bit confusing to mention start date as 2020. It is understood later that before 2020 elevation changes are considered too small to make a difference. But at this place it may be better to give the period of the entire simulation (2006 - 2150). Note also that the RCP is not defined beyond 2100, so it is better to mention "prolonged RCP 8.5 scenario".

Recommendation followed.

P1.L5 It seems confusing to call this simple method "no coupling", since it represents a one-way coupling. See also general comment on naming the experiments.

As advised we have change the name of the experiment: No coupling (NC) becomes No Feedbacks (NF) experiment and one-way coupling (1-W) becomes Parameterised Feedbacks (PF) experiment.

P1.L6 *Could mention that this one-way coupling methods attempts to incorporate or parametrise two-way interaction. It represents an intermediate method between one-way and two-way coupling. See also general comment on naming the experiments.*

In the abstract, we first present the two-way coupled approach, and then the one-way coupling experiment (i.e. NF). The parameterised feedback experiment is then defined as an *“alternative one-way coupling approach in which the elevation changes feedbacks are parameterised in the ice-sheet model*”.

P1.L7 *I suggest to omit "offline". The correction may be offline to MAR, but it is online to the ice sheet model, as the correction is updated every time step and dependent on the current ice sheet* *elevation. Could add what is happening with the extent, since it has been explicitly mentioned for the former method.*

We agree, it's offline to MAR but not for GRISLI. The ice sheet extent is not updated in the one way parameterised coupling method (PF). Only ice sheet topography changes computed by GRISLI relative to the observations are used to correct the SMB fields.

In the revised version of the manuscript we do not mention explicitly what happens with the ice sheet extent in the PF experiment, but we explain that only the surface mass balance - elevation feedbacks are parameterised, which implies that the ice-sheet extent in the atmospheric model is kept constant as in the NF experiment.

P1.L9 Clearer to replace "ice sheet elevation feedback" by "surface mass balance – elevation feedback".

"Ice-sheet elevation feedback" has been removed from the entire text in favour of SMB-elevation feedback, melt-elevation feedback or temperature-elevation feedback, depending on the context.

P1.L9 Maybe ", the one-way and two-way coupling methods ..." since the amplification occurs in both cases.

We agree. In the revised manuscript, this part of the abstract has been completely reorganised but we paid attention to make clear that SMB-elevation feedbacks are amplified over time both in the PF and the 2W experiments.

P1.L11 Some ice sheet margins are not in the coastal region. Replace by "ice sheet margins" or similar. This should be followed throughout the document for other occurrences.

Recommendation followed.

P1.L15 "52 400 km^2 smaller"

In the revised version of the manuscript, we discuss the relative changes (in %) in ablation area (rather than changes in ice-sheet extent) between NF vs 2W and PF vs. 2W.

P1.L16 "fixed ice sheet mask"

OK, modified

P1.L20 "always" is only true for the end of the simulation. In the first decades or so the volume loss difference cannot be significant. Maybe give an estimate for a time scale where this is true similar to the comparison one-way vs. two-way.

In the revised version of the abstract, we just mention that the effect of feedbacks is amplified over time. However, in Section 4.4, we specify that the feedbacks make the three simulations diverging from each other only a few years after taking into account the feedbacks (i.e. after 2020). This is illustrated in Figure 12. However, we also mention that the effect of the feedbacks becomes significant only after the end of the 21st century.

P2.L6 "the ablation" (singular) <–> "are processes" (plural). Reformulate

OK, this has been reformulated.

P2.L6 Some risk for confusion here. It is a bit simplistic for a paper discussing an RCM as an important component to reduce the interaction to changes in SMB and temperature. It is understood that these are the two variables used to force the ice sheet model, but a bit more detail is required. How does the change of ice extent change the albedo and therefore the energy balance?

In the revised version, our arguments have been a bit more developed. The first paragraph of the Introduction has been modified as follows:

"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. 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 Refrew, 2005) causing changes in heat and humidity transports".

Does temperature enter the correction method and how?

Yes, both ST and SMB, used as forcings of the ice-sheet model are corrected in the PF experiment, using the Franco's et al. (2012) method. In the 2W experiment, they are explicitly computed as a function of the evolving topography (computed by GRISLI), following the same procedure as in the PF experiment.

OK, accumulation and ablation are sensitive to ST, but why and how?

Processes have been clarified in the modified paragraph reported above

Also, what is the role of ST other than its influence on SMB, as boundary condition to ice thermodynamics? Does it have an impact on the simulations at all (I don't expect it, but would be good to say something about why not and being able to exclude it).

The surface temperature (ST) applied as a boundary condition of the ISM allows to compute the vertical temperature profile (i.e. the surface conditions diffuse from the surface to the base of the ice sheet and modify the ice flow by changing the viscosity of the ice). Using ST as a climate forcing is therefore a pre-requisite to run the ice-sheet model. However, at the century timescale, ST has not time enough to diffuse farther than the surface layer. Thus, in the present study, changes in ST during the 150-yr experiment have only a very limited impact on ice dynamics.

P2.L6 Maybe already intended, but make really clear that the changes in ST have no direct effect on thickness volume and extent. Reformulate.

We acknowledge that this sentence was not clear. The overall paragraph has been reformulated (see above).

P2.L9 Replace "disrupt" by "modify"
OK, modified

P2.L19 More detail needed. Amplification of mass loss by what process under what forcing and compared to what other (control) experiment?
We clarified these points in the revised version:

"Compared to a control experiment in which the ISM is forced off-line by the atmospheric model run with the fixed present-day GrIS topography, they found an amplification of ice mass loss of 8–11 % and 24–31 % in 2100 and AD 2300 respectively, when the elevation feedbacks are taken into account (i.e. in the coupled experiment). This results from the combination of the positive elevation-SMB feedback in low lying areas, the negative feedback related to the elevation-desertification effect in accumulation areas, and the changes of surface slopes resulting from high mass loss in ablation areas and slight snowfall increase in the accumulation zone, enhancing the ice transport from the central regions to the ice margins".

P2.L22 The beginning of this sentence suggests (and I agree) that increased resolution would help to improve the modelling compared to observations, while "more detailed physics" is at least for the ice sheet model typically associated with 'less approximation', i.e higher order physics. Could you add some detail to distinguish these.
This sentence has been completely reformulated. In particular, we specified that the ISM (SICOPOLIS) used in Vizcaino et al (2015) is based on the shallow ice approximation and is therefore not able to properly capture fast flowing of outlet glaciers. As suggested by Reviewer 1, we also mentioned the study of Löfverström and Liakka (2017) who confirmed the importance of the spatial resolution in coupled climate – ice sheet experiments in a paleo-climatic context. They explain that ISM results are limited by the capacity of the climate model to simulate atmospheric temperature and precipitation at low spatial resolution as a consequence of the poorly resolved planetary waves and smooth topography.

P2.L26 *Should introduce RCMs and add references to MAR, RACMO, HIRHAM ... already here, as that is the obvious choice to increased resolution. Introducing the Franco and Edwards methods is already a step further as it is based on RCM output.*
As recommended we have firstly introduced RCMs with references for MAR (Fettweis et al. 2017), RACMO2 (Noël et al., 2015), Polar MM5 (Box et al. 2013) and HIRHAM5 (Langen et al. 2015). We then mentioned the altitude corrective methods of Franco et al. (2012) and Edwards et al. (2014b).

P3.L3 Sentence misses references for examples of RCMs.

We added the same references as those mentioned in our response to the comment P2.L26 (see just above).

P3.L9 Specify again for what it is a requirement.
The sentence has been modified as follows:
"The second fundamental requirement to describe the interactions between atmosphere and GrIS 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".

