Response to Reviewers' comments on Manuscript 'Simulation of the Greenland Ice sheet over two glacial cycles: Investigating a sub-ice shelf melt parameterisation and relative sea level forcing in an ice sheet-ice shelf model.

We thank the two reviewers for their constructive and helpful comments regarding the manuscript. As both reviewers highlighted that the manuscript was too lengthy and contained too much information which was not necessary for the reader. Following these comments, we have drastically revised and reduced the manuscript, removing all the supplementary material, Methods 1-3 and the discussion of the ESL forcing and sheet-only simulations. We hope this results in a clearer and easier to read manuscript.

The comments from each reviewer are shown in italics, with our responses given in bold and any revised text highlighted in red. Comments relating to style and formatting are listed at the end of each section and have all been corrected.

Reviewer 1:

General Comments:

1) On the climate forcing: I understand that the focus of this study is on testing the sub-ice shelf melt parameterizations and the effect of sea level forcing on ice sheet evolution. However, the climate forcing the ice sheet model simulation over the last two glacial cycles is important, and will likely largely affect the simulated spatial and temporal extent.
The SAT forcing is taken from Helsen et al. (2013), but this forcing is not discussed. Is this forcing really representative for the entire model domain?

We agree with the reviewer's comment that using a SAT forcing record, which is developed using ice core records from Antarctica and Greenland, is of course not representative of the spatial variability in climate across the Greenland Ice sheet during these two glacial-interglacial cycles. However, this is a limitation of all standalone ice sheet models. As an intermediate step, we have previously investigated using a schematic GCM forcing coupled to a regional climate model coupled to an ice sheet model (Helsen et al., 2013), but this is not feasible for ensemble runs of two glacial-interglacial cycles. However, to clarify this limitation within the paper and provide further background information on the SAT forcing development, we have added the following information into Sect. 3.1, lines 201-207.

Lines 201-207: Secondly, each simulation was ran for 240 kyr using a spatially uniform SAT forcing taken from Helsen et al., 2013 (Fig. 2a) combined with a SSM parametrisation (Sect.3.2) and sea level forcing (derived from a GIA model, Sect.3.3), to simulate the GrIS over the two glacial-interglacial cycles. As there is no GrIS SAT record that extends beyond 128 kyr BP, this SAT forcing record was produced by combining the Vostok ice core (Petit et al., 1999) with the GRIP ice core record (Johnsen et al., 2001) using the glacial-index method (Greve, 2005). We note that using a SAT forcing record derived from ice cores will not account for any spatial variability in the SAT during these two glacial-interglacial cycle.

How is the SMB calculated from the SAT forcing? The timing and extent of the simulated Greenland ice sheet will depend on the SMB evolution.

We agree that the discussion of the SMB forcing was lacking enough detail in the previous version of the manuscript. We have added the following information to Sect.3.1 to clarify this.

Lines 208-214: The SMB-gradient method (Helsen et al., 2012) was applied at each time step to calculate a new SMB field resulting from this SAT forcing. In this approach, first this uniform temperature forcing (Fig. 2a) is converted into a spatially variable climate-driven surface elevation change using an atmosphere lapse rate of -7.4 K km^{-1}. Second, the SMB gradient fields are calculated based on a linear regression between this new surface elevation field and the mean SMB in an area with a radius of 150km. With this approach, the spatially uniform temperature forcing (Fig. 2a) can be translated in the spatially varying SMB field and ensures that the local mass balance height feedback is captured.

(2) Related to this: Page 12, lines 425-457 discusses spatial variability of the simulations and links this to the SAT. However, my understanding of the SAT forcing is that it only varies over time, not spatially, which would mean that this discussion over-interprets the results.

The reviewer is correct that the adopted SAT forcing does not consider any spatial variability, and following the earlier suggestions we have added a sentence to clarify this (lines 201-207). However, with the SMB-gradient method (as we mention in response to comment 1) a spatially uniform temperature perturbation can be converted into a spatially variable SMB forcing as it captures the changes in surface elevation resulting from the spatial variation from the ice sheet model. This implies that the important mass balance height feedback is captured. Therefore, we feel that we do not over-interpret the results.

(3) PD ice sheet: Yes, indeed a common feature of SIA models is the overestimation of ice on the margin of the ice sheet (p. 6, lines 199-200). However, studies using these models focus mostly only on grounded ice, while this study investigates the ice shelves. How will the overestimation of marginal ice effect your ice shelves (thickness, dynamics, ...)?

We have removed the sentence referring to this feature of SIA models.

