

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

Reply to List of Comments

by JJ. 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 #1

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General comments

The paper uses numerical modelling to assess the relative contributions of surface mass balance change and accelerated discharge from marine terminating outlet glaciers to Greenland ice loss during the next three centuries. Overall the paper is interesting and makes a good contribution. However, a couple of elements of the methodology need explaining in more detail, particularly the section linking ice velocities and oceanic forcing. There are a number of small errors in grammar / syntax that should be addressed. I give my detailed comments below.

We want to thank the reviewer for the overall constructive comments. These comments helped to improve the legibility and clarity of the manuscript. We have addressed almost all the comments in the revised manuscript.

Specific comments

Abstract Line2: specify dates

Corrected. *The last decade refers to the period 2000-2010, as is now added.*

Line 9: What about other potential controls on ice discharge, e.g. hydrofracture causing an increase in calving or loss of sea ice?

It is clear that ice discharge is influenced and controlled by many small-scale processes which effects are not well quantified to this day. On the floating tongue,

these concern crevasse/damage formation and advection, buttressing from a mélange cover, weakening of shear margins, basal melting that depends on the fjord circulation and the basal melt-water discharge, etc. On the grounded part, ice discharge is potentially affected by a loss of lateral flow resistance or changes in basal friction due to variable water discharge through the basal drainage system. On top of this, the superposition of these processes are likely different for each individual glacier setup. These processes are not directly taken into account in this model application as its large-scale character inhibits to resolve them. The introduced parameterisation rather assumes a first order control from oceanic and atmospheric temperatures. We have clarified in the manuscript that the parameterisation does not aim at reproducing this complexity.

Adjusted Sect.2.2 (now 2.3) accordingly.

Line12: Why use only the low emissions scenarios for the longer runs? The introduction seems unnecessarily long and gives a lot of detail for each example, although the examples themselves are relevant. I suggest restructuring around the idea of oceanic forcing being important (which is the main point), so that the argument is more tightly focused and uses the examples more effectively.

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

For the reason why only the low emission scenarios were prolonged up to the year 2300, see the response to the comment by this reviewer made at P 3870 Line 12 further below.

P 3853 Line 15: Contradicts the previous sentence.

Sentences removed during the shortening of the introduction.

P 3853 Line 19: Add Carr et al 2013.

Included citation.

P 3854 Line 13-17 & 25: Needs referencing.

Re-referenced Murray et al. (2010).

P 3854 Line 26: There is only evidence that the water can overtop sills and a few Greenland glaciers (the Straneo paper looks at 6). This implies it over tops all around Greenland, so please re-phrase.

Adjusted accordingly.

P 3854 Line 28: Indicate how the mélange cover can impact on glacier retreat/calving.

The ice mélange is believed to play a role on the seasonal cycles at the glacier front by the mechanical back-stress it applies on the calving face, but the underlying processes are poorly understood.

A sentence was added to that effect.

P 3858 Line 13: How were they adjusted?

Added the reference to Goelzer et al. (2013). *There the following is specified:*

'We use a Cartesian grid on a polar stereographic projection with standard parallel at 71°N and standard meridian at 44°W, which differs from the standard meridian of 39°W used by Bamber and others (2013). Their dataset is re-projected and interpolated from the original 1km grid to the ice-sheet model grid of 5 km and a geoid correction is applied to reference the dataset to mean sea level. Since the model does not treat floating ice shelves, all floating ice is removed using a flotation criterion for an effective ice density of 910 kg m³ and a sea-water density of 1028 kg m³.'

P 3858: Line 21: Peak melt water doesn't necessarily mean peak ice velocities (Schoof, 2010; Sundal et al., 2011; Vieli et al., 2004). Also, the Rignot et al., 2010 paper focuses on submarine melting, rather than seasonal velocities, so I suggest using another reference.