P3.L10 Reformulate "usually used" to "typically used" or similar.
This has been reformulated (see the sentence reported just above in our previous answer).

P3.L11 Add reference to Goelzer et al. 2017 here, since it is specifically on GrIS models.
Sorry for this omission. The reference has been added.

P3.L18 Remove "high resolution" or specify explicitly at what resolution GRISLI is run.
We removed "high resolution" from the sentence and specified at what resolution MAR and GRISLI are run in section 2.

P3.L21 "two-way"
OK modified

P3.L25 I would consider the three methods part of the experimental setup and therefore name initialisation and experimental setup first.
The paragraph describing the organisation of the paper has been reformulated according to the new structure of our revised version. Section 2 (entitled Models) describes the atmospheric and the ice-sheet models together with their respective spin-up procedures and boundary conditions. Section 3 (entitled Coupling methods) is now focussed on the description of the three coupling methods.

P4.L4 "developed". Correct also throughout the manuscript.
OK corrected everywhere

P4.L4 "SISVAT" requires a reference and description of the acronym.
OK specified.

P4.L16 "ice albedo that has been improved by parametrising the impact of melt ponds on the albedo."
OK corrected.

P4.L19 Replace "provided by" by "taken from"
OK corrected

P4.L20 "forced with 6-hourly atmospheric fields". See also P8.L6
OK corrected.

P4.L24 Remove "forcing"
OK removed

P4.L27 Suggest reformulation to "... because it has been shown by Fettweis et al. (2013), to be the best choice from the CMIP5 data-base to reproduce the present-dayclimate compared to results of MAR forced by reanalyses."
OK, modified.

P5.L1 Heading "Climate model initialisation and experiment"

Section 2 (and its related subsections) has been reorganised following the recommendations of the three reviewers and heading is now "Models" This section is still divided in two subsections 2.1 and 2.2 devoted to the description of the MAR and GRISLI models respectively. Section 3 is devoted on the description of the three "coupling" experiments.

P5.L2 What is the difference between "spurious drifts" and "unwanted trends" or are they one and the same? Reformulate.
We apologise for this misunderstanding. We used two different expressions to deal with "unwanted trends". The sentence has been reformulated as:
"Before starting our experiments, MAR needs to be properly initialised to limit unwanted trends in the results".

P5.L3 Replace "SISVAT requires more than 6 years", by "SISVAT requires less than 7 years" to make clear that the chosen 7-year period is long enough. Or otherwise explain why 7 years is considered OK.
OK modified.

P5.L5 Replace "provided by" by "taken from". Add explanation how the data was interpolated to the coarse MAR grid.
We replaced "provided" by "taken from" as suggested.
In the revised version, we specified that the GrIS topography from the Bamber et al. (2013) dataset is aggregated on the MAR grid.

P5.L5 Be consistent in if SISVAT is written in italic or not.
OK corrected

P5.L6 Replace "following year 1976" by "from 1977 onward".
Sorry for this misunderstanding. In the revised version we clarified that MAR is initialized with MIROC5 climatic fields from 1970 to 1975 included. The MAR simulations start in 1976, but the results presented in this paper are for the period 2000-2150. This has been clarified in the revised manuscript. Therefore we changed the sentence in:

“Here, MAR is initialised with the atmospheric forcing fields from MIROC5 from 1970 until 1975 and the MAR simulations start in 1976. However, in this paper, the MAR results will be analysed for the period spanning from years 2000 to 2150”.

P5.L10 Need to explain in more detail why 2095 can be considered representative for the 2090s. Is it e.g. the year that is closest to the decadal mean? Are trends so linear that the middle of the decade are representative for the average? Typically one would use the decadal mean to represent the long-term average and not one individual year, unless it doesn’t matter for some reason.

We chose to force MAR with the 2095 climate from 2101 to 2150 because, averaged over the entire GrIS, the 2095 climate is one the closest to the decadal 2090-2100 mean climate. We acknowledge that, in the absence of a MIROC5 simulation run under a prolonged RCP8.5 scenario it would have been more appropriate to repeat the ten years (2090 -2100) until 2150, but it would have been more complex to set up.

P5.L12 Better to omit "coupled" here, since it is not clear what is coupled to what and it is further detailed later.

OK, “coupled” has been removed.

*P5.L15 "the northern hemisphere ice sheet*s* (NH references) and the Greenland ice sheet (GrIS references)". or "the northern hemisphere ice sheet*s* and the Greenland ice sheet (all references)".*

OK modified according to the suggestion.

P5.L16 "... covering Greenland with ...", since the coverage extends outside of the ice sheet mask. Add information about the vertical.

We have also specified that GRISLI has 21 vertical evenly spaced levels.

P5.L17 Need to specify what "hybrid" means.

The word "hydrid” has been removed from this part of the text and introduced after having explained the basic principles of both the shallow-ice and the shallow-shelf approximations: *“Using a hybrid model (i.e. based on both SIA and SSA approximations) allows to better represent the different deformation regimes found in an ice sheet”.*

P5.L19 Need to add explanation on the thermodynamic aspect of the model.

We added new information on the thermodynamic aspects:

”Basal melting occurs when the basal temperature is at the pressure melting point. The ice temperature plays a crucial role in the dynamics of the ice sheet because it also affects the viscosity, and thus the ice flow in the entire ice column (Ritz et al., 1997, 2001). In turn, heat released by internal ice deformation and basal dragging over the bedrock modifies the temperature. The temperature field is computed by solving a time-dependent heat equation both in the ice and in the bedrock accounting for advection and vertical diffusion processes. At the surface, the boundary condition is provided by the prescribed surface temperature. At the

base of the ice sheet, the boundary condition is given either by the geothermal heat flux or by the temperature melting point at the ice bed interface".

See also P5.L27 SIA velocity is even stronger controlled by ice thickness.

Both the ice surface slopes and the ice thickness occur in the computation of the SIA and the SSA velocity with the same exponent. We therefore modified the sentence as:

"The ice thickness and the ice-sheet surface slopes control the SIA and the SSA velocity components, but the SSA is also governed by basal dragging"

P5.L28 "SSA component is mainly controlled by the ice flux" is confusing because ice flux is velocity x ice thickness. Clarify!

This has been clarified and corrected in the revised version (see our previous response).

P5.L29 "rheologies" is the wrong term here. Maybe "deformation regimes".

Yes, you are right. We replaced by "deformation regimes"

P5.L30 Replace "ice melting point" by "pressure melting point"

OK, corrected

P6.L3 Replace "floating criterion" by "floatation criterion"

Ok, corrected

P6.L4 What does "characteristics of the Greenland bedrock" mean? Explain

We acknowledge that this expression was too vague. It referred to the nature of the bedrock (i.e. water-saturated sediment or not). However, this part of the manuscript has been re-written and the sentence has been deleted.

P6.L5 Does that mean the enhancement factor differs for different regions? Explain.

Alike most ice-sheet models, GRISLI considers the ice as a non-Newtonian viscous fluid that follows the Glen's flow law (with the coefficient n generally fixed to 3). However, a particularity a GRISLI is also to account for a Newtonian contribution (i.e n =1) for low deformation rates leading to a polynomial Glen's flow law in which we apply an enhancement factor in SIA areas to favour longitudinal deformations. In addition, a fixed ratio between the SIA and the SSA enhancement factor is used. The polynomial Glen's flow law is expressed as :

$$\frac{1}{\eta} = (E_1\, B_1\,(T) + E_3\, B_3\,(T) \cdot \tau^2\,)\tau'_{ij}$$

where η is the ice viscosity, τ is the shear stress tensor and τ'_{ij} is the deviatoric stress tensor, B1(T) and B3(T) are temperature-dependent coefficients following and Arrhenius equation for coefficients n= 1 and n= 3 respectively and E1 and E3 are the corresponding enhancement factors. As a result there are theoretically four enhancement factors (2 for the SIA component of the velocity with n= 1 and n=3 and 2 for the SSA component of the velocity). In practice, for the simulations presented in this paper, we used $E_{1\_SIA} = E_{3\_SIA} = 1$ and $E_{i\_SIA}/E_{i\_SSA} = 0.125$.