(4) Why focus on 2 glacial cycles? Many of the inputs/forcings are only available for the last glacial cycle, as are the data observations to compare the model results to.
What is the added value of including the earlier glacial cycle, apart from model spin-up?

We do not believe that the only interest in running two glacial cycles is for model spin-up purpose. The previous glacial cycle (225-118 kyr BP) is a glacial-interglacial period which is interesting in its own right. The main reasons for simulating the two glacial cycles within this study are (1) to examine the contribution of the GrIS to the last interglacial highstand, a question which is yet to be resolved, (2) the influence of the PGM - LIG glacial history on the LGM-PD glacial history, (3) Identify any variations in these two glacial histories.
For example, in Section 5, we have highlighted the influence of the PGM-LIG on the LGM-PD glacial history:

Lines 394-407: The SSM at deeper water depths (> W_D1), controlled by SSM2, also strongly influences the behaviour of the NW margin via the impact on the PGM to LIG glacial history. Fig. 7c-d compares the difference in the simulated water depth between two simulations

($A_vA_s+A_vSSM1$ and $A_vA_s+A_vSSM1\_redSSM2$) where the SSM2 is reduced by 25 m/yr (from 100 m/yr to 75 m/yr). It could be assumed given the reduction in SSM at deeper water depth, that the retreat would be later. However, the onset of retreat is 2 kyr earlier (8.9 kyr BP c.f 6.9 kyr BP). This is due to the influence of the PGM to LIG glacial history (first glacial-interglacial cycle) on the dynamics of the LGM to PD retreat. In the $A_vA_s+A_vSSM1\_redSSM2$ simulation, during the first advance of the ice sheet, the lower SSM at water depths > 400m results in a thicker ice sheet across the Nares Strait and eastern Ellesmere Island. This increases the bedrock subsidence and the water depth (Fig.7c) resulting in a higher SSM surrounding the retreated ice margin during the subsequent glacial-interglacial cycle (after the LIG minimum). This higher SSM restricts the maximum spatial extent that the grounded ice margin reaches during the subsequent LGM -PD cycle (compare Fig.7d to Fig 7a and 7b). Therefore, with a smaller ice extent, surrounded by a region of higher SSM, this induces an earlier onset of retreat.

(5) *The set-up of the sea level/water depth forcing is not clear to me. In the ESL method global mean sea level change is used as forcing, but local changes in the solid earth field are also included. Correct? Especially the comparison of the total water depth from the different methods (page 8, lines 290-298) is very confusing. Please clarify this section.Note that it is also confusing to call the first method "ESL", as the text also uses "ESL" as unit for global mean sea level change.*

We have removed the discussion of the ESL forcing only method throughout the manuscript, following the suggestions from both reviewers. We feel that this now makes Sect. 3.3 and references to the method clearer to follow throughout the manuscript. Additionally, we have revised the text in Sect.3.3 to simplify and clarify the explanation of the method and equations.

In the RSL method, local and non-local geoid and solid earth changes are included, but they seem to not be consistent, and are calculated from different (not necessarily compatible) models.

The reviewer is correct that the local isostatic response is calculated within IMAU-ICE using a more simplistic model that used within the GIA model. Within the ice model IMAU-ICE, the isostatic response is based on a simple 1D elastic lithosphere overlying a relaxed asthenosphere with a decay time of 3kyr (ELRA). The ELRA method has been shown in Le *Meur and Huybrechts, Annals of Glaciology,23, 1996, Greve and Blatter, 2005* to produce, to a first-order a similar deformation field as produced from a 'self-gravitating visco-elastic' GIA model when adopting average earth model parameters, such as used in this study. Additional the approach of combining an isostatic response from an ELRA method within a GIA model is not new and has been adopted in previous studies Whitehouse, et al., 2012, QSR and Lecavalier et al., 2014 for example, with the former identifying close agreement between the output from the two approaches. This justification is addressed in the revised manuscript.

(7) In my opinion, the use of supplements in a CP publication should not be necessary. It is difficult as reviewer to find your way through the different texts, tables and figures. This will therefore be very confusing for the reader.
(8) Related to this: the present manuscript contains too much information. What is the main message of the manuscript? And what information is needed to verify and understand that message? For example, I think that methods 1-3 of the SSM parameterization are not

essential, and could be omitted. Similarly, is it really necessary to include the ESL forcing method? Maybe better to only focus on explaining the RSL method and present those results more clearly

We agree with both reviewers that the manuscript contained too much additional information in the supplementary. This has now been removed. Additionally, the manuscript has undergone a drastic rewrite, removing the section on Methods 1-3 and ESL forcing. We hope this makes the manuscript easier to read.