Corrected accordingly as follows: *'Observational studies often report on successive distinct speed-up events during the melt season (Zwally et al., 2002; Bartholomew et al., 2011; Andrews et al., 2014; Tedstone et al., 2014).'*

P 3859 Line23 & P 3860 line 24: How representative are these values for the rest of the ice sheet and how much does the choice of these parameters effect your results?

As mentioned in the manuscript, there are only a few direct observations on the link between speed-up, runoff and basal drainage. We also mention that the observations might be biased to the western flank of the Greenland ice sheet. Therefore the values are not representative for the entire ice sheet. Due to that reason, Shannon et al. (2013) conducted a comprehensive sensitivity study showing the effect of choosing these values on the centennial ice sheet evolution. They conclude that in any case the impact of basal lubrication on the centennial ice loss is no more than 5% of the integrated SMB. We already referred to this study but we clarify that the sensitivity of the projections to these values is considered small. The manuscript now reads:

'Within a comprehensive uncertainty study on the lubrication effect, Shannon et al. (2013) find that its effect is of secondary importance in terms of the centennial ice volume evolution. Therefore, only one set of parameters is used for the projections here.'

Adjusted Sect. 2.1 (now 2.2) accordingly.

P 3860: Line 15: Why not include these in the calculation of the parameter values?

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.

P 3861 Line 1-11: This approach seems a substantial over simplification of the relationship between ocean melt and glacier velocities. First, there is very large variability in the response of individual glaciers to oceanic forcing within each region, which is likely due to localised topography, so we cannot assume that glaciers will respond to future ocean forcing in the same way as they have done in the past (e.g. if they are now on the far side of an overdeepening). It assumes that all changes in velocity are ocean driven and proportional to the forcing applied, which is not necessarily the case. Looking at the south-east in particular, the speed up between 2000-10 consists of acceleration, followed by deceleration, so looking at a decadal scale response could disguise important detail. I also do not understand how / why these are scaled to the entire ice sheet: is the assumption that all areas of the ice sheet respond to ocean temperatures in the same way? To take an extreme example, we cannot say that northern Greenland glaciers (with extensive floating tongues) will respond in the same way as south-western Greenland glaciers. I think this section needs better explanation and justification.

The reviewer raises important issues that we are not able to answer within the scope of this paper. Apart from the many involved processes that are not well quantified, the small-scale geometric details of these glaciers, that set the frame for their individual responses, are not resolved in this large-scale model application. Despite the expected non-uniform response of glaciers, the pattern of recent glacier accelerations is, to a certain degree, consistent with the variability in offshore ocean temperatures around Greenland (Straneo et al., 2012). Here, we make a first attempt at linking changes in ocean temperature to changes in ice discharge from a limited amount of direct observations and poorly understood processes. The parameterisation is therefore kept simple but is still believed to capture first-order processes informed by more detailed modelling of the most important outflow glaciers (work from Faezeh Nick).

In response to the reviewers concerns, we clarify the deliberate choice for a simple relationship between changes in ocean temperatures and in ice discharge.

Added a passage in Sect. 2.2 (now 2.3) and adjusted the conclusion accordingly.

P 3862 Line 1: Has this distance been used in other modelling studies, e.g. further south on GrIS?

This treatment was inspired by previous work on the inland transmission of marginal perturbations at marine margins with the same ice-dynamic model. This distance was found as the typical distance over which changes at the calving front were instantaneously transmitted by longitudinal stress coupling (Fürst et al., 2013)

Added a reference to Fürst et al. (2013).

P 3863 Line 5: Why were these parameter ranges selected?

Parameter ranges are centred around values obtained from a previous tuning of this flow model within ranges informed from past experience with the model. The range for the positive degree-day parameters is chosen rather narrow as the

model spin-up is very sensitive to them. The ranges for the sliding coefficient and the rate factor are larger, as they are not as well constrained. The range selection obtains additional justification because the best-fit parameter combination does not hit the limits.

Added a sentence for clarification.