After a careful examination of the paper and reviewers comments, we do believe that any mention to the enhancement factor does not provide any added value to the manuscript. We therefore removed the corresponding sentence.

P6.L6 "ice loading changes"
OK, corrected.

P6.L7 Add a reference for the used isostatic model.
OK, Le Meur et al. (1996) added for the ELRA model.

P6.L8 Add a reference describing the thermodynamic model.
As previously mentioned, we added a new paragraph to describe the thermodynamic aspects of the GRISLI model and added the references Ritz et al (1997, 2001).

P6.L10 This whole paragraph needs to be reworked. Be more specific. What is considered a boundary condition, what is input data and what is considered a forcing? What variables are concerned for ice flow, ice thermodynamics and isostasy?
We acknowledge that this paragraph was very confusing. In the revised version, subsections 2.2.1 is now devoted to the description of the GRISLI ice-sheet model and section 2.2.2 is focused on the spin-up procedure. Following your recommendation, the paragraph concerning climate forcing, initial conditions and input data has been entirely re-written. Now it reads as: *"The climatic forcing is given by the mean annual SMB and the mean annual ST. Because seasonal variations of surface temperature are rapidly dampened, ST is considered as a good approximation of the bottom snowpack temperature. The initial GrIS surface and bedrock topographies come from Bamber et al. (2013) and the geothermal heat flux is taken from Fox Maule et al. (2009)".*

P6.L11 Is there are a difference between "The annual mean near surface air temperature" and ST? If yes, explain, if not, use TS instead.
No, there is no difference: ST represents the mean annual near surface air temperature. This has been clarified in the new version of the manuscript.

P6.L13 What data are these 'boundary conditions' and which variables are taken from which data set? Surface elevation, bedrock elevation and ice thickness are not boundary conditions to the equations that GRISLI solves in the proper sense. You could call this "input data" instead.
As mentioned above, we clarified the text.

P6.L14 "The climatic forcings". Say what they are! TS and SMB?
The climatic fields used as GRISLI forcings are the SMB and the ST. This has been clearly specified in the new version.

P6.L15 If basal drag were a boundary condition, it could hardly be computed. Reformulate to make this clearer.

The basal drag coefficient is only adjusted during the initialization. In forward experiments, it remains constant through time and its spatial distribution is fixed to that obtained at the end of the initialisation. It can be thus considered as an input data, at least for transient experiments.

P6.L18 Heading "Ice sheet model initialisation and experiments"
As previously mentioned this sub-section has been canceled and the text has been moved to the main section 2.2

P6.L19 The motivation is not quite correct. I would argue that to equilibrate the model to a steady state is not a necessity given the approximations, but rather a choice. One could envision a transient spinup as initialisation with the exact same model.
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"*

P6.L20 Again, more precision needed. What the ice sheet model equilibrates to is rather the climate forcing held constant for this particular initial steady state experiment.
The text has been modified as follows:
"the aim of the initialisation is to start the simulations from a present-day ice sheet geometry as close as possible to the observed one while ensuring consistency between internal properties of the ice-sheet (e.g. basal sliding velocities and vertical profile of temperature) with the climate forcing".

P6.L20 Replace "sensitivity" by "forced" or "forward".
We replaced "sensitivity" by "forward".

P6.L20 Reference Le clec'h et al. (in prep) is not in the reference list. If you are referring to the present manuscript, say that instead of using an external reference.
No, we did not refer to the present manuscript. We simply omitted to add the reference *Le clec'h et al. (in prep)* in the reference list. This paper describes in details the initialization procedure. Since the submission of the present manuscript, *Le clec'h et al. (in prep)* has been published in GMDD. In the following (as well as in the revised manuscript) it is referred to as *Le clec'h et al. (2018)*. As a result, in the revised version of the present manuscript, this reference appears as *Le clec'h et al. (2018)*. Moreover, we made the choice to only present the basic principles of the initialisation procedure to avoid redundancy with the GMDD paper.

P6.L22 Replace "avoid" by "reduce", since the method is not perfect. Also I'd suggest the formulation ". reduce an initial adjustment of the model during the first years of the simulation due to factors not related to the climate forcing alone." or similar.
In the revised version, we no longer speak about *"an initial adjustment of the model". Instead, we explain that the aim of the initialization procedure is to "reduce the difference between the observed and the simulated ice thickness".*

P6.L25 Reformulate "just over the bedrock". Maybe "basal conditions".

We agree with this suggestion. However, as a result of the simplified description of the initialisation procedure, the corresponding sentence has been removed in the new version of our manuscript.

*P6.L26 If basal conditions are "likely to change in time" your method to define spatially variable but *temporally fixed* basal drag coefficient could never be successful. Should add here that your method assumes them to be constant over the 150 years of your experiment.*

This is actually a limitation of the method and this is why it cannot be applied for long-term transient experiments. However, over the 150 years of the experiments, we assume that basal conditions do not change so much and that the best guess for the basal drag coefficient obtained at the end of the spin-up procedure is a good approximation of the basal dragging at the century time scale. Because of the simplified description of the spin-up procedure, this part of the text has been removed. However, we specified in the revised manuscript that *"β is a time constant but spatially variable basal drag coefficient*".

P6.L27 Suggest to remove sentence "As a result any error in the basal velocity computation can spread vertically in the ice and generate slowdown or acceleration of ice sheet motion." In its present form this sentence is generally true in any case and doesn't support your chose of assimilation method.

We followed this suggestion and removed the sentence.

P6.L29 It is not clear to me at what point in the procedure observed velocities are actually used. Which observational data set is used? Reference needed.

The observed velocities (Joughin et al., 2010) are only used as input data for the first iteration. The actual target is to reduce as best as possible the mismatch between the observed and the simulated ice thickness.

P6.L30 "three main steps:" Make a numbered list (possible with lists of sub-steps) to facilitate navigation of the different steps.

We acknowledge that is part of the text was not well written and contained several misleading formulations. As explained above, the presentation of the spin-up method has been reduced to its basic principles (see section 2.2.2) in this revised paper because the full description of the method can be found in Le chlec'h et al. (2018).

P6.L31 Replace "not necessary consistent between them" by "not necessarily mutually consistent".

Recommendation followed.

P6.L32 It looks to me like the first guess of basal drag mentioned here is a very good first guess and further adjustment of the basal drag coefficient is very much based on it. At any rate, a full description of the procedure used to arrive at that stage should be included, otherwise the method is not reproducible with another model (and not even with GRISLI itself). See also general comment on initialisation.

The first guess of the basal drag coefficient comes from a preliminary version of the spin-up procedure summarized in the present paper and fully detailed in Le clec'h et al. (2018). This former procedure was set up for Ice2Sea simulations carried out for with exactly the same GRISLI model version and the same initial Greenland ice-sheet topography (Bamber et al. 2013) as those used in the present study, but with a different climate forcing, implying the need for adjusting the basal drag coefficient. Furthermore Le clec'h et al. (2018) have shown that the final value of the basal drag coefficient (i.e. used for forward experiments) obtained after the spin-up procedure is very poorly dependent on the initial guess (see Fig. 3 in the GMDD paper).

