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The Cryosphere Discussions

Interactive comment on "The effect of more realistic forcings and boundary conditions on the modelled geometry and sensitivity of the Greenland ice-sheet" *by* E. J. Stone et al.

Anonymous Referee #1

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General comments

The paper presents results of a sensitivity analysis of a numerical ice sheet model of the Greenland Ice Sheet and shows it to be sensitive to climate forcings, boundary conditions and input parameters. Importantly, the paper shows that future predictions are extremely sensitive to the parameter choices. This sort of work is imperative and topical, and is of significant value to the present literature, and highly relevant subject matter to The Cryosphere journal. Whilst a number of similar sensitivity studies have been published (e.g. Ritz et al., 1997 & Hebeler et al., 2008) this is the first to demonstrate the high sensitivity of numerical models of Greenland for future predic-

tions, which are critical for understanding the impact the Greenland Ice Sheet will have on future sea level change. It demonstrates the need for future predictions to be made with an associated understanding of the sensitivity of the predictions to the parameter sets, leading to a prediction range rather than a single scenario. The paper is also the first to apply a more sophisticated uncertainty analysis technique (Latin Hypercube Sampling) in large-scale ice sheet modelling.

The paper is well written, well structured and easy to follow. There are a number of places the manuscript could be improved however, one methodological/structural issue (discussed under specific comments) and some minor editing issues (listed under technical comments).

Specific comments

I have a couple of problems with the approach of the paper, which you do cover in the discussion, however I think these are very important and the discussion of the future results should be more thoroughly grounded within the simplifications.

The simplifications of the model which are important to consider are a) the impact of running the model offline & to equilibrium (present day runs), and b) not including a full representation of the fast flow (ice stream) features. The first you justify satisfactorily, however the second simplification here is critical and needs to be emphasised more carefully.

Not having any basal sliding in the model is fairly fundamental for the future predictions, and I don't think you make enough of it. The fast flow features (& marine interaction & subglacial hydrology..?) control the evolution of the ice sheet and if the role of these features is not represented, then any conclusions about the future of the ice sheet need to be made with caution. Having basal sliding in the model will mean that you will get more mass loss dynamically so you won't need to have such a high PDD factor (for example), this will affect the evolution of the interior massively in the future runs as you show with plot 12. Also p262, lines 7-9: you suggest that including fast flow features

TCD
4, C194–C201, 2010

could lead to larger dynamical changes. Yes, but if then you can have a smaller PDD factor, then this is a counter argument. Also if the dynamical changes are marine driven, then they will not last very long once the fast flow features are no longer marine terminating.

I think there is still value in the findings as they are, but they must be thoroughly grounded in the simplifications. I think the best way to address this is to restructure the discussion and conclusions. It felt like it ended a bit sharply, the last thing should be what you found, so someone can look to the last paragraph to get the main findings (i.e. the whole first part of the discussion up to p261, line 10, maybe reworded more briefly as a summary). But, these need to be seen in light of the discussion of the sources of uncertainty, which you present well, but you need to more strongly emphasise the problems of basal sliding and resolving the fast flow features. This part is the discussion and needs to come before the conclusion.

The final conclusion paragraph should be a strongly worded message that future predictions for Greenland are highly sensitive to a number of factors, but that this includes the physical basis of the model, and that we really don't have this tied down very well at yet. You don't draw any direct conclusions about the future of the Greenland Ice Sheet from your modelling, and I think this is important not to do that. The conclusion of your work should relate to the model and not to the Greenland Ice Sheet. You have shown that the model is very sensitive to parameters, and hence any conclusions drawn from it about the future of the ice sheet itself should be seen in the light of this sensitivity and the model deficiencies. Otherwise it would be easy for someone to take Fig. 12f, in particular, out of context, to show that Greenland may not exist in a 400ppmv world.

Another structural issue – in sections 3.2 & 4, talk about the datasets in the same order in both sections, it just helps to follow it.

Technical comments

Title

TCD
4, C194–C201, 2010

I don't think the title is worded very well, I particularly don't like the phrase 'more realistic forcings' very much, you don't know conclusively that they are more realistic over the entire ice sheet. You also don't mention parameters when this is a large part of what you investigate. You should probably mention that you run future scenarios as well as investigate the present day. This is hard to summarise briefly, but I think it could be done better.

Perhaps it should be something like: 'Investigating the sensitivity of numerical model simulations of the future of the Greenland Ice Sheet to climate forcings, boundary conditions and model parameters.'

Abstract

p234, line 8: remove 'in terms'.

Introduction

p236, line 3: Split these references to put them after the dataset they refer to, e.g. ...topography and ice thickness (Bamber et al., 2001) as well as...

Model description

p237, line 12: change 'ice-sheet density' to 'ice density'.