P 3865 Line 7: Do you see spatial patterns in precipitation, as well as temperature?

As we could not find a general pattern in the AOGCM precipitation changes, we focussed on the average changes over Greenland, showing an increase.

Added the following sentence. *The patterns of future precipitation changes are also AOGCM dependent and cannot be generalised'*

P 3865 Line 15 onwards: needs references and could be explained more easily by adding these currents and water body names to Fig 4.

Added reference and locations of ocean currents and water bodies to Fig.4.

P 3866 Line 3-6: How representative are these offshore ocean temperatures of what is happening at the glacier front? Is there sufficiently detailed bathymetric data available to identify sills that might block warmer water from entering glacier fjords? Also, how valid is it to use lines of latitude as boundaries for your oceanic units? E.g. using a divider at 70 N means that glaciers located up to 3 degrees north of the Denmark Strait are included in the south-east region. However, it is unlikely that warm water from the Irminger Sea will penetrate this far north and will be much less prevalent than on the south-east coast of Greenland.

We are aware of all the complications of waters penetrating to calving fronts, details of which are badly understood. That's why we introduced a parameterisation linking modelled ocean temperatures to observed ice discharge fluctuations calibrated to observations and more detailed modelling results. In this parameterisation, offshore ocean temperatures are an indicator for the potential temperature changes near the ice front. This finds justification from the fact that warm water at depth near six glaciers all around Greenland has an Atlantic source (Straneo et al., 2012; Jackson et al. 2014). Intrinsically, the parameterisation assumes a direct scaling relation between the offshore and fjord temperature changes. This assumption very likely breaks for short-term warming events and ignores any delays in the ocean system. Yet, adding more details is not warranted by the current understanding of the issues at stake. We assume that the parameterisation captures first-order effects in a more or less realistic way.

Concerning the delineation, we agree with the reviewer that this is just a first attempt to distinguish regional differences. Concerning the dynamic response, results are presented and discussed with respect to an average ocean warming in all basins. Consequently, a sensitivity study on the basin delineation would not add much to the general findings of this manuscript.

Added paragraph on limitations in Sect. 2.2 (now 2.3).

P 3866 Line 10: How much does your choice of depth averaging influence your results?

We present a comprehensive discussion on the sensitivity to the single value (now α) in the parameterisation for ocean-induced discharge changes (Eq. 3). The manuscript was completed by a sensitivity analysis on α . We expect that different choices in depth-averaging of the temperature will show a similar sensitivity, as both together determine the effect on the discharge. Yet we admit, that a decrease in the layer thickness, used for averaging will increase the amplitude of any variability and favour short-term changes. As the parameterisation is meant for long-term trends in temperature changes, the smaller the layer thicknesses the less appropriate the approximation. Averaging over a larger depth is expected to reduce the signal which could be accommodated by increasing α . We therefore think that the added sensitivity study on α largely covers the sensitivity to changes in the temperature averaging.

Addressed by added sensitivity analysis (Sect. 5 + additional table) on other parameter in response to comment from reviewer #2

P 3867 Line 9: Can you quantify 'fairly well'? Some areas look quite different, e.g. north east Greenland.

As the reviewer is certainly aware of, a direct differencing will only highlight inherent differences that arise from the spin-up method. We therefore followed a qualitative assessment to the best of our abilities, as the spin-up is not tuned to exactly reproduce the observed velocity field. Even for inversion methods that aim at reducing the mismatch between modelled and observed velocities under a given geometry, a qualitative comparison is preferable (Morlighem et al., 2013). As we argue in the text, it seems more relevant to capture the regional distribution of ice discharge.

No extra quantification necessary as presented differences in ice discharge seem more relevant.

P 3870 Line 12: Why use the two lowest scenarios? Why not do the highest ones as well?