P6.L32 Edwards et al is a multi-model intercomparison and does not give specific details on the assimilation technique for GRISLI. The model reference there is given as Quiquet et al., 2012), which does not provide information on spatially variables tuning. Again, the method to produce the first guess basal drag needs to be made transparent for other modellers to be able to reproduce the results.

The reviewer is right concerning the reference Edwards et al (2014a). This was cited to inform the reader that the initial guess of the basal drag coefficient was coming from the Ice2Sea project. We acknowledge this was not appropriate since this paper does not contain any detail about the assimilation technique. In the revised manuscript, we provide a piece of information about the method used to obtain this first guess (see our previous response).

P6.L32 "surface and bottom". I think you mean surface elevation and bedrock topography. Be more specific!

This is right. This part of the text has been reformulated (see our response related to the main steps of the initialization procedure).

P7.L1 "vertical fields" Be more specific!

We dealt with the vertical and temperature profiles. Once again, this part has been reformulated.

P7.L2 If I understand correctly, you calculate something here in the first step to be used in the second step. Maybe you should say that. Confusing to mention already here "to have an ice flux as close as possible to observation" when diagnostically calculating something here will not have any influence on the match of the ice flux with observations in this step. This could be mentioned in the second step or as a general motivation for your method before.

Completely reformulated to make clearer the description. Indeed, the basal drag coefficient computed during the 1st step (see new description) is used in the 2nd step. This has been specified in the new version.

P7.L3 Not clear to me how to derive a factor (a/b) from a difference (H1-H0). Please provide an equation or better explanation what the underlying idea is, what is done here, and how it is calculated?

The revised manuscript includes equations supporting the spin-up description. We hope this will help to avoid any ambiguities.

P7.L4 *You are mixing topography differences and ice thickness differences. Possibly similar or identical in absence of bedrock adjustment, but is it necessary to distinguish them?*
Since bedrock adjustment is negligible in the present study (owing to the addressed time scales), surface elevation differences and ice thickness differences are supposed to be very similar. However, to avoid any confusion, we only use the term “ice thickness” throughout the revised manuscript.

*P7.L4 Again "the factor allows to decrease (resp. increase) the surface ice velocity" is confusing, because this is not happening in this first step. Also "If *locally* the topography difference *is* positive ..."*
The previous description was misleading. In the revised manuscript, the first step consists in computing a new value of the basal drag coefficient from the value obtained at the previous iteration and from the ratio of the sliding velocity over the corrected sliding velocity. This ratio represents the corrective factor to reduce the mismatch between observed and simulated ice thicknesses. The way the corrected sliding velocity is computed is now fully described in the revised manuscript. For the very first iteration (i.e. just after the 5-year relaxation), the first step is skipped because there is no difference between observed and simulated ice thicknesses and the procedure starts at the second step. This has been also specified in the new version of the paper.

P7.L5 How does deltaH translate into deltaV?
The relationship between the ratio of H^G/H^{obs} and the ratio of the vertically averaged velocity is given by Equation 4.

How does the new velocity compare to observed surface velocities?
There was a confusion in the revised manuscript when dealing with surface ice velocity. Actually, surface ice velocity have to be replaced by “vertically-averaged velocity”. As a result, we do not compare the new surface ice velocities to the observed ones in the present paper. However, this comparison can be found in the GMDD paper (see Fig. 8 herein): we show that the overall patterns of the simulated ice surface velocities are generally in good agreement with observations (particularly in regions of fast ice flows), despite slight differences in the central plateau where the ice velocities are low.

P7.L11 How is the new coefficient calculated? Explain in detail.
The description of the method has been clarified in the revised manuscript and the way the new basal drag coefficient is calculated has been explained in detail (see the new section 2.2.2 in the revised paper).

This is reminiscent of the method of Pollard and DeConto 2012, could you describe the similarities and differences to their approach?
The reviewer is right. In the revised manuscript, we specified that our spin-up method is based on the same basic principles as that of Pollard and Deconto (2012) in that their basal sliding coefficient is adjusted so as to reduce the difference between simulated and observed ice-sheet

topography. We also mentioned the main differences between their method and ours. The new paragraph is:

"Based on the same basic principles as that of Pollard and DeConto (2012), our method consists in the adjustment of the spatially-varying basal drag coefficient (and thus of the basal sliding velocities, see equation 3) so as to reduce the difference between the observed and the simulated ice thickness. However, while the study by Pollard and DeConto (2012) requires long (multi-millennial) integrations for the method to converge, we suggest instead an iterative method of short (decadal to centennial) integrations starting from the observed ice thickness".

Surprisingly your adjustment goes very fast (in total less than 2000 years). This makes me believe that the original basal drag was already a good guess and you only need minor adjustments. Is that correct? How different is the final basal drag field from the initial one? Can we see a figure for this comparison?

As previously mentioned, we have shown in the GMDD paper that the convergence of our spin-up method is only poorly dependent on the choice of the original basal drag coefficient. Sensitivity tests performed with a uniform β coefficient (β=1) and with the same spin-up parameters (i.e. 20 years for the duration of each iteration, 200 years for the free-evolving simulations and Nbcycle = 8) results in negligible differences in the final basal dragging compared to that inferred from our "standard method" (i.e. a first guess for β coming from Ice2Sea simulations), and in an ice thickness root mean square error (+ 62 m) fully comparable to that obtained in the present study after 8 cycles (+ 63 m). These results are illustrated in Figure 3 in the GMDD paper. Hence, they are not reported in the present study.

P7.L15 Replace "minimum gap" by "error".

This part has been reformulated. Throughout the manuscript we use "mismatch" or "differences" between simulated and observed ice thicknesses.

P7.L15 You additionally need to convince the reader here that this method is optimal in the parameter choices (adjustment time 20 y, relaxation time 200 years) and to make clear in how far the results are (not) dependent on these choices.

The results (in terms of time of convergence and ice thickness root mean square error) are obviously dependent on the choice of the adjustment and relaxation time and of the number of cycles. As explained below (see response concerning the stopping criterion) they have been chosen to minimise the ice thickness RMSE. This has been mentioned in the revised paper. Numerous sensitivity studies with different sets of parameters have also been carried out and presented in Le clec'h et al. (2018). In the revised paper, we specify that: *"The overall process is stopped when the ice thickness root mean square error is not significantly improved. This ensures a good compromise between the reduction of the mismatch between observed and simulated ice thickness and the rapidity of the convergence of the spin-up method. In the present paper, the number of cycles that provides the best fit with observations (RMSE = + 63 m) is Nb_{cycle} = 8".*

P7.L20 After each step you have "a new set of initial conditions" for the next step. Maybe better to only name the final result of your initialisation your initial state as input for the forward experiments.
Completely reformulated

*P7.L21 After 30 kyr, T is in equilibrium with the climate *and with the fixed geometry*, but not the other way around. In the next step of retuning basal drag, you further evolve the geometry and the ice temperature? Could you quantify, give an estimate how far from equilibrium you are now? Why could you not run (part of the initialisation) with freely evolving temperature?*
We apologise for the confusion. Actually, there is no temperature equilibrium in the spin-up procedure used for the MAR-GRISLI experiments. However, this issue has been examined in the GMDD paper in which the 30,000-yr temperature equilibrium run appears as a sensitivity experiment. In the present paper, the temperature evolves freely at any stage of the initialisation procedure. Initial conditions inferred from the relaxation run are just restored before starting a new iteration.

