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The Cryosphere Discussions
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Interactive comment on "Sensitivity, stability and future evolution of the world's northernmost ice cap, Hans Tausen Iskappe (Greenland)" *by* Harry Zekollari et al.

Anonymous Referee #1

Received and published: 12 January 2017

In their paper *Sensitivity, stability and future evolution of the world's northernmost ice cap, Hans Tausen Iskappe (Greenland)*, Zekollari et al. present results from a suite of high-resolution higher-order ice sheet model simulations. I very much enjoyed reading this manuscript as it describes a set of well-designed experiments and is well-written. I am confident that with a moderate amount of editing this publication will be a valuable contribution to the field of arctic glaciology.

General comments:

Several of the figures need a bit of work to make them easier to read. I will provide specific comments below.

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Discussion paper

Page 6, lines 6 and following: The description of where positive SMBs are permitted is somewhat unclear. (Do I get it right, that a positive SMB on an ice-free area is only permitted, where there is present-day ice cover?) Please rephrase.

Please spend a few lines on why you decided not to take the RACMO temperatures in combination with a lapse rate, but your analytical expression.

Please spend a few lines on how the bedrock elevations were obtained / interpolated. This is one of the key fields for ice flow modeling and low data quality in certain areas might explain velocity mismatches (c.f. Aschwanden et al. (2016)). I expected a few words on this in the model setup section. Can you specify how highly resolved they are (in terms of smallest features that are/can be resolved, not in terms grid spacing in the file)?

While you write that there are several shallow cores from which a precipitation parametrization was derived, you only compare the RACMO data to four cores in Table 1. Are these all cores that can be compared? If not, why/how were they selected? For more data, a scatter plot could be useful.

In section 4.2 please provide the MAR SMB for comparison. Otherwise the main message is a sign flip. There is some overlap with section 5.1.1. Please clean this up.

Section 5.1.4: Disagreement on the ice thickness / surface elevation might not just be the cause of a velocity error. It might also be a consequence...

I don't fully understand why you kept the SMB constant in the 500 m grid resolution experiment. Am I correct in assuming that the SIA experiment was performed with SMB-elevation feedback?

Section 6.2.1
Can you provide summer temperature changes for this region from the CMIP5 RCP8.5 simulations? Is there a matching RACMO-experiment? Polar amplification usually is strongest in winter, which is of little significance to the ice SMB.

Figure 5:

Please (also) plot the difference between your modeled and the observed ice cap thickness.

Technical comments:

Page 1

Line 14:
Please flip the direction of the comparison (SIA is the *erroneous* experiment) and then replace *modifies* with *decreases* (if I got the direction right in the main part of the manuscript). A new text would then be something like *Using the Shallow-Ice Approximation decreases . . . .* I would actually prefer omitting the entire sentence, as I don't see it as relevant to your main message.

Line 22:
Please replace *corresponding* with a word that clearly describes causality (*following?*).

Line 26
I think, it should read *disappear around 2400 and 2200 A.D.* ***respectively****,*
Replace *irrespective* with *independent* (also in other locations in the text).

Page 3
Line 29:
often terminate up to several hundred meters is vague in multiple ways. Also, often is with respect to time, not the individual glacier. Maybe replace with *many of them terminate several hundred meters . . .*?

Eqn (5)
The left hand side should read $\partial_z \tau_{iz}$ (∂_z instead of ∂_i), e.g. eqn 5.76 of Greve and Blatter (2009)

Page 6
Line 19:
sub/ should probably by *sub-*.

Line 20:
Please specify more details on the firn warming. Are we talking about firn modeled by your PDD model?

Page 9
Line 21:
please convert i.e. to w.e. (even if it means keeping the number unchanged).

Line 22:
Please change *occurred* to *were performed* or something similar.

Page 14
Line 2:
Importance **of** initial conditions

Lines 20/21:
Please rephrase *the shape of the volume evolution curve is far less exponential compared to case 1*. Maybe *the growth is slower than in case 1*?

Page 15
Line 11:
Remove *largely*. Do you mean *strongly*? Maybe provide the area change in %?

Page 16
Line 30:
I would suggest using ΔT instead of T.

Page 17
Line 21: I would suggest pulling the reference to Bolch et al. (2013) to the front of the list, as it is the ice cap under investigation in this manuscript.

Figures

Several of the figures have a very dense raster in the background. I find it way more disturbing than helpful. Please reconsider.

Figure 1

I found the figure hard to read. Maybe you could decrease the vertical exaggeration?

Figure 3

(a and b) maybe a discrete colormap would make the comparison between a and b easier. This way, it is virtually impossible to read temperature values from Fig 3b. Maybe you could display RACMO downscaled with the same lapse rate as used in the analytical expression (and with bilinear/... interpolation between the grid cells)? This would make the comparison of the two plots a lot easier. Same for (c,d). Both temperature plots could (should) use the same colormap. The colormap in (c,d) very prominently marks the difference between regions with annual mean temperatures above or below -15C. Is there a specific reason for this, otherwise a linear colormap might be better.

Figure 6b

What are you telling the reader with Fig. 6b? I think, it can be omitted without loss of information.

Figure 7

In 7 a and b you could zoom in a bit more on the ice covered domain. Then it would be displayed bigger and easier to read. I would suggest using the same color scale for the top and bottom figures. Currently one uses linear and the other one log scaling, although the data ranges seem pretty much identical. The color scale in Fig 7 c-f has large ranges where the color hardly changes at all and then sharp transitions (looks like I'd have difficulty telling 60 from 75 m/yr, but 75 to 85 is very clear). A more linear color scale (or focus on the low velocities as in a log scale) would be more helpful.

The 3d-projection in c-f does not seem to add information. To me it's rather confusing.

Most likely a 2d plot would do a better job.

Figure 8
Please add “resolution” in the caption to make clear that 250 m and 500 m refer to the model resolution.

Figure 9
Again, I find the 3d-Plots very hard to read. They appear very dark. I don’t think it’s necessary to cite yourself again for the plot tool. The reference in Fig. 1 should be enough.

Figure 11
I find Fig. 11a extremely hard to read as there is minimal contrast between the colors of lines and text and the background raster. Please change this. The same color problem applies to Fig. 11d. What is the color shading in Fig. 11c about? This should be a line plot.

Greve, R., & Blatter, H. (2009). Dynamics of Ice Sheets and Glaciers. Dordrecht [u.a.]: Springer.

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