For RCP6.0, many AOGCMs simply did not continue the projections beyond 2300. For the high impact scenario, available AOGCM data was limited to a few models that showed a highly divergent response, making our ensemble approach questionable. A prolongation of the RCP8.5 ice volume projections would therefore only reflect the dominant uncertainties in the modelled climate response, and undermine the relevance of climate modelling. Therefore, we decided to focus on the first century response.

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.'

Technical corrections

There are a number of minor errors in grammar / syntax and placing of brackets around references throughout the paper, but particularly in the earlier sections. I have highlighted some here, but it would benefit from a detailed proof read.

Corrected. *In the processes of re-editing the manuscript, all authors paid attention to detect minor issues on grammar and syntax. We therefore hope to have been able to remove most of them during this revision. We verified the placement of brackets around citations/abbreviations and could remove some inconsistencies.*

Throughout the paper, 'but' is used at the start of certain sentences. Although this is not technically incorrect, it looks colloquial, so please change.

Corrected by adjusting the respective sentences.

Abstract Line 5: 'with a relative contribution of 40 and 60% respectively'.

Corrected placement of 'respectively'.

Line 13: This sentence has grammatical errors and is hard to follow.

Corrected by splitting the sentence to improve legibility.

P 3853 Line 9: In>During

Corrected as suggested.

P 3854 Line 18: Petermann Glacier (delete 'the')

Corrected as suggested.

P 3854 Line 24: with>of

Corrected as suggested.

P 3856: Line 17: 'Here we include more: : :'

Corrected as suggested.

P 3856: Line 19: 'with the aim of better assessing: : :'

Corrected as suggested.

P 3856: Line 20: (RCP) (Moss et al., 2010).

Corrected by rearranging the sentence structure

P3857 Line 1-3: The model evaluation in the recent past: : ... and the sea level projections for the Greenland ice sheet are presented in Section 5'.

Corrected by reformulating the passage following to the comment.

P3857Line 19: delete 'representative'.

Corrected as suggested.

P 3858: Line 21: on>of

Corrected as suggested.

P 3859: Line 2: 'the annual increase in sliding, relative to the winter reference'.

Corrected in the course of reformulating this passage.

P 3859: Line 13: is preferred> develops.

Corrected as suggested.

P 3860: Line 2: beyond>above

Corrected as suggested.

P 3860: Line 7: 'the mass balance model used here'

Corrected.

P 3862 Line 11: In order to initialise to the::

Corrected as suggested.

P 3862 Line 13: ::::regional surface temperatures, precipitation and sea level.

Corrected as suggested.

P 3862 Line 14: Although the general approach is unchanged [unchanged from what?]: :: (Appendix A).

Corrected by adding reference to Huybrechts (2002).

P 3863 Line 9: separate these criteria using semicolons for clarity.

Corrected as suggested.

P 3864 Line 4: as is often done> as in previous studies.

Corrected as suggested.

P 3864 Line 16: ::::to avoid any potential bias associated with the mean states: ::

Corrected according to suggestion from the reviewer.

P 3864 Line 21: precipitations>precipitation.

Corrected as suggested.

P 3865 Line 5: For a given latitude, the difference in warming between the east and west of the ice sheet depends strongly on the individual AOGCM.

Corrected as suggested.

P 3865 Line 14: inspired> based on / determined from

Corrected as suggested.

P 3869 Line 21: Combine this and the previous sentence and alter to 'but the AR5 is the first to attempt to quantify: :.'

Corrected as suggested.

P 3869 Line 24: ': : suffers from including multiple studies that do not have forcing factors or setups that are directly comparable when: :..

Corrected by reformulating this passages to make it more concise.

P 3870 Line 8-9: twice as high as other RCPs.

Corrected as suggested.

P 3870 Line 24: For runs extending to both 210 and 2300, the sensitivity: :.

Corrected as suggested.

P 3871 Line 7: This comprises both directly induced: :.

Corrected as suggested.

P 3872 Line 26: this is because many of the smaller glaciers: :.

Corrected as suggested.