What is your stopping criterium at this point and the reason for not iterating further?
Our target is to obtain the mininum ice thickness root mean square error (here RMSE = + 63 m). We stopped the iterations when the RMSE is not significantly improved (here after 8 cycles). This ensures a good compromise between the reduction of the mismatch between observed and simulated ice thickness and the rapidity of the convergence of the spin-up method. This has been clearly explained in the revised manuscript.

P7.L27 It is not clear why evaluation of the initial state should be based on an experiment which includes further relaxation steps. The control experiment that offers itself naturally and should be used for that purpose is just running the model after step 3 forward with constant forcing. This would give a good indication of the match with observations (at t=0 or t=25) and the remaining model drift (after 150 years), since this is the model state actually used as initial state for the forward experiments. It anyhow seems strange to impose the observed geometry, when the model has been relaxed to a different geometry in step 3.
After the last step 2 (i.e. after the end of the 8th cycle), a 2000-yr free evolving GRISLI run is carried out under conditions identical to those used in step 2 in terms of climate forcing, initial vertical temperatures and velocity profiles. As such, the value of the basal drag coefficient is that obtained at the end of the 1st step of the 8th cycle. This has been specified in the revised manuscript.

P7.L32 It is a bit unusual to specify errors in ice thickness as median values, given that errors locally could be positive or negative. Why not specify the absolute error or root mean squared error augmented with the quantiles given already. A map of the mismatch with observations should be given (possibly in the appendix), but then for the model state after step 3, which is assumed as the initial state for the projections.
We agree with you that median computed from ice thickness errors with respect to observations is not always informative because of both positive and negative values. For this reason, the description of ice thickness changes has now been given as a function of different

surface elevations (see section 4.1.2) in order to aggregate regions that present similar tendencies.
As explained just above, the 2000-yr GRISLI simulation has been performed to reduce the ice volume drift. The state obtained at the end of this run is used as initial state of the forward experiments. In the revised version we mention both the new ice thickness RMSE (= + 132 m), which is different from that obtained at the end of the last step 2 (+ 63 m), the 5th and the 95th quantiles, and also the sea-level equivalent model drift (~10^{-5} mm yr^{-1}). We added in the Supplementary Materials a figure (Fig. S1) showing the differences between the observed and the GRISLI topographies, with the GRISLI topography taken at the end of the 2000-yr relaxation run.

P8.L1 The model state that has been compared to other models in the initMIP exercise appears to be different from the state used in the forward experiments, because it includes re-imposing the observed geometry and additional relaxation for 2000 years. This should be made very clear, especially in light of the claim that the model is one of the best in the model comparison.
This statement in particular requires further qualification and needs to specify what criteria to consider, since the Goelzer et al paper does not provide any explicit ranking of the models and goes into length about how different criteria for evaluating models are not independent. Please use such community efforts to improve your model, but don't misuse them to gain credibility for your model.
To avoid confusion and misleading interpretations we removed the comparison to other models in the initMIP exercise.

P8.L5 SLR contribution as the most abstract change could be named last.
Corrected in the text following the recommendation. The new sentence reads as: *"The aim of this study is to assess to what extent accounting for the atmosphere-GrIS interactions influences the GrIS evolution in terms of changes in SMB, ST, ice thickness and SLR".*

P8.L16 Is the elevation difference used for the correction calculated between Bamber (at 5 km) and Bamber (at 25 km) bi-linearly interpolated to 5 km? Please describe.
The horizontal interpolation is made using an inverse distance weighting method, as it is now specified in the revised manuscript. Moreover, to account for the differences in surface elevations between the 25 and 5 km Bamber et al. (2013) topographies, we also apply a vertical correction following Franco et al. (2012) who derived a local vertical gradient of each SMB component as a function of altitude.

P8.L21 This seems to imply that at least until 2020, NC is an appropriate approximation to the full problem. This should enter the discussion and the abstract, following an earlier comment. Is there any reason why the modification starts at 2020 and not at 2000? It would seem like a cleaner comparison to start the interaction from the moment it is possible (i.e. 2000).
For all the experiments, the "coupling" starts in 2020 when the SMB simulated by MAR at a given time is enough different from the SMB simulated at the beginning of the simulation to induce significant changes in the GrIS topography. Thus, in the PF and 2W experiments, GRISLI is forced by MAR outputs from 2005 to 2020, following the same procedure as in the NF

experiment. It would have been possible to start the coupling in 2000 or 2005, but the results would have been similar to those presented here as the SMB changes through 2005-2020 do not produce any significant topography changes in GRISLI. This has been specified in the revised abstract and in the main text.

P8.L23 Another "coupling method" that is already discussed in the text and could be formally listed here as well is the one where MAR SMB anomalies alone are used to generate a changing ice sheet geometry (in the absence of an ice sheet model). This experiment can be performed with or without taking into account the surface elevation - SMB feedback and with or without fixed ice sheet extent.

As previously specified in our response to the General Comment", the GrIS sea-level rise estimated from SMB integrations over fixed and time variable ice sheet masks have been discussed in Section 4.4. We decided not to present them as additional experiments since the SLR estimates are inferred from diagnostics stemming from NF and 2W experiments.

P9.L13 This section reveals that the models are not actually fully coupled and also gives indications why a full coupling is so much more difficult to achieve. See general comment.

We agree with this comment. In the revised manuscript, we removed expressions such as "fully coupled" (see also our response to the general comment). We also explained why we have chosen the anomaly method (see section 3.2):

"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."

In the discussion section (i.e. section 5), we also discussed the limits of this experiment with respect to a real fully coupling method between RCM and ISM:

"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. As an example, a reasonable compromise to avoid the use of anomaly method would be to use the topography obtained at the end of the spin-up iterative process (rather than S_{ctrl}) as initial GRISLI topography to keep the mismatch with the observed topography as low as possible, and to initialise and perform MAR simulations with this spin-up topography"

P10.L2 The mean decrease in SMB explains the shift in the ELA not the other way around. The ELA is an abstract concept, the SMB change is 'real'.

The sentence has been modified as:

"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."

P10.L5 I am not sure reporting the changes in ice thickness changes as mean and standard deviation makes much sense, given the bipolar nature of thickening in the centre and thinning at the margins. More useful would be for me to describe the changes for specific regions.

In order to clarify the text, the description of ice thickness changes has been given as a function of different surface elevation (see section 4.1.2). Besides the mean ice thickness anomaly values and the corresponding standard deviations, we have also reported the 5^{th} and the 95^{th} percentiles to indicate the range of ice thickness changes:

"The ice thickness anomaly (Fig. 4) also presents two distinct patterns. For surface elevations higher than 2000 m in the northern part, and higher than 2500 m in the central and southern parts of the ice sheet, the ice thickness increases by +5 m on average, with the increase ranging from +1.5 m (5^{th} percentile) to +17 m (95^{th} percentile). On the other hand, in regions whose surface elevation is lower than 2000 m, the ice thickness decreases from -248 m (5^{th} percentile) to -3 m (95^{th} percentile) with a mean value equal to -100 m".

P10.L12 What exactly is the impact of ice temperature on ice dynamics? Are you implying that changes of the surface boundary conditions modify the temperature structure of the ice and its deformation?

This was a shortcut. Actually, as the model was forced by a warming scenario (i.e. the RCP8.5 and extension of year 2095 to 2150), we simplified by "warming scenario" instead of simply explaining that the ice dynamics was also impacted (in addition to ice thickness). In the revised manuscript, changes in ice velocities are related to changes in ice thickness. As a result, the new sentence has been changed in:

"The ice dynamics is also impacted by changes ice sheet geometry as illustrated by the mean surface velocity anomaly (Fig. 6a)".

P10.L13 Are you talking about velocity or velocity anomalies here? Figure 4A shows anomalies! Please clarify.

