Ice dynamic projections of the Greenland ice sheet in response to atmospheric and oceanic warming

Reply to List of Comments

by J.J. Fürst, H. Goelzer and P. Huybrechts

First of all we want to thank the two reviewers for the critical and useful comments they gave on the manuscript. All comments are considered and helped to significantly improve the quality of our work. In the following the responses to the reviewers comments are denoted in italic and indented.

Anonymous Referee #2

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This manuscript assesses the response of Greenland Ice Sheet over the next 1-3 centuries to climate change as simulated by climate model output. This is a useful and pertinent exercise (but which has been carried out in various forms by other groups, and also previously by these authors). Thus, the paper makes a contribution to our understanding of the cryospheric response to climate change, though I think the authors need to contrast against previous studies to highlight why exactly this study is unique enough to justify a new publication. More importantly, I have identified a few major issues in their methodology and presentation which I have tried to highlight below, that I feel should be addressed before this manuscript can be published.

> *Indeed, other studies have focused on single aspects such as the lubrication effect (Shannon et al. 2013) or the generalisation of the dynamic response of individual outlet glaciers under future warming (Goelzer et al., 2013). The former study presents a comprehensive uncertainty analysis while the latter aims at a best projection relying on additional input for speedup from a flow-line model and SMB changes from a regional climate model. Consequently, both studies concentrated on a single climate scenario. Similarly, SMB projections with regional climate models often focus on individual climate trajectories. Here, we follow an ensemble approach for a suite of AOGCMs to assess the range of the future ice sheet response. In addition, this study comprises all 4 climate scenarios / emission pathways, suggested for the last IPCC AR5. Informed by the earlier studies, our approach accounts for basal lubrication and ice discharge increase due to ocean warming, and uses a higher-order ice-flow model incorporating longitudinal stresses. On the basis of a poor knowledge on how efficient offshore waters enter the local fjord systems on Greenland and assuming a first-order control from atmospheric and oceanic temperatures on the variety of small-scale processes controlling ice discharge, we decided for a simple parameterisation in*

our large-scale model application. The parameterisation finds justification from the fact that the regional acceleration pattern is consistent with an assessment of offshore temperature variability around Greenland (Straneo et al., 2012; Jackson et al., 2014). The manuscript is therefore a first attempt to explain and project the ice discharge response with temperature changes in the ocean.

The above discussion is recurrently addressed under different angles throughout the manuscript (and prominently in the abstract and the conclusion). The authors are therefore convinced that this study is sufficiently contrasted against previous work.

General comments

General: enhancement of mass loss due to warmer ocean temperatures seems an important result, however, this is based on a very simple (one-equation) parameterization of the influence of ocean forcing on the ice sheet. Thus, the quality of this important parameterization becomes quite important to one of the 'takeaways' of this manuscript. Yet no example of the sensitivity of this parameterization to, e.g.: the value 5.2 in Equation 3, the breakdown of ocean basins, the distance up-glacier to which the parameterization is applied, the assumption of uniform outlet glacier response, etc. etc., is documented. It seems incumbent on the authors to assess the sensitivity of their results on the nature and constant values of this equation, so that they can assert that the apparently significant effect of marine-based acceleration they find is potentially realistic, given the very simple parameterization used.

This comment is largely congruent with the major concern of reviewer #1. Main aspects have been addressed in detail in the answers to this reviewer. The new aspect here is the sensitivity of the projections to the single parameter (now called α) in Equation (3). We agree that without knowing the sensitivity, the projections are difficult to assess.

Corrected by including a paragraph and a table on the sensitivity. *When the model was initially set up for the presented projections, the determination of this parameter was an essential step. For 3 values, projections for the entire ensemble and all scenarios were computed. These results are now briefly presented in Sect. 5. The main messages are:*

1. The choice of this parameter mainly influences the present day contribution from ice discharge increase to total ice loss. If the parameter is chosen such that this partitioning reproduces the observational record, we expect a sensitivity of the projected sea-level contribution of about 15% by 2100 and less than 10% by 2300. This is significantly below the spread in projected ice loss introduced by the climate models.

2. The sensitivity of the sea-level projections decreases with increasing amplitude of the future warming and with the length of the projections.

3. The RMS deviation of the ensemble ice loss for single RCPs is very robust under the different choices of this parameter.

General: there is significant emphasis placed on matching recent ice discharge trends, yet the model is forced with ocean forcing from ocean models, which cannot be expected to match the phase of observed climate variability.

