Anonymous Referee #1

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.
We thank the reviewer for his positive general appreciation of the manuscript.

General comments:

Several of the figures need a bit of work to make them easier to read. I will provide specific comments below.
The comments regarding the figures have all been addressed and the figures were updated accordingly.

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.

Basically there are three regions:

i. Regions covered by the present-day ice cap: here the ice cap can freely grow
ii. Ice-free regions within the present-day ice cap: here no ice can build up and this is imposed (in order to represent the removal of accumulation through processes that cannot be caught by our model, such as wind drift)
iii. Regions outside the present-day ice cap. Here the ice cap can grow (and to answer the reviewer's comment: the surface mass balance can also be positive in this case!), as long as the particular grid cell is connected to Hans Tausen Iskappe and the bedrock elevation is above -50 m. i.e. the ice cap can only expand from its interior, and not grow from neighbouring ice masses (as these cannot be modelled as they are not entirely part of the domain).

In order to emphasize the latter point (iii) an additional passage was added:

The ice can subsequently expand freely, without any constraints (e.g. can connect to the GrIS), and both negative and positive surface mass balance can thus be obtained for areas outside the present-day ice cap. The ice cap cannot expand for areas where the bedrock elevation is lower than -50 m, where the ice is removed to crudely represent calving.

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.

The main reason for not taking the RACMO temperatures, but rather opting for the analytical expression is twofold:

i. RACMO temperatures contain an imprint of the present-day ice cap geometry and surrounding ice masses. This imprint should not be present for the long timescales considered in this study (up to several thousands of years), having very different geometric settings.
ii. The analytical solution is flexible in its application and can directly be

applied at any model resolution without a need to rely on a downscaling (which would be the case if the RACMO data was to be used)

This is now also explained in the updated manuscript:

A temperature parameterization is preferred over lapse-rate corrected RCM temperatures to remove the bias from the present-day imprint of the ice cap on its own temperature field in a different geometric setting. Furthermore the temperature parameterization is flexible and allows for a direct application at different resolutions, without the need for complex downscaling methods (needed for RCM data)

However note that given the similarities between the RACMO and the parameterized temperatures (see Figure 3 and section 4.1.2.), a relatively similar SMB would have been obtained if the RACMO temperatures had been used in the PDD model, using the same lapse rate).

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)?

This is valuable point raised by the reviewer. The bedrock DEM was constructed by Starzer and Reeh (2001) by a direct interpolation for the interior (with a dense direct measurement network) and a parameterization for the outlet glaciers (where measurements are scarcer). This is now mentioned in the updated manuscript:

Whereas the interior of the ice cap has a dense network of ice thickness measurements (up to several points per square kilometer), measurements on the outlet glaciers are scarcer and here a parameterization relating the surface slope to the ice thickness is used (Starzer and Reeh, 2001)

Given the limited number of direct measurements in the outlet glaciers, a part of the model-observation discrepancy may therefore be related to errors in the bedrock DEM. We also mention this at the end of section 5.1.3 (where the observed and modelled ice cap geometry are compared):

Notice that given the limited amount of direct ice thickness measurements in the outlet glaciers (Starzer and Reeh, 2001), part of the model-observation discrepancy may be related to local errors in the bedrock DEM.

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 the original manuscript this was indeed not clearly defined. There are only four cores available that span over several decades. To clarify this in the updated manuscript we do not refer to 'several' ice cores anymore, but to 'four' ice cores:

The accumulation has been derived from field measurements and four shallow cores that cover most of the 20th century (Reeh et al., 2001)

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.

Good point, as the sign flip is indeed not our message at all! The MAR SMB is now also mentioned in section 4.2:

In another widely used RCM for Greenland, MAR3.5.2 (20 km run, downscaled to the 5 km Bamber et al. (2013) DEM) (Fettweis et al., 2012) an integrated SMB of +0.03 m w.e. a^{-1} is obtained. Given the different topographic input a direct comparison between with RACMO2.3 and the PDD approach is difficult, but also here the RCM output suggests a near-zero SMB for this period.

Section 5.1.1 was cleaned up to avoid any overlap:

The modelled limited areal changes under the 1961-1990 average conditions are supported by the RCM output that indicates a near-zero average integrated SMB over this period (see section 4.2)

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...

This is true and is in fact a 'chicken-and-egg' problem. We now also mention this:

Notice that as the surface velocities and the modelled geometry are related, the surface velocity discrepancy may be a consequence of the geometry discrepancy. The inverse may however also be true: i.e. the surface velocity discrepancy is the cause for the geometry discrepancy.

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?

In both cases (for the 250m/500m comparison and the SIA/HO comparison) the SMB is kept constant to produce the figures (7d,e,f and 8). In this way a 'clean' visual comparison is possible. This is indeed somewhat confusing, as later in the text the effect of the SMB-elevation feedback is discussed (when comparing the area and volume). In order to make this clearer, the reference to the constant SMB is removed from the text (where it is not used), and it is only included in the captions of figure 7 and 8:

Notice that the SMB is fixed in time (1961-1990 climatology applied on the present-day geometry) in order to make a 'clean' comparison and avoid effects related to the 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.