We are talking about surface velocity anomaly. We have clarified the text (see section 4.1.3).

P10.L15 This statement calls for a figure comparing modelled and observed velocities! Add a panel to substantiate this point.

The panel showing the observed surface velocities has been added (see Fig. 7 in the revised manuscript).

P10.L18 Add "in this area" after "ice velocities" and remove it in the sentence after.

The comments related to the new figure 7 have been reorganized and the sentence you refer to has been removed from the revised manuscript. The examples of the Jakobshavn and the Kangerlussuaq glaciers are now distinguished, and the new paragraph (section 4.1.3) reads as:

"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-g). 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)".

P10.L30 "amplification of all the changes" is a bit too general here. Better "amplification of the changes".
This has been reformulated. Following the recommendations of Reviewer 2, the Results section (Section 4) to emphasize the 2W experiments. As a consequence Section 4 has been reorganised and the changes occurring in 2100 are now discussed in Section 4.4

P10.L31 A figure showing the absolute sea-level changes for the different experiments would be in place, possible as additional panel in figure 7.
Following your suggestion, we added a figure showing the absolute sea-level changes (Fig. 12a in the revised manuscript). We also made a zoom-figure displaying the sea-level anomalies between 2000 and 2100 to better illustrate the divergence of the three experiments as soon as 2025-2030 (Fig. 12b).

P11.L3 ST is already defined
Thanks for this remark. ST is now defined once, in Section 1.

P11.L4 Replace "is strongly colder" by "sees a strong cooling" or similar.

This has been corrected

*P11.L10 "Thus, the *stronger* ST decrease in 2-W compared to NC ...", assuming there is decrease in both cases. To check also in other places that you discuss differences in changes, not changes itself.*

We made our best to remove all ambiguities related to changes and differences in changes. We hope the text is now clearer.

P11.L10 Not sure where "the middle of the slope is". Clarify!

We replaced "middle of the slope by "along the slope"

P11.L14 Costal regions don't exist inland from the ice edge.

We reformulated in "in the interior of the GrIS".

P11.L28 Replace "SMB anomalies increases by a factor of 10" by "SMB anomalies decreases by 10 cm yr-1"

This sentence was referred to Table 1 which does no longer appears in the revised manuscript. The new Table 1 provides values of the GrIS contribution to sea-level rise in 2050, 2100 and 2150 for the three experiments. Moreover, the section describing the SMB differences between the 2W and the NF experiments has been re-written (see Section 4.2.1)

P12.L1 Again mixing discussion of surface elevation and ice thickness here. Revise.

This has been revised and corrected in the entire revised manuscript

P12.L1 Add "difference" after "surface elevation change" and reformulate to "follow the patterns of SMB anomaly differences (Fig. 6B)".

Replaced by: *"The ice thickness anomaly pattern is essentially mimicking the SMB differences between 2W and NF (Fig. 8a)"*

P12.L5 Do you mean lower surface temperature in 2-W is the cause for higher SMB and therefore increasing ice thickness, or is the lower surface temperature directly impacting ice thickness (i.e. not through its effect on SMB)? In the first case, lower surface temperature and its effect on SMB should be mentioned first and higher SMB as a consequence. More precision needed here.

This part of the text has been clarified and precisions have been added:

"The main SMB differences between both experiments, averaged over the 2140-2150 period, highlight lower SMB values in 2W compared to NF for altitudes below 2000 m, with the exception of some margin locations in the eastern part (Fig. 8a). This SMB anomaly behaviour is driven by a snowfall reduction in low altitude areas (Fig. S6) and by the runoff increase in 2W with respect to NF (Fig. S7). This increased runoff results from warmer temperatures over the whole GrIS (up to 0.8°C in the western and northern parts, Fig. 8b), except in the region at the edge of the GrIS, which sees a strong cooling (as low as -10°C, Fig. 8b). The warming can be explained by the temperature-altitude feedback being active in 2W, resulting in lower altitudes

(section 4.1.2 and Fig. 8c) and therefore warmer temperatures. The cooling over the very edge of the ice sheet occurs despite the ice sheet thinning over these regions. It can be explained by changes in atmospheric circulation".

P12.L6 "in areas of lower ST"? In my eyes, ST and ST differences (Fig. 6A) are both high and positive in regions of negative thickness anomaly. Clarify that statement.

The ST differences between the 2W and the NF experiments are positive in most of GrIS areas. However, at the very edge of the ice sheet, these differences are negative, showing that the 2W surface temperatures are lower than the NF ones as a result of the effect of katabatic winds. In the revised manuscript we changed the ST color scale, making the negative ST differences more visible.

P12.L11 I thought you are trying to describe here the impact on the ice thickness evolution of two-way coupling as opposed to no coupling. In this part, you however come to the impact on the atmospheric circulation (katabatic winds) and land model changes (albedo). From line 15 on, you go back again to ice dynamic changes. Could this material be better organised to avoid jumping between the different aspects?

Following your suggestion, this part has been reorganized. In this section, we only emphasize the effect of katabatic winds. The reduction of the ice-sheet extent simulated in the 2W experiment (and thus the effect on albedo changes) is now discussed in section 4.4.

Also, if I understand correctly, the anomalous katabatic winds created by 2-W have visible impact mainly on the narrow marginal areas of the ice sheet where anomalous cooling increases SMB. This should then be counteracted by the albedo changes described L12 and following. It is not really resolved for me how these different factors influence each other and which is the dominant mechanism in which region.

Taking into account the effect of katabatic winds leads to a cooling in 2W with respect to NF at the very edge of the ice sheet. Since the 2W temperature is lower than the NF one the predominant effect is that induced by the katabatic winds, not by albedo changes.

P12.L13 What is the difference between snow-free and snow free?

Sorry, this was a typo error

P12.L22 Melting itself does not necessarily contribute to SLR since melt water can be refrozen in the snow pack. Better replace "melting contribution" by "ice sheet contribution" or similar, also in the rest of the manuscript.

We followed your suggestion and the occurrences of "melting contribution" have been replaced by "GrIS contribution"

P12.L22 These numbers should be calculated against a control experiment to remove the contribution from remaining model drift. Has this been done?

Yes ; it has. Our control experiment is the 2000-yr GRISLI relaxation run. As specified in the revised manuscript, the remaining model drift in terms of ice volume is only 10^{-5} mm yr^{-1}, fully negligible with respect the GrIS contribution to sea-level rise.

P12.L23 I would suggest to add a panel to figure 7 with the total contributions for the three experiments and include the integrated SMB mentioned further below in this section.
This has been done. Figure 7 has become Figure 12.

P12.L25 Since you discuss 2-W against NC, the surface elevation - SMB feedback which operates all over the ice sheet should also be mentioned, not just the processes at the margin.
We paid attention to describe through the entire revised manuscript the processes operating in the interior of the ice sheet. In particular, the results are most often presented as a function of surface elevation: we distinguished regions of low to medium altitudes from regions of high altitudes (See in particular Section 4.1 for the description of the results inferred from the 2W method and Section 4.2 for the effects of the katabatic winds which strongly differ from central regions to margin areas.