P237, line 21: change 'used to change the speed of ice flow' to 'used to change the flow law parameter , and, hence, change the ice flow velocity.'. Then start a new sentence: The flow enhancement factor accounts for...'

p238, eq.4: use a letter for melt, rather than spelling it out.

p238, line 11: singularise albedos & densities: i.e. albedo & density.

p239, line 19: add the words 'surface' before topography to clarify you are talking about the ice sheet surface topography rather than bed topography.

p239, line 23: remove definition of sG, you have defined this already, and you have

TCD
4, C194–C201, 2010

also given it the wrong definition.

Datasets

p240, line 5: change ‘section’ to ‘part’.

p240, line 5: remove the word ‘realistically’.

p240, lines 8-10: change this sentence to a list to make it easier to understand, i.e. ‘...changes under a) steady state present climate conditions, b) ...’

p240, line 15 & lots of other occasions: remove hyphen between 20 and km.

p240, line 21: should be ‘Hsurf’.

p240, line 22: Ta is already defined.

p241, line 7: do you mean surface topography here? Or do you mean bed topography? Check this sentence.

p242, line 11: You state ‘it has been shown to be reasonable for Greenland’ is this based on one of the references? This sentence is a bit unqualified on its own.

Sensitivity

p244, line 23: Add something like ‘this can be explained by’ before the explanation of why the extent changes but not the volume.

p246, lines 8-10: This is a bit awkward, clarify what you mean by the bedrock topography determining the ice thickness.

p246, line 13: change to ‘compared to the Letreguilly datasets, with the ice thickness...’ & other occasions, refer to ‘the Letreguilly dataset’ rather the just Letreguilly.

p246, line 26: again be careful how you refer to the datasets, change ‘Bamber grid’ to the ‘Bamber datasets’.

p246, line 27: Is Table 2 needed? It just repeats what can easily be seen from Figure

TCD
4, C194–C201, 2010

1? You can talk about the % change figures in the text without needing the table.

p247, line 16: This sentence sounds a little harsh on Glimmer!

p249, line 15: See the note above, I don't think you can dismiss basal sliding quite so easily as you do.

p249, line 23: commas around 'therefore' & a few other occasions.

p250, line 12 & numerous other occasions: add a space between W and m-2. It may be neater to change the units to mW m-2?

p251, you mix using 'PDD factor for ice' and alphaice in this paragraph, use one or the other to make it easier to follow.

p251, line 13: add a space between 3 and mm.

p252, line 20: is the optimum parameter really the same for the ice volume as or the max ice thickness? Unless I am mis-interpreting the graph, it seems to be that you haven't reached the optimum flow parameter for ice volume (at 0% change)?

p252, line 28: comma after 'In contrast'.

P253, second paragraph: This paragraph needs tidying a bit – you talk about how the geothermal heat flux could affect the ice sheet through basal sliding, but then say well, actually it doesn't matter anyway because you don't have any sliding anyway.

p253, line 18: 'forcing was so cold resulting in low ice temperatures...' is awkwardly worded.

p254, first paragraph up to spatially varied: is this paragraph necessary? It seems like a bit of repetition.

p254, line 10 & around, also figure 7: Be careful with talking about lapse rates here. You say a 'ice surface extent increases with an increase in lapse rate'. You then go on to quote negative lapse rates when you have positive lapse rates on the figure.

TCD

4, C194–C201, 2010

Technically your lapse rates should be negative, and then is it not a decrease in lapse rates?! Maybe you should talk about more or less negative values?

p254, line 13: remove ‘s’ from ‘results’.

p255, line 18 & other occasions: I’m not sure I like the word ‘setups’, perhaps instead ‘parameter sets’?

p255, line 18: comma after ‘ice sheet’.

p256, line 1: remove capitalisation of Error.

p256, line 5: Add a linking sentence to explain that you will remove any that are similar, otherwise it is hard to follow where you are going with this.

p257, line 1: Remove ‘obviously’. If it’s obvious you shouldn’t have to write it!!

p257, line 21: change ‘ESIMINT’ to EISMINT.

p258, line 13: add km-1 to units for lapse rate, and again talk about in terms of more or less negative.

p260, line 8: reword part starting ‘where the faster...’

p260, line 17: See general comments above – the high PDD factor is probably compensating for the lack of basal sliding. This needs to be emphasised more strongly.

p261, line 23: change to ‘we only initiate the ice sheet model from the present day...’

p261, line 27: change to ‘current net mass loss’ - the ice sheet is losing mass all the time!

p262, line 4: change ‘on’ to ‘in’.

Tables

Table 1: Density should be kg m-3.

TCD
4, C194–C201, 2010

Figures

Figure 1: Increase the font size, it is a little hard to read.

Figure 1: Caption, last sentence is not worded very well.

Figure 7: Lapse rate values should be negative.

Figure 9: change to ‘The small black dots represent...’

Figure 12: where did the ‘2’ come from in future warming scenarios?

Interactive comment on The Cryosphere Discuss., 4, 233, 2010.