We admit that the model is not forced with ocean temperature reanalysis data for the reference period 1958-2005. As we do not expect that the AOGCMs reproduce recent trends in atmospheric and oceanic warming around Greenland, it indeed has no sense to tune the discharge increase to observations. As mentioned in Sect. 2.2 (now 2.3), the tuning aims at the relative contribution of ice discharge and surface mass balance to the total ice loss of the model ensemble. The only assumption is therefore that AOGCMs are able to reproduce the relation between oceanic and atmospheric warming realistically, as this directly controls the relative ice loss contributions under a warming.

Rephrased passages *in Sect. 4. (in line with later comments) to clarify the difficulty of a direct comparison of observed and modelled changes in ice discharge.*

General: some main conclusions of the paper are not new, in that they have already been documented in other papers (including papers by the present authors).

This comment is similar to aspects raised in the reviewer's initial remark and is briefly discussed here. ***For details refer to answer to the first remark.***

The novelty of this study lies in the ensemble approach covering multiple AOGCMs and the four RCP emission scenarios, suggested for the AR5. Informed by earlier studies, two parameterisations are suggested that address the dynamic response of the ice sheet to ocean and atmospheric forcing. Altogether, this allows an assessment of the full range of the future evolution of the Greenland ice sheet under many possible climate trajectories. In this way, the presented study clearly stands out against earlier work.

General: ensure all figures are referenced in the text, and in order.

We found that all figures were referenced in the right order. We assured this also for the revised manuscript.

No correction necessary.

Specific comments:

Abstract:

Line 6: 'initialized' often seems to refer to inverse or adjoint-based procedures these days. I recommend 'spun up' or similar term, instead

Corrected *in the entire manuscript as suggested.*

Line 11: SLR referenced to which base sea level? Present day?

Corrected. *Base line is the year 2000.*

Line 12: why only low emission scenarios considered to 2300?

Refer to response to reviewer #1.

Added explanation in Sect. 5.

Line 19: refer to other papers which also find a minimal contribution from basal sliding to long-term response

Corrected by adjusting the abstract. *The references itself are given in the main text of the manuscript.*

Line 23: also refer to other papers which find large climate-side uncertainty (mentioned more below)

Corrected *according to reply to later comment in this review.*

P3853L7: suggest, refer to Enderlin et al for updated breakdown of surface melt vs. marine ice loss (system is trending towards relatively more surface melt)

Corrected. *Two references were added and main findings are explicitly specified.*

P3853-3854: this detailed overview of recent yearly-scale ice sheet 'weather' is interesting, but perhaps out of place or at least overly detailed, in a paper that looks at the long-term ice sheet 'climate' response. Suggest a summary that is relevant to the simulations detailed later in the paper.

The introduction has been shortened to be more concise and to the point.

P3855L2: if there's a physical explanation it isn't a coincidence

Adjusted the formulation *as follows: '[…] simultaneous occurrence […]'*

P3855L8: suggest referencing recent meltwater–velocity studies that suggest that annually-averaged effect might be small.

Corrected. *A recent reference is added (Tedstone et al., 2014) suggesting that the effect from lubrication on summer to winter differences might be small.*

P3855: negative trend since 1990s may be largely due to NAO, and not a long-term secular trend - see Fettweis work.

Reformulated. *We want to avoid invoking any notion on whether the recent SMB decrease is linked to long or short-term climate variations/trends.*

P3856,L5: "The physical complexity..." this sentence is unclear to me.

Reformulated *as follows: 'Computational constraints typically limit RCM applications […]'*

P3856,L16: "This feedback..." I do not understand this sentence. For example, it is not only ice discharge that affects geometry, and therefore SMB. In fact SMB affects geometry as well.

Deleted sentence *as it did not add anything.*

Section 2: suggest making separate "Ice sheet dynamics" and "SMB" sections

__Adjusted__ by adding the relevant sections.

P3856L17: Perhaps it is worthwhile to note other efforts to model more ice dynamical processes in ice sheet models... for example, others have certainly pubilshed ocean forcing effects, and/or runoff-based basal lubrication experiments. To what extent are your experiments a unique contribution?

The comment addresses a passage where a short overview of the manuscript is presented, not suited for full referencing. Relevant literature is given in Sect. 2 where the parameterisations of the dynamic processes are specified and in Sect.5 during the discussion of the results.

No action taken.

P3857L11: I think you need to expand on the description of the nature of the 'higher order' ice physics in this paper, even if it is described more fully in other publications. Especially given that a main point of your paper is "Here we use a higher-order ice flow model..." (third sentence of abstract). At this point the nature of the higher order physics is mysterious to readers of this paper.

__Added specific terminology.__ Reference to Hindmarsh (2004).

P3857L17: the term "parametric SMB model" is not clear - to me, this refers to an SMB model that, for example, parameterizes things as a function of latitude... which I don't think is the case here.