(9) *Page 6, lines 196-200: This tuning of the PD ice sheet should be explained in a separate section, or included in Sect. 4.*

We do not feel, given the focus and (as already commented) extensive length of the manuscript that additional information about the tuning of the present-day ice sheet will add to the scientific arguments of the paper. The aim of the study was not to replicate an ideal present-day ice sheet. When comparing the results of the ensemble of simulations to the observational data no simulation was rejected based on its present-day extent. We have however moved the previous Fig. S9, which illustrates the misfit of the simulated present-day ice sheet into the main manuscript into the now revised Fig.4e.

(10) *Abstract, lines 14-16; and Conclusions, line 512: Make clear that only the solid Earth influence of the LIS and IIS on GrIS was explored. How changes in atmospheric circulation due to the vicinity of these large ice sheets affect the GrIS is not discussed.*

We have added additional text into the conclusion and introduction to make it more explicit within the manuscript that the impact on the LIS on the atmosphere was not considered.

Abstract.
Lines 19-22: In this paper, we investigated the evolution of the GrIS over the two most recent glacial-interglacial cycles (240 kyr BP to present day), using the ice sheet-ice shelf model, IMAU-ICE and investigated the solid earth influence of the LIS and IIS via an offline relative sea level (RSL) forcing generated by a GIA model.

Conclusion.
Lines 415-417: We note that we do not investigate the influence of these two ice sheets (LIS and IIS) on the atmospheric circulation; there was no climate model used within our study.

Technical comments:
1. Order of references: this seems random, please change to chronological or alphabetical, and be consistent.
2. References in the text need to be formatted to: … Name et al. (year) …
3. Use spaces between Table of Fig. and number (i.e. Table 1 instead of Table1).
We have reviewed the formatting of the references, Figs and Table labels within text.
4. Please check all apostrophes (e.g. forcings instead of forcing's for plural)
Corrected.

5. "sheet-only" should be "ice sheet only", similarly "shelves" should be "ice shelves"

We have removed all discussion of sheet only simulations and ice shelves

6. "on Table" should be "in Table" ***corrected.***

<u>Reviewer 2:</u>

General comments:

(1) *The description of the IMAU-ICE model lacks information. How are the ice streams treated? Since the sliding factor As plays an important role in the analysis, I suggest to describe the sliding law in detail. Also, how is the surface mass balance calculated*?

We agree that description of these factors within IMAU-ICE was limited. We have added the following information regarding the sliding law and SMB method to clarify this.

Lines 177-185: At regions within the ice sheet where the basal temperature reaches pressure melting point, the ice sheet is allowed to slide using a Weertman-type sliding law, which relates the sliding velocity (v_b), to the basal shear stress (τ_b^p) such that

$$v_b = A_s \frac{\tau_b^p}{Z^q} \qquad (1)$$

Where A_s is defined as the sliding coefficient which can be taken as inversely proportional to the bed roughness, z is the reduced normal load and p and q are spatially uniform constants over the ice sheet domain. As the roughness at the base of ice sheet is a relatively unknown quantity, a range of sliding coefficients were investigated, between 0.04×10^{-10} and 1.8×10^{-10} $m^8N^{-3}yr^{-1}$.

Lines 208-214: The SMB-gradient method (Helsen et al., 2012) was applied at each time step to calculate a new SMB field resulting from this SAT forcing. In this approach, first this uniform temperature forcing (Fig. 2a) is converted into a spatially variable climate-driven surface elevation change using an atmosphere lapse rate of -7.4 K km^{-1}. Second, the SMB gradient fields are calculated based on a linear regression between this new surface elevation field and the mean SMB in an area with a radius of 150km. With this approach, the spatially uniform temperature forcing (Fig. 2a) can be translated in the spatially varying SMB field and ensures that the local mass balance height feedback is captured.

(2*) Sea level forcing and WD forcing: The comparison between the ΔWD from Eq. (5) and the ΔWD from Eq (4) is not clear to me.*

Is the ΔWD from Eq. (4) calculated from the eustatic sea level (ESL) forcing? But if this is the case, the ΔG, although spatially uniform, accounts for global changes in the geoid, while ΔR from the ELRA model would account only for local GrIS bedrock deformations. So the comparison between ΔWD from Eq. (5) and the ΔWD from Eq (4) would miss not only the ΔGL but also ΔRNL term.

We agree that the previous discussion of the ESL forcing and RSL forcing was complicated to follow. However, the calculation of ΔWD does not involve the ESL

forcing as the reviewer suggested. We believe with the removal of all references to the ESL forcing the discussion of equation (5) and (6) is more transparent in Section 3.3.