P12.L26 The difference of 52400 km2 is at the end of the experiment and then it increases with time? Reformulate
As mentionned above, absolute changes in ice-sheet extent are no longer discussed in the revised manuscript. Now, this aspect is only addressed in terms of relative changes between NF vs 2W and PF vs. 2W. This has been therefore reformulated in: *"Compared to the NF and the PF experiments for which the ice-sheet mask is fixed to observations from 2000 to AD 2150, the 2W ice sheet extent is reduced by ~2.8 % in 2150 as a result of increased ablation".*

P12.L27 I think all you are saying is that the high resolution ISM mask changes are translated to partial mask changes for MAR. Clarify that the ice sheet mask (Fig S2B) is the one seen by MAR.
This part has been re-written:
"Compared to the NF and the PF experiments for which the ice-sheet mask is fixed to observations from 2000 to AD 2150, the 2W ice sheet extent is reduced by ~ 2.8 % in 2150 as a result of increased ablation. As MAR sees the ice sheet retreating over time in 2W concomitantly with the increase in bare ground or tundra fractions (Fig. S5b), the albedo feedback takes place favouring further the ice melting. Although the ice sheet retreats, the extent of the ablation zone increases with time. 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). As a consequence, the ELA is located further inland in 2W compared to NF with a maximum inland retreat of 120 km located in northeastern Greenland (Fig. 3)."

P12.L31 I think the point to make here is not about increase in uncertainty. You can show that when a fixed mask is used, you simply get the wrong result and overestimate the mass loss. Could you quantify the relative importance of this effect compared to the error that is made when not taking into account the surface elevation - SMB feedback?
In Section 4.4, we quantified 1/the error made when the SMB-elevation feedbacks are ignored (i.e. 7.6%, deduced from the comparison between the SLR contributions in NF and PF

experiments) 2/ the error made when all the feedbacks are ignored (i.e. 9.3 %, deduced from the comparison between the SLR contributions from NF and 2W) and 3/ the error made when using a fixed ice-sheet mask (i.e. 6 %). To follow the suggestion of the reviewer, we added the following sentence at the end of the section:

"[…] 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 %)".

P13.L3 Please specify the resulting SLR.

The resulting sea-level rises obtained with the integrated-SMB methods have been explicitly mentioned in the text of the revised manuscript (Section 4.4) and reported in the new Table 2.

P13.L14 This is exactly the reason why median results are not very meaningful in this context. Mean absolute or root mean squared differences are easier to interpret.

In the revised version, we no longer mention the median values. We provide instead the ice thickness root mean square errors as well as the 5th and the 95th percentiles in ice thickness differences for regions showing similar patterns (margins vs. interior).

P13.L15 After showing figure 8, figure 9 does not add substantial information in my view. I would remove it and continue discussion about differences between 2W and 1W based on figure 8. The only reason to show figure 9 would be if you wanted to attempt modifying the parameterisation used in 1W to incorporate the katabatic wind effect, which could be a logical next step.

We acknowledge that part of the information provided in Figure 9 (Figure 11 in the revised manuscript) can be found in Figure 8 (Figure 10 in the revised manuscript). However, we believe that the new Figure 11 better illustrates the differences between the three experiments in the simulated spatial variability as a function of the altitude. This is why we finally kept this figure in the revised paper.

P13.L27 These sentences are just stating the obvious. I'd suggest to remove them.

We agree with this statement: the sentences have been removed.

P14.L5 It is not clear to me why a higher resolution should lead to increase the SLR and not the opposite. Unless there are convincing arguments to support that claim, I would leave the sign of the change open.

The reviewer is totally right. We recognize this was an overstatement. In the revised manuscript, the Discussion section has been extended and we better explained the possible influence of outlet glaciers on projected SLR. We also mention the possible decreasing influence of the outlet glacier dynamics with time:

"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., 2014a, 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".

The same applies to the limitation of constant basal drag in the next sentence. With all the complexities surrounding the evolution of the basal conditions over time, I don't think there is any evidence that acceleration of ice flow has to be the dominant response. Again, putting forward some convincing arguments would be appreciated.

Again, we fully agree with the reviewer and this sentence has been removed from the revised manuscript. In particular, we discussed the limitations related to the time constant basal drag coefficient and to the lack of any infiltration scheme in our ice-sheet model:

"Our spin-up method adjusts the basal drag coefficient in such a way that the departure between the observed and the initial GRISLI topographies is reduced. The resulting ⍰ coefficient is spatially varying but is constant in time. This assumption may likely be valid for short-term forward simulations but is probably overly simplistic. On the one hand, the basal drag tends to be smaller towards the margins with respect to the interior. As the ice sheet retreats inland, it can be expected a reduction in basal drag for a specific location, due for example to a decreasing effective pressure. On the other hand, changing basal hydrological conditions can also alter the basal drag. This can occur as a result of rainfall or surface meltwater infiltration that can refreeze at depth or propagate all the way to the bottom of the ice sheet and increase basal lubrication (Kulessa et al., 2017). Therefore, a time constant basal drag coefficient inferred under present-day conditions may underestimate the ice flow acceleration. A few models describing the vertical inflow exist (e.g. Banwell et al., 2016, Clason et al., 2015; Koziol et al., 2017) but are generally run at the regional scale and at very high spatial resolution (a few tens to a few hundreds of meters at most). Implementing such models in large-scale ice-sheet models is currently outside the realm of possibilities. However, as there is a growing interest in performing ice-sheet projections over multi-centennial time scale, the GRISLI-like models would undoubtedly benefit from the implementation of simplified infiltration schemes (e.g. Goelzer et al., 2013) so as to account for the impact of ongoing changes in surface meltwater on ice dynamics".

P14.L19 Additional limitations that should be discussed: - Ignoring the glacial-interglacial signature of past climate changes in this steady state spin-up of temperature typically makes the ice too warm. This needs to be compensated by other factors (likely a different set of basal drag parameters). - The steady state initialisation also ignores any influence of transients in the *observed ice sheet evolution – Mismatch of the modelled ice sheet geometry and velocity structure with observations leads to uncertainties in the projected evolution.*

We added the following paragraph in the Discussion section (Section 5):

"An additional limitation related to the choice of our spin-up procedure is that the glacial-interglacial signature of past climatic changes is ignored. Neglecting the climate history of the Greenland ice sheet implies too warm ice temperatures. This may have an impact on the future GrIS evolution and on its contribution to sea-level rise. Indeed, the basal drag coefficient inferred from the inverse method may be too high so as to compensate the errors induced by the artificial warm bias. However, using a higher-order ice flow model, Seroussi et al. (2013) showed that at the centennial time scale the basal conditions and the GrIS projections are only poorly sensitive to the initial vertical temperature profile but are critically dependent on atmospheric conditions".

P14.L28 Add "in this comparison" after "atmosphere-GrIS feedbacks". I hope you don't think this statement is universally true.

Due to the huge changes made to the original text, this issue has been presented differently and mentioned in Section 3.3 devoted to the description of the 2W experiment: *"Compared to the NF and PF approaches, this two-way coupled method is the most accurate to represent the GrIS-atmosphere feedbacks"*.

P14.L30 While this statement seems true for the given results, the conclusion hinges on the change in behaviour of 2W at 2110. Unless investigated in more detail, it cannot be excluded that such change could happen at an earlier point in time, e.g. for a different model used as boundary condition to MAR.

In the new version, we showed that the results from the three experiments start to diverge from each other as soon as 2025-2030, that is a few years only after the start of the coupling. This means that the feedbacks that are accounted for in the PF (SMB-elevation feedbacks) or in the 2W experiment start to operate as early as this period. However, we also explain that the influence of the feedbacks increases over time and that they become dominant at the end of the 21st century (See in particular Section 4.4). Moreover, in the Discussion section, we clearly explain the possible dependence of our results with the GCM forcing used to force MAR:

"Whatever the experimental design, the large spread in SLR projections raises the question as to whether the ice-sheet response simulated in our 2W experiment relative to that of the NF and PF experiments would be similar, amplified or mitigated with a different GCM climate forcing having a different sensitivity from MIROC5. […].There is therefore a strong need for iterating the present study with different global climate simulations run under an extended RCP8.5 scenario and used as a MAR forcing, to assess more accurately the impact of the different GrIS-atmosphere feedbacks and to better evaluate the uncertainty associated with the projected sea-level rise contribution from GrIS".