Though we think that the term is self-explicatory, the authors want to avoid any misunderstanding and the concerned passages were rephrased.

Rephrased passage for clarity.

P3857L26: perhaps more justification would be good here, as to why you think PDD method is robust, especially in the far future, and with constant assumed variability in daily temperatures.

The PDD model lends its robustness from a basically correct simulation of present-day annual mass balance characteristics and an acceptable simulation of the past ice-sheet history. No attempt was made to change these parameters in the future owing to a lack of solid information on how these might change.

No action undertaken.

P3858L1: does the snow model have multiple layers? Is there a multi-year memory of capillary water, and how does the capillary space compact with time? More explanation here as to the complexity/simplifications of the snow model would be good.

The snow model consists of only one layer. All capillary water is assumed to refreeze over the next winter turning the saturated snow into superimposed ice.

Additional details mentioned in text.

P3858L8: suggest a quantitative comparison to back up this currently unsupported statement of a good comparison to RACMO variability.

> *R^2 coefficients of determination between annual mass-balance components from our model and RACMO2.1/GR for the period 1958-2010 were 0.79 for SMB, 0.84 for precipitation, and 0.75 for runoff (Hanna et al., 2011). Such values for R^2 are considered to indicate a good agreement. A quantitative comparison between the SMB model, used here, and other RCMs is presented in Sect. 4 as evaluation during the reference period.*
>
> ***R^2 correlation coefficients added in text.*** *Quantitative comparison in Sect. 4.*

P3858,L10: suggest moving this description of ISM discretization up to an "Ice dynamics" section

> ***Corrected as suggested.***

P3858L12: "geometric input": do you mean bed topography?

> *Indeed, the model primarily requires the bed topography as input. However, information on ice thickness and surface elevation is also required for the initial model setup and when it comes to the evaluation of the present-day state after the free evolving spin-up phase (see Sect.2.3 (now 2.4)). Therefore we keep 'geometric input'.*
>
> ***No adjustment was considered necessary.***

P3858L13: "slight adjustments": describe briefly for completeness

> ***Added reference to Goelzer et al. (2013).*** *For details see response to review #1.*

P3858L17: can you describe these Gaussian functions more, or provide a reference that explains them?

> *The Gaussian functions correct the background geothermal heat flux in the vicinity of ice-core sites with (i) the value derived from temperature calculations at ice-core sites where basal temperature has been measured (ii) a gradual decrease of the difference with respect to the background field with distance according to a Gauss function with a standard deviation of 100 km radial distance. The method is described more fully in Pattyn (2010): Antarctic subglacial conditions inferred from a hybrid ice sheet/ice stream model, Earth and Planetary Science Letters 295 (2010) 451-461, doi: 10.1016/j.epsl.2010.04.025*
>
> ***Additional explanation and the reference to Pattyn (2010) is added.***

P3859: Schoof (2010) and others don't so much relate sliding to annual average (or cumulative?) runoff, as much as to large, individual events. Also, see, e.g. new paper by (Andrews et al., Nature). So, it isn't so much the values integrated over a year, but more the amount of discrete events: : : Based on this, I think your justification for your particular sliding law needs more justification, even if it turns out it isn't important.

We agree with the reviewer that the research interest is often in singular or successive but distinct speed-up events rather than the cumulative effect. The melt component of the SMB model however works with monthly mean temperature fields using a statistic to account for daily variations. Thus the model is not meant to produce individual melt peaks and therefore our interest is in the integrated effect. Sundal et al. (2011) argue that mean summer speed-up is positively correlated with daily runoff as long as runoff rates do not exceed a certain threshold. As this timescale is not explicitly resolved in the SMB model, we decided to work with annual values. We therefore accept that a certain value for local annual runoff can be obtained in various ways over the melt season. In general however, the annual value should scale with the periods during which a critical daily runoff is exceeded.

Reformulated paragraph with main reference to Sundal et al. (2010).

P3859: is S_BL spatially varying? Or applied ice-sheet-wide?

Yes, S_BL varies spatially. An ice sheet-wide average would not have much meaning.

No correction necessary.

P3859,L14: so if no surface runoff, then S_BL=1?

The reviewer is correct. ***No action undertaken.***

Equation 2: Can you possibly refer to the plot of this relationship here, instead of a few sentences lower?

Corrected as suggested.

Equation 2: can you provide more physical and/or theoretical basis for this equation? At present is seems quite arbitrary to the naive reader why this form was chosen.

We justify our choice by referring to the best-fit parameterisation of the lubrication effect in Shannon et al. (2013).

Added reference.