The annual warming is indeed more pronounced than the summer warming. Based on the CMIP5 experiments (IPCC AR5), the annual warming over northern Greenland under RCP8.5 (2081-2100 vs. 1986-2005) is typically around 10°C, while the summer warming (JJA) is typically 5-7°C (50% percentile values of CMIP5 runs). As our reference level is 1961-1990, a +8°C warming can be considered as an extreme summer warming. This is now also described in the updated manuscript and we also added a reference to the IPCC's AR5 'Atlas of

Global and Regional Climate Projections' (Annex I Supplementary Material RCP8.5): *For a high emission scenario (IPCC RCP8.5) the 2100 global average surface temperature is projected to rise by 3 to 5°C compared to the 1961-1990 average. Over high Arctic regions such as Peary Land the temperature could potentially increase by up to 7-11°C due to the polar amplification (Collins et al., 2013). This warming is most pronounced in winter, and summer temperatures (June-July-August) are projected to rise up to 8°C over northern Greenland in 2100 (vs. 1961-1990) (van Oldenborgh et al., 2013).*

Figure 5: Please (also) plot the difference between your modeled and the observed ice cap thickness.
An additional was plot was added (Figure 5b)

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.
This is indeed not one of the main messages of this paper and we therefore decided to omit this sentence from the abstract.

Page 1 – Line 22: Please replace corresponding with a word that clearly describes causality (following?).
Corresponding was replaced with *resultant* to emphasize the causality.

Page 1 – 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).
This was changed as suggested by the reviewer. *Irrespective* was changed to *independent*, also for the two other occurrences in the text (last sentence of section 6.1 and last paragraph of the conclusion)

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 . . . ?
This sentence was modified and now reads: *The outlet glaciers are mostly land terminating and many of them terminate up to several hundred meters above sea level*

Page 3 – 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)
This was modified.

Page 6 – Line 19: *sub/* should probably by *sub-*.
Indeed. This was changed.

Page 6 – Line 20: Please specify more details on the firn warming. Are we talking about firn modeled by your PDD model?
A surface warming is applied where a net annual refreezing occurs, which is only possible above the ELA, where the surface is snow/firn covered. The firn layers and their long-term evolution are not explicitly modelled in our PDD model. In order to avoid any confusion, the reference to firn was removed and the passage now reads: *Based on field measurements (Reeh et al., 2001) a surface warming of 22 K/ m w.e. of refreezing is used.*

Page 9 – Line 21: please convert i.e. to w.e. (even if it means keeping the number unchanged).
This was modified → 20 K / (m i.e.) = 20 K / (0.91 m w.e.) = 22 K / m w.e.

Page 9 – Line 22: Please change *occurred* to *were performed* or something similar.
Changed as suggested.

Page 14 – Line 2: Importance **of** initial conditions
Modified.

Page 14 – 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?
We modified this: *As a consequence of this particular ice supply, here the growth is substantially slower than in case 1 (Figure 9, 1961-1990 -0.5°C).*

Page 15 – Line 11: Remove *largely*. Do you mean *strongly*? Maybe provide the area change in %?
Largely was replaced by *strongly* as suggested and the area change in percentage is now also mentioned: *For a cooling of only 0.5°C compared to the 1961-1990 conditions the ice cap strongly expands (21% area increase) (see Figure 9b) and the volume increases by 26%*

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

ΔT is now being used:

$P = 0.132\,\Delta T^2 + 0.316\,\Delta T + 1$ (11)

Where ΔT is the temperature forcing and P the corresponding precipitation forcing (scaling factor) (both vs. 1961-1990) needed to prevent a mass loss (vs. 1961-1990 steady state).

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.

The order was modified as suggested:

This is in agreement with recent ICESat observations on Arctic ice caps, which indicate a marginal ice loss and local thickening (for the interior). This is the case for the Hans Tausen ice cap (Bolch et al., 2013) and for instance also for Austfonna ice cap (Svalbard) (Moholdt et al., 2010) and the Flade Isblink ice cap (Greenland) (Rinne et al., 2011; Bolch et al., 2013).

Figures

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

The dense raster is indeed not everywhere appropriate. For figures in which a variable is plotted, the raster is kept as this helps to visually identify the exact values. For 2-D representations of a variable (i.e. a plane view visualization) the dense raster was removed (Figure 2b, Figure 7a,b, Figure 8a,b).

Figure 1: I found the figure hard to read. Maybe you could decrease the vertical exaggeration?

The vertical exaggeration of the figure was decreased.

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.

For Fig 3 a,b,c,d a discrete colorbar is now used as suggested by the reviewer. Figure 3b and 3d were not downscaled, as we think it is important to show the model resolution here, which is one of the main reasons not to work with the RACMO temperature output directly (see also related questions above). In Fig 3c and d, there is indeed no reason to have a sharp transition at -15°C. This was now modified by removing the white component from the colormap.

Figure 6b: What are you telling the reader with Fig. 6b? I think, it can be omitted without loss of information.
Figure 6b gives a visual support for the 3-D calculation of the temperature field. It illustrates the change in surface and bottom temperatures with elevation and the strong contrast beween the tongue (where bed is almost at pressure melting point) and the higher parts of the outlet glacier (where basal temperatures are lower than -10°C). Furthermore it also gives a clear visualization of the ice temperature change within an ice column. We would therefore like to maintain this figure in the manuscript.

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 7a and 7b were modified by zooming into the ice-covered domain. The colour scale of the figure was modified as suggested by the reviewer and now the logarithmic scale (that was originally only used for the plane view figures, a&b) is used for all figures (a-f). Our reason for opting for a 3-D projection resides in the possibility of combining topographic information with velocity information, which is not possible in a 2-D visualisation.

Figure 8: Please add "resolution" in the caption to make clear that 250 m and 500 m refer to the model resolution.
The figure caption was modified as suggested by the reviewer.

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.
In order to improve the readability of the 3-D plots, a lighter colour scheme was used for the bedrock. The reference to the plotting toolbox was omitted.

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.
The colours in Fig.11a and Fig.11d were modified in order to enhance the contrast. The colour shading in Fig. 11c is explained in the figure caption: *The blue area broadly corresponds to the range where an attenuation of the mass loss is possible, whilst the red area represents the range under which the ice cap is to (largely) disappear*