P15.L5 This comparison is a bit awkward. Wouldn't it be more appropriate to compare the +0.5 cm to the total projected SLR as a relative error?

We acknowledge that this comparison was not fully appropriate and we removed it from the revised manuscript.

P15.L14 Remove repeated "respectively" after SLR.
The sentence has been changed.

P15.L19 It would be good to additionally put this number (21%) in perspective to the underestimation due to ignoring feedbacks, i.e. the difference between 2W and NC.
This comparison has been done (see our response to comment P12.L31).

P15.L24 Again, I don't see any evidence for the interpretation that higher resolution and higher order physics increase the response.
We agree with you. This comparison was not appropriate (See our response to comment P14.L15).

P15.L29 Replace "disrupt" by "modify"
OK, all occurrences of "disrupt" have been removed.

Table 1 Does "after 50 yrs" mean at year 2050? Maybe that would be a better indication. Or do you not want to assign an absolute date to your simulation? The historic and future RCP forcing is clearly linked to an absolute date, though. Since the ablation area changes so much, it may be interesting to calculate additional diagnostics for a constant region, e.g. for the observed present day ablation zone, or backwards for the area of the ablation zone area after 150 years. This way, the convolution with a changing area could be avoided.
In the revised version, most results have been discussed as a function of altitude. We mainly distinguished two type of areas: areas of high altitude (generally higher than 2000 or 2500 m) and areas of low to medium altitude (< 1000 or 1500 m). As a result it does no longer make sense to present SMB and ST values (Table 1) or GrIS thickness or ice velocities (Table 2) at the scale of the whole ice sheet, as it was done in the first version of the paper. Moreover, the results computed at the ice-sheet scale are not really informative because of the large spatial variability in the 2W-NF anomaly. In addition, the changes in ablation area and in ice-sheet extent have been discussed in the revised paper in terms of relative changes (see Section 4.4). To our opinion, they don't need to appear in a table. We therefore removed both the former Tables 1 and 2 from this new version of the manuscript. However, we replaced these tables by new ones providing the GrIS contribution to sea-level rise inferred from NF, PW and 2W experiments (new Table 1) and from the SMB-integrated method (Table 2). We also replaced "after 50 years" by the absolute dates.

Table 2 Not sure how to interpret a velocity change of e.g. -3.0+-25.0. The noise being much larger than the signal, is the valid interpretation 'no significant' change?
We fully agree. See our previous response to comment related to Table 1.

Figures ——-

The labels in the figures are upper case (A,B,C), but the panel references in the captions are all in lower case (a,b,c). Make consistent.

OK, the labels in the figures and in the captions are now identical

Figure 2 Why are figures B and C so different? At least in the interior, one would expect a pattern very similar to the SMB anomalies in this experiment. My guess is that this is indicative of a remaining model drift. Results of a control experiment starting after step 3 with constant forcing should be shown here or in the appendix and the origin of this difference should be discussed.

To address the comments raised by Reviewer 2, the organization of the paper has been modified. 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 NF results are only presented in terms of differences with the 2W method. Moreover, we think that the differences you mentioned between both plots can be attributed to the choice of the color scale. The new figures (Figs 2a and 4) present similar patterns for SMB and ice thickness anomalies simulated in the 2W experiment. However, the discussion requested to explain the differences between the SMB and the ice thickness patterns has been provided for the 2W method (see Figs 2a, 4 and 5 in the revised manuscript and section 4.1.3). These differences are explained by the ice dynamics. In Section 4.1.3, we added the following paragraph to support this argument:

"The ice thickness anomaly is due to the complex combination of changes in surface atmospheric conditions (SMB, Fig. 5a), ice dynamics (ice flux divergence, Fig. 5b) and basal melting (not shown), following the continuity equation (Eq. 2). 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".

Figure 3 The displayed field is ice thickness, not surface elevation as written in the caption. Since the discussion is about ELA and surface elevation - SMB feedback, it may be useful to show surface elevation instead.

Figure 3 has been modified. Now it displays the surface elevation

Figure 4 The colour scale in A is not easy to read with small positive and small negative values sharing the exact same colour (green). This should be improved. Have you tried to plot velocity ratios instead of anomalies? Since velocity magnitudes cover several orders of magnitude, a large relative change is not visible because of the cutoff at 2 myr-1, while small relative changes at the margin appear exaggerated.

The surface velocity anomalies are now plotted in the new figure 6 1/ for the 2W experiment (instead of NF) between the end (2140-2150) and the beginning (2000-2010) of the simulation and 2/ for the anomalies between 2W and NF and between 2W and PF at the end of the simulation. The colour scale (in log10) has been also modified to better illustrate positive and negative changes.

Figure 5 Why not use the same colour mapping here and in figure 4 for the velocity anomalies? That would make it easier to compare the two figures.
Figure 5 (in the first manuscript) is now Figure 7 and uses the same colour scale as Fig. 4.

Caption: "left panel" The figure caption has been modified

Figure 6 There appears to be a slight instability in one or both of the experiments compared in figure 6A. Also Figure 8A shows signs of instability in form of a checker board pattern. While these instabilities are likely not critical for the interpretation of the large scale results presented here, they should at least be mentioned.
Figures 6A and 8A are now Figures 8b and 10b. The features the reviewer refers to are related to the method used to correct for the altitude difference between the MAR and the GRISLI topographies.

Figure 7 Add a panel with absolute contributions of the three experiments. Note that results shown so far are double differences, i.e. differences in anomalous contributions since year 2000 between different experiments. Could also show sea-level contribution differences calculated from difference to a control experiment with constant forcing to remove the model drift. Same consideration holds for the absolute contributions.
Figure 7 appears now as Figure 12. We added a panel showing the absolute contributions (Fig. 12a). In all the simulations presented here, the model drift has been taken into account but is fully negligible (see our response to comment P7.L32).

There seems to be a step change around 2060 and again around 2110, where the behaviour of 2w-1w (yellow) changes dramatically. By comparison with 2w-nc it appears to be caused by the evolution of 2W. What is happening at these moments in 2w? Please investigate this further.
The figure showing the anomalies of sea-level contributions has been replaced by a zoom-figure. Thanks to this new panel, we show that the three experiments start to diverge from each other as soon as 2025-2030 and not only around 2060. We do not observe any significant change in slope in 2060 nor in 2110.

Caption: "Differences in Greenland ice sheet sea-level contribution between the different experiments." Then explain how it is calculated.
We provided further details in the figure 12 caption.

Figure 9 is not needed in my estimation.
See our response to comment P13.L15. Note also that Figure 9 now appears as Figure 11.

References:

Format of many references in the text are non-standard. A few examples are given

here, but all should be re-checked.
The format of the references has been re-checked.

P3.L13 add e.g. before Gagliardini
OK, added

P3.L19 reformat list of reference and avoid double brackets
P4.L10 "(e.g. Fettweis et al., 2013
OK, reformatted

P6.L12 Author is called Fox Maule. Check reference.
OK, modified

P6.L20 Reference Le clec'h et al. (in prep) is not in the reference list.
This reference now appears as Le clec'h et al. (2018).

P8.L2 add Goelzer et al., 2017 to the reference list.
OK, added in the reference list

References:
Fox Maule, C., Purucker, M. E., Olsen, N., and Mosegaard, K.: Heat flux anomalies in Antarctica revealed by satellite magnetic data, Science, 309, 464-467, doi: 10.1126/science.1106888, 2005.
The reference has been corrected