Equation 2: are you solving for the basal drag as part of your force balance, or does it come from some prescribed basal drag field?

In this model variant, the basal sliding coefficient (A_S) is modified spatially by the two parameterisations. If runoff is locally non-zero, sliding is increased following the lubrication argumentation. Close to the marine margins, A_S is increased/decreased according to ocean temperature changes. So yes we solve for the basal drag as part of the solution.

No corrections necessary.

P3859L27:"...with annual accelerations of up to 20% above the winter background: : :": it is not clear what this sentence means. For example, acceleration is not the same units as winter background (velocity?). Do you mean the annually-averaged velocity is 20% greater than the winter velocity? Or peak summer velocity is 20% greater?

Corrected as suggested. *We mean annual mean velocities.*

P3860L5: again, do you mean acceleration peak, or velocity peak?

Corrected for velocity peak.

P3860L15: did you intend to 'hold back' Swiss Camp velocity data to use as validation (as opposed to tuning)? If so, perhaps state this clearly. Else, bundle the Swiss Camp comparison into earlier discussion of the K-transect data-based estimates of a,b and c.

Observations at these locations either are affected by the vicinity of the marine front or can only serve to give limits on the functional dependence.

Not adjusted.

P3860L15: Now, for Swiss Camp, you are discussing 'annual motion increases' and not 'accelerations'. Suggest using the same metric for all discussion.

Corrected as suggested.

P3860,L20: the term 'annual speedup' is unclear. Do you mean, increases to annually averaged velocities, relative to other years? Also, the 'of not more' is confusing. What happens for runoffs of greater than, e.g. 1m/yr (for the Swiss Camp discussion)?

Adjusted *according to previous comments on terminology.*

P3860L25: "Yet the approach: : :" this sentence is perhaps in the wrong spot? It appears to be an assessment of the runoff component of the SMB model. There is no mention of ice velocity changes here. Also, which is the unnamed model that is mentioned here?

Removed the sentence. *Repetition. The effect from differences between modelled and observed runoff on the lubrication parameterisation is already discussed earlier.*

P3861L5:"...to temperature variabliity diagnosed from five ocean basins in available AOGCMs for the decade 2000-2010." AOGCMs do not capture the 'absolute' timing of climate variability. So there is no reason to expect that AOGCM ocean variability is at all syncronized with observed ice sheet variability.

We fully agree with the reviewer on this point and the issue is amply discussed further below in the discussion Section of the manuscript.

No correction necessary.

P3861L13: how can regional climate models infer ice discharge variability?

Reformulated to clarify. *We meant that observations on volume change and SMB estimates from regional climate models can serve to discern the variability in ice discharge.*

P3861L20: "... support the choice: : :": in what way?

The exponential relation was adopted because this gave the closest agreement of increased discharge with the mentioned studies for a similar warming scenario.

No correction necessary.

P3861L21:"The selected relationship is calibrated such that the ice sheet model reproduces the relative contribution of the discharge increase to the total ice loss over the last decade in response to the considered climate models": As commented before, I am not convinced that this is a robust approach, given that AOGCMs aren't expected to actually simulate the phase of decadal-scale variability in an absolute sense (if they do, it is simply a coincidence). So, calibrating the ocean-discharge to the 2000-2010 period almost certainly introduces aliasing due to inaccurate sampling of the simulated climate record. Why not calibrate rather to something much closer to observations, such as e.g. the World Ocean Atlas?

Here we want to refer to our detailed reply to the second comment by the same reviewer. We agree that AOGCMs are not expected to reproduce decadal scale temperature variability in an absolute sense (as mentioned in the manuscript). Therefore we decided to calibrate the relative contribution of ice discharge increase to total ice loss. In this way, we only assume that oceanic and atmospheric warming are related such that the ice-sheet model reproduces the observed partitioning of the ice loss. Generally, the climate models show a warming during the decade 2000-2010, but the simulated ensemble-mean ice loss is lower than was observed. This just reflects the incapability of the climate models to reproduce the magnitude and timing of the observed warming over Greenland. Yet our tuning allows the reproduction of a realistic partitioning of the ice loss, assuming that the relative warming in the ocean as compared to the atmosphere warming is reliable in the AOGCMs. Therefore we believe that our tuning holds.

Not adjusted *as a similar discussion is included in Sect. 4.*

Ocean reanalysis data would have certainly been preferable but multi-decadal time series were not available to us at the time. Though surface water temperatures are well known, we have doubts that reanalysis temperatures are well constrained at intermediate depth around Greenland. That's where we require reliable information on temperature variations to tune the parameterisation.

Equation 3: what determines the constant values in this equation?

