

## ***Interactive comment on “Ice-dynamic projections of the Greenland ice sheet in response to atmospheric and oceanic warming” by J. J. Fürst et al.***

**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.

General: enhancement of mass loss due to warmer ocean temperatures seems an  
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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., 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.

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.

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).

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

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

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

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

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

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

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more below)

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)

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.

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

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

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

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

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.

Section 2: suggest making separate "Ice sheet dynamics" and "SMB" 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 published ocean forcing effects, and/or runoff-based basal lubrication experiments. To what extent are your experiments a unique contribution?

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

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physics is mysterious to readers of this paper.

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.

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.

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.

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

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

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

P3858L13: "slight adjustments": describe briefly for completeness

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

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.

P3859: is S<sub>BL</sub> spatially varying? Or applied ice-sheet-wide?

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P3859,L14: so if no surface runoff, then  $S_{BL}=1$ ?

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

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

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?

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?

P3860L5: again, do you mean acceleration peak, or 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.

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

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)?

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?

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P3861L5: "...to temperature variability 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 synchronized with observed ice sheet variability.

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

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

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?

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

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.

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

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

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.

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

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.

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

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).

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

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

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

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

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?

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

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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?

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.

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

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

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?

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

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:

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“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.

P3869L3: An average increase, relative to what?

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.

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.

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

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

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

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’.

P3869L19: ‘...for the future discharge increase...’ yet you say in the abstract that “enhanced discharge decreases over time...”. Are these statements compatible?

P3869L23: “The new AR5 suffers from...”: this statement is a little unclear - suggest rewriting.

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

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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.

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

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.

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

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.

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), Quiquet 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.

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.

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