(3*) In SSM Methods 3 and 4 the melting rate at the sea level is set 0 m/yr, differently from the previously discussed parameterisations. Is there any particular motivation to set no melting at the sea level here?*

We are not sure where the reviewer has read this, but the melting rate at 0 m is the same in all methods. However, given the removal of the discussion of Method 1-3, we feel this confusion has been resolved.

(4) *As Reviewer#1, I don't see the necessity of publishing the supplementary information for this work. The main results of SSM Methods 1-3 (Section S1) have already been commented in the main manuscript. Since that part shows a sensitivity analysis relative to parameterisations already discussed in previous studies and diverts from the main message of the work, I suggest you not to discuss that analysis in detail.*

We agree with both reviewers that the manuscript contained too much additional information in the supplementary. This has now been removed. Additionally, the manuscript has undergone a drastic rewrite, removing the section on Methods 1-3 and ESL forcing. We hope this makes the manuscript easier to read.

(5) The design of the experiment should be more linear. The analysis implies many parameters to play with (ice-sheet only, with ice shelves, ESL forcing, RSL forcing, As, and all the tunable parameters related to the SSM parameterisations). However, to me not all of them are worth to be discussed.Choose the most interesting and do the discussion following the main message and conclusions of the work. For example, the analysis done with the ESL forcing seems not to be really necessary. I suggest to delete that part.

As we have stated above, the previous version of the manuscript was lengthy and contained too much information which was not necessary to provide to the reader. We hope with the removal of the SOM and discussion of the ESL-forcing and sheet-only simulations this results in clearer, more focused manuscript.

(6) *In the Abstract you say that the sea level drop simulated at the LGM (-2,59 m) is "considerably more than most previous studies". However, this is not true if you consider the results suggested by recent works (such as Lecavalier et al., 2014, Simpson et al., 2009), in which the LGM sea-level reduction is higher than that presented here. Since these studies are considered to present a more realistic GrIS glacial extent (Vasskog et al., 2015*), I suggest to modify the sentence.*

This sentence has now been removed.

7) Pag. 5 lines 173-175: most of the cited works are based on the ice sheet-only version of the ANICE model, while only the work from de Boer et al., 2014 refers to an ice sheet-ice shelf model, such as the one you use in the study. Please, correct the sentence.

We have altered the references to reflect the reviewers comment.

(8) *Pag. 5, lines 80-81: The grounding line treatment is not very clear to me. It should be described in further depth including references to previous works.*
added a clarification statement that not grounding line migration….

We have added a sentence in Sect 3.1 to make this clearer.

Lines 171-173: The model does not accurately solve for grounding line dynamics, rather the grounding line is defined as the transition between ice sheet (grounded) and ice shelf (floating) points using the flotation criterion.

(9) Differ the acronym of the Eustatic Sea Level (ESL) from that of the Equivalent Sea Level (ESL).

As we have now removed the discussion on the Eustatic sea level (ESL) forcing, this comment has been resolved.

Technical comments:
- Pag 1, line 18 (and many times across the manuscript): “parametersiation” should be “parameterisation” ***changed.***
- Pag 1, line 32: “sub surface melt (SSM)” should be “sub-ice shelf melting” as in Pag. 3 line 76. The
first expression can be referred to melting below grounded ice. ***changed.***

- Pag 2, line 58: “Lecavalier 2015” should be “Lecavalier 2014”

- Pag 3, line 80: The citation “Colleoni et al., 2014” can't be found in the References
- Pag 3, line 106: The citation “Funder et al., 2011” can't be found in the References

- Pag 5, line 174: “Graversen et al, 2011” should be “Graversen et al., 2010”
All references have been checked.

- Pag 5, line 178: “ice sheet points” should be “ice shelf points” ***corrected.***

- Pag 6, line 209: “including sub ice shelf” should be “including sub ice shelf melting” ***corrected***

*- Pag 7 line 250: “as represent” should be “as represented****” corrected***

- Pag 9, line 320: “in thicker” should be “is thicker” ***corrected***
- Pag 9, line 334: “a lower As” should be “an increasing As”, right? ***changed to a higher As.***

*- Pag 10, line 363: “the choice sliding coefficient” should be “the chosen sliding coefficient****”***
paragraph removed.

- Pag 12, line 420: “Lecavalier 2004” should be “Lecavalier 2014”

- Pag 27, Table 1: “Dyke et al., 2004” should be “Dyke et al., 2014”

- *Suppl. Info, pag 1, line 19: "mm/yr" should be "m/yr"*