Sensitivity study and table added. *For details see above reply to general comments.*

Equation 3: as previously mentioned in General Comments, given the importance of this equation to the final results, I think the authors need to do more work to assess uncertainty their results coming from uncertainty in this parameterization.

Sensitivity study and table added. *For details see above reply to general comments.*

P3862L5: "more regular than the amplification of the sliding coefficient: : :" this point statement is unclear to me.

Reformulated text. *We removed the unclear formulation and rephrased this passage as follows:*

'For a one-degree centennial warming, the sliding coefficient is increased by a factor 5.2 after 100 years. Yet, ice discharge does not even double. One reason is a geometric adjustment and thinning at the marine margins that limits the attainable ice export (Fürst et al., 2013). Another reason is that basal velocities do not necessarily scale linearly with changes of A_S in a higher-order flow model.'

P3862L11:"As initialisation"->"For initialization"

Corrected as suggested.

P3862L21: "Experiments have shown"... for this statement and others like it, without a reference I think the authors need to provide some form of (even just basic) quantitative description.

We refer here to our own experiments.

Reformulated the sentence to that effect.

P3863L2: LHS technique has also been used by several other ice-sheet-specific studies (e.g. Applegate et al 2010, Fyke et al 2014).

Added references.

P3863L3: DDF factors were previously stated to be definite values: : :"Melt rates are then determined: : : with degree-day factors : : : of : : : 0.0030 and 0.0079: : :" but here they are allowed to vary as LHS parameters. Perhaps the text could be made more consistent.

Corrected *by adjusting the description of the SMB model such that it is clear that these values are determined during the model tuning in Sect. 2.3 (now 2.4).*

P3863L6: these +/- ranges seem arbitrary (e.g. 36-450% for m).

No correction necessary. *As reviewer #1 makes a similar comment, we refer to our elaborate reply there and keep the discussion brief here. The chosen parameter ranges allow significant influence on the spin-up. Together with the fact that the best-fit combination does not hit any range limits, our selection receives justification.*

P3863L19: It would seem important to state the size of the LHS ensemble, to ensure that the parameter volume is sampled statistically sufficiently (rule of thumb 10 ensemble members/free parameter).

> ***Corrected by adding the number.*** *We used 100 samples, thus twice the amount according to the reviewer's rule of thumb.*

P3863L18: By what method are the criteria actually combined to determined the 'best' ensemble member?

> *The authors combined all criteria into a single value by a linear combination. Yet this value could not be used for the final decision. We want to illustrate the issues that arose on the basis of the ice volume criterion. As mentioned in the text, the simulated ice-sheet margin is typically too thick. In order to match the total ice-sheet volume, parameter combinations with thinner ice in the interior would be preferred. It is difficult to assess what is an acceptable offset/uncertainty in this criterion for such a spin-up without loosing in efficiency to discriminate sample members. This argumentation is readily transferable to some other criteria and inhibited to find the best fit from a single value assessment. We therefore opted for a more qualitative assessment of all sample members to select the best-fit.*
>
> ***Corrected by adding:*** *'One best-fit, reference parameter set and 7 additional combinations were selected on the basis of a qualitative assessment of respectively all or individual criteria (Table 1).'*

P3863L19: Table 1 shows 7 parameter sets, not one.

> ***Corrected*** *by reformulating passage (see previous reply).*

P3863L25: To what extent does switching to ECMWF anomalies, then to AOGCMbased anomalies, introduce step functions in the SMB forcing?

> *The background fields of precipitation and temperature remain unaffected by switching to ECMWF anomalies in 1958 and later to AOGCM anomalies after 2005. As all anomalies are formed with respect to the period 1960-1990, the moments where anomalies are spliced fall into a 15-yr range. In atmospheric sciences, periods of more than 30 years are considered climatologically significant and thus changes on shorter time scales are considered inter-annual variability. Consequently, the discontinuity at 2005 is generally below the magnitude of the inter-annual variability in temperature and precipitation. Prior to 1958, the anomalies from ice core records are used which are also referenced to the period 1960-1990. By definition, this transition is also dominated by inter-annual variability.*
>
> ***Adjusted by adding sentence in Sect. 3.1:*** *'Discontinuities in these anomalies, when switching the forcing in 1958 and 2005, generally fall below the inter-annual variability.'*

P3864L5: what are the baseline ocean temperatures onto which anomalies are applied? What oceanic anomalies are used for the historical period?

As mentioned in the manuscript, the baseline ocean temperatures are the 1960-1990 average fields for the AOGCMs. For the historic, spin-up period, no ocean anomalies are applied. Ocean forcing gradually starts in the reference period.

No correction necessary.

P3864L12: "... and their capability: : :" what exactly is being assessed here in terms of AOGCM performance? And what measure is taken to assess whether the particular metric for model performance isn't being compensated for other climate model behaviour that could affect future simulations?

Our assessment is based on the temperature product of all AOGCMs. This involved a comparison of 1960-1990 averages to the ECMWF reference. In addition, models where rejected if the mean surface air-temperature increase over the projection period was a clear outlier as compared to the entire AOGCM ensemble.

Reformulated by specifying: *'The selection of climate models was based on the scenario coverage, the covered projection period and whether surface air temperatures, averaged for 1960-1990, generally agreed with the ECMWF product. Outliers in terms of average warming by 2100 and 2300 where identified from the AOGCM ensemble and hence rejected.'*

P3864L16: "...is used to avoid a bias by the mean states of the AOGCMs". But future projections could still be potentially affected by AOGCM biases. For example, if the climate model is too cold around the GrIS margins, then the warming to the point where a 0C surface (the maximum surface temperature of snow/ice surfaces) is obtained will be greater. When applied as an anomaly, this will artificially appear as greater warming (due to the initially cold state). Can the authors potentially assess in any way that AOGCM biases are not suspiciously correlated to the temperature changes they simulate in future projections?

We are aware of this possibility. However, we believe that our prior AOGCM selection could already reject badly performing models that were clearly deficient in reproducing Greenland surface temperature during our initial model selection. Since we don't know the details of the surface schemes employed by all AOGCMs we have to accept any remaining shortcoming in the selected CMIP5 suite and feel unable to make the assessment the reviewer point to.

No action taken.

P3864L21: Are the monthly SAT and P anomalies area-mean anomalies over the entire ice sheet, or spatial fields? It seems spatial fields are used, but it is not quite clear that this is the case, from the text.

These are spatial fields. Spatial patterns appearing in the anomaly fields are described in more detail in the next paragraph. Therefore we think, it is very clear that 2D anomalies are applied.

No correction necessary.

P3865L3: "... north south gradient: : :" perhaps note which direction this gradient goes (presumably, more warming farther polewards?)

Corrected *by mentioning direction of gradient in this paragraph.*

P3867L3: I think a plot of the difference between observed and simulated ice thicknesses would be very important for the reader to see.

The overestimation of margin thicknesses after spinning up an ice flow model for large-scale geometries is a well-known and well-quantified effect (refer to the 4 references given in the text). The next comment of the reviewer even refers to this as common ice-sheet model deficiencies. In conclusion, the authors consider it a redundant exercise to present and quantify these differences here.

No additional figure introduced.

P3867L6: It would seem to me that thicker margins would actually cause a faster velocities right near the margins (due to steeper surface slope to the margin). Perhaps instead, the lower margin ice velocities can be attributed to the relative lack of ice streams or other (common ice sheet model) deficiencies?

Without regard to the reasons of these higher ice margins, ice thicknesses are increased over some tens of kilometres upstream. The resultant relative changes in surface slopes are typically much higher than in the thickness field. From a simple SIA point of view, this will lead to reduced velocities. In addition, consider a steady-state ice sheet that has to export a certain accumulation. If a certain flux has to pass an area of thicker ice, lower flow velocities are required (irrespective of whether this is controlled by sliding or deformation).

No corrections required.

P3867L18: "On 5 km resolution, ice flow toward the margin is more channelised: : :": relative to what?

The comparison is with respect to earlier model versions on coarser grids.

Corrected *by referring to model versions with coarser grid sizes.*

P3868L20: As noted previously: climate model simulations cannot be expected to capture the absolute phasing of climate variability. Thus, while SMB from ECMWF-based atmospheric forcing can be assessed compared to, e.g. 2005-2010 period, the '2005-2010' ocean forcing from climate models cannot be assumed to be on the same climate variability pathway as the real world. So, it is likely that HadGEM2-ES fortuitously simulated the ocean T change over this period correctly (was this why it was somewhat arbitrarily highlighted?). I note that the authors do seem aware of this general point, from the statement "not all AOGCMs are expected to correctly reproduce the real trend over such a short time period". I would strengthen this statement to something like: "no AOGCMs are expected to correctly reproduce the real trend over such a short time period, except by pure good luck." and ensure that this fact is represented throughout the manuscript and methodology.

We fully agree with the reviewer's comment. This issue has already been discussed in response to various comments from both reviewers (see above). Therefore we only specify our undertaken action for this passage.

Corrected the statement as suggested to: *'This reflects that AOGCMs are not expected to correctly reproduce the real trend over such a short time period.'*

P3869L3: An average increase, relative to what?

Relative to the 1990-2000 average.

Added reference period as follows: *'[…]with respect to the average value in the 1990s.'*

P3869L3: Also, again, it is not clear that assessing ensemble performance over a 5 year period is useful, given that the ensemble average of a set of climate models cannot be expected to reflect the real phasing of climate variability.

Apart from the phasing of the climate variability between model fields and observations, which was discussed in our replies on several comments throughout the rebuttal, the reviewer is certainly aware of the fact, that our assessment, here, is limited by the short observational record on ice discharge. At least, our discussion indicates that the sign of the observed warming by 2010 is generally reproduced by the AOGCMs.

No correction possible *as limited by the length of the observational record.*

Figure 3: Mean annual surface air temperature anomaly is much less important than summer margin air temperature anomaly (which is the temperature subset that actually determines melting in a PDD model). Suggest plotting this instead, or in addition to, mean annual SAT anomaly.

We agree with the assessment of the reviewer on the subject of the importance of the summer temperature anomalies over the ice sheet margin for the simulated melting. Yet it is not evident to define the margin under such variable forcing and such long projections periods. In the most extreme scenarios, the melt area extents over most of the ice sheet. The two options are thus to prescribe a fixed mask or to trace the melt area through time to determine an average temperature increase. In both cases we introduce a bias in the average field. Therefore, we keep the original and objective method to average over the entire ice sheet domain. In addition, the annual average is kept, as we rather want to inform on the general warming over the ice sheet.

Not corrected.

Figure 3: does not show oceanic warming trends, but the text refers to ocean warming trends in Figure 3.

Corrected. *Indeed the wrong figure was referenced. We now refer to Fig.* 5.

Table 4: "mean atmospheric and oceanic warming": mean around GrIS? Global?

The caption for Table 4 indeed did not specify the character of the presented averages. We added this now.

Corrected. *Caption reads now as follows:*

'Ice sheet-wide mean atmospheric warming, basin-mean oceanic warming, and ensemble-average contribution of the Greenland ice sheet to global sea-level change by 2100 AD and 2300 AD. Sea-level changes are calculated with respect to the year 2000. Ensemble averages for each scenario use equal weights for individual AOGCMs. The root mean square (RMS) deviation from the mean ensemble realisation is added to estimate the variability.'

Table 4: is it correct to call the +/- values 'error estimates'? Or are they more accurately 'uncertainty ranges'?

The ranges given here are calculated as an RMS deviation from the ensemble mean, as was specified initially. We reformulated the caption to avoid any misunderstanding.

Corrected. *See previous action.*

P3869L14: It seems strange that if the IPCC AR4 SLR range is smaller, yet you say it additionally considers 'additional uncertainty arising from the SMB model'.

The AR4 sea-level projections comprise a very conservative estimate for future ice discharge evolution. Yet even the AR4 states, that when scaling dynamic ice loss trends with global temperature change, the 2100 sea-level contribution from both ice sheets could be increase by up to 20 cm. This value is equal to the full range from the SMB contribution (both ice sheets) even under the highest warming. As future discharge changes are directly estimated in this study and as they explain about 40% of the entire sea-level signal from the Greenland ice sheet, it seems not surprising that AR4 found lower values relying on the conservative approach for ice dynamics. The variability in the SMB models therefore seems to be lower than the dynamic signal or this variability was simply underestimated by the time of the AR4.

No correction required.

P3869L19: '...for the future discharge increase: : :" yet you say in the abstract that "enhanced discharge decreases over time: : :". Are these statements compatible?

This sentence could indeed be misunderstood and imply an incompatibility with the rest of the manuscript.

Corrected by adapting the sentence *as follows: 'Yet the larger range is attributed to directly accounting for future changes in ice discharge.'*

P3869L23: "The new AR5 suffers from: : :": this statement is a little unclear – suggest rewriting.

Reformulated *as follows: 'The new AR5 is however not able to quantify the importance of the interaction between ice dynamics and surface mass balance, as it suffers from the fact that the considered studies are not directly comparable either in terms of forcing or setup.'*

P3870L12: Can you explain why the high-emissions scenarios weren't continued to 2300?

The authors want to only briefly answer this comment as a more comprehensive reply is given to reviewer #1. For RCP6.0, the reason for this decision is that many AOGCMs were not continued beyond 2100. For RCP8.5, available AOGCM data was limited to a few models, showing a very large warming spread, making our ensemble approach questionable.

Reformulated sentence as follows: *'As AOGCM input was not available for RCP6.0 beyond 2100 and as the divergent response of the few AOGCMs under RCP8.5 is not considered compatible with our ensemble approach, projections were continued until 2300 AD only for the two lowest scenarios.'*

P3870L19: For the ISM runs forced with RCP26 and RCP45, is SMB typically negative at the end of 2300? This would give some indication as to whether a true 'stable' ice sheet configuration has actually been reached.

We are very grateful for this comment from the reviewer, as it adds another nuance to the brief stability discussion in Sect. 5.

For RCP2.6, all models show still a positive total SMB when averaged over the last 10 years of the experiment. This certainly is a first indicator for future stability. The picture changes for RCP4.5, where the SMB by 2300 falls below zero for most of the AOGCMs. For the three AOGCMs with lowest warming, the ice sheet shows an SMB close to zero or slightly positive.

Added information on the sign of the SMB by 2300.

P3871L2: "With forcing from MIROC-ESM-CHEM": perhaps note for completeness why this particular model was used.

Replaced by general sensitivity analysis covering all AOGCMs. *Particular AOGCM reference no longer mentioned as a general sensitivity study covering all climate models is introduced.*

Figure 9: This figure is nice, but really dense (and too small). Perhaps a clearer form of conveying this information? Also, it is not clear what the % values in each panel actually represent. Also, the 'overcompensation' due to negative discharge cumulative effects is not clear to me, at least via the graphics representation.

This is the central figure of our study and the authors worked hard on a representation that summarises the effect of ice discharge on the future ice volume evolution for each AOGCM and each scenario. Deliberately this figure is very dense. In this way, it might not be evident at first what the stated relative contributions mean in detail. Consequently, the authors reformulated the caption to better clarify and specify these subtleties.

Reformulated parts of the caption *to clarify the denoted relative values.*

P3873L11: doesn't Figure 10b show the relative thickening effect?

The references to Eq. (2) and Fig.1 were meant to guide the reader back to the respective parameterisation. The reviewer is right that the relative thickening is shown in Fig. 10b. This figure is already referenced a few sentences before.

***Corrected**. Added reference to this figure.*

P3873L26: Again, I'm not confident that the model architecture is suitable for making statements of rates of ice mass change over the very short 2005-2010 period. To that extent, I would suggest that the good comparison to, e.g. Shepherd et al., 2012, is only fortuitous.

Reformulated passage according to the above discussion on the raised issue here.

***Corrected as follows:** 'When considering climate forcing from ECMWF reanalysis data and ocean temperatures from an AOGCM that shows an expressed warming over the period 2005-2010, we find an ice loss rate of 0.62 mm/yr over the same period that is explained by ~40% from increased ice-discharge, in agreement with the observational range.'*

P3874L25: This conclusion, that the largest source of uncertainty in ice sheet mass changes comes from SMB (i.e., climate), has been demonstrated by others previously: see, for example, Pollard et al 2000 (10.1016/S0921-8181(99)00071-5), Quiquiet et al 2012 (10.5194/tc-6-999-2012), Yoshimori et al 2012 (10.1175/2011JCLI4011.1), Fyke et al 2014 (10.1007/s00382-014-2050-7), and probably others as well. Suggest the authors reference and discuss at least some of these existing studies, with respect to this finding.

This conclusion certainly supports many studies on ice sheet volume projections on century and millennium time scales, including some of the early work of one of the authors (Huybrechts). Yet this conclusion is continuously challenged, especially when looking at (very) short observational records of changes in calving rates, and therefore merits to be stressed once more. Our study moreover adds a side-aspect in that uncertainties on volume evolution arising from future ice discharge changes are comparably smaller than the climate uncertainty. This conclusion finds even more back-up from the sensitivity study added during the revision of this manuscript. Nevertheless, we agree to acknowledge the more recent studies on this subject (Yoshimori & Abe-Ouchi, 2011, Quiquet et al., 2012, Fyke et al., 2014), not yet referenced in this manuscript.

***Corrected** by including and referencing these studies in the discussion of our results in Sect. 5.*

P3875L6: Similarly, the finding that ice discharge at calving fronts is self-limited by ice dynamics (and the competition from SMB increases) has been shown previously, for example, even in some nice papers by the authors of the present manuscript (Goelzer et al., 2013), but also, Gillet-Chaulet (2012/2013, :10.5194/tc-6-1561-2012), Lipscomb et al., 2013 (10.1175/JCLI-D-12-00557.1), and perhaps others.

We agree that the concept of self-limitation by geometric changes is by itself not a new finding and has been suspected by many earlier on. The novelty is that even when scaling ice discharge with climatic variables, as in our study, ice discharge is still destined to decreases on the long term.

***Added and discuss references** in Sect. 5 where this effect is assessed in detail.*