

Anonymous Referee #2

General comments

This paper describes a modelling study for an ice cap in northern Greenland, outside the main ice sheet. It applies a coupled ice flow – mass balance model with PDD – runoff/retention model and precipitation downscaled from RACMO. The experiments are well designed and conclusions drawn from this study are interesting, one being that there is no gain in performing time dependent simulations for the ice cap, Holocene or future, as the response of the model is independent of the model initial conditions and there appears to be 4 sets of steady states possible for the ice cap.

The model study shows hysteresis response for a narrow band of temperature forcing (around 0.5°C higher temperature than 1961-1990 average). Authors also point out that the SMB-elevation feedback is a crucial mechanism for the ice cap evolution and stability and that infiltrating meltwater in the ablation area is necessary in order to simulate englacial temperatures. The paper is clearly written and the conclusions are clear, some minor comments for improvements are suggested below. To improve the overall clarity of the paper, better description of the model resolution as well as the temperature forcing would be good, see below.

We thank the reviewer for his positive general appreciation of the manuscript and addressed all his comments below.

Specific comments

It is not clear whether the input fields for the PDD scheme, precipitation from RACMO and parameterized temperatures are downscaled to the ice flow model grid resolution or if the SMB field is downscaled. Clarification in the model setup section is needed, see suggestions below. In several places it is stated that RACMO is run at 11 km and then precipitation downscaled to 1 km, but is it then further downscaled to 500 m or 250 m?

This was indeed not clear in the original manuscript. In all cases the ice flow and the surface mass balance model are run at the same resolution (250 m or 500 m depending on the experiment). In neither case is the SMB field downscaled. This has now been addressed based on the reviewer's suggestions below. The RACMO precipitation is in all cases downscaled to the model resolution. We added a sentence to clarify this in section 4.1:

For all simulations this precipitation field is further downscaled to the model resolution

Some confusion is in the discussion of the results, the description of the forcing is not clear, are all the forcing scenarios shifted relative to the 1961-1990 condition? And then some of them fall onto 1981-2010 or 2005-2014 mean conditions? Figure 9 has both 1961-1990 -0.5°C and different periods, but Figure 10 has all temperature scenarios relative to the reference. Some clarification in the model or experimental description is needed.

Not all forcing scenarios are shifted compared to the 1961-1990 conditions. Some of them are run under 1981-2010 conditions, while others are run with 2005-2014 conditions (with temperature and precipitation for this period). Some runs are indeed based on conditions that are shifted compared to 1961-1990 (-0.5°C and in

figure 10), in which case they have the 1961-1990 precipitation, but another temperature. The reason behind this is twofold: (i) to be able to reproduce colder conditions (cannot reproduce conditions as cold as 1961-1990 -0.5°C by taking a period of >10 years belonging to the ERA-40 period) and (ii) to allow for a continuous range of climatic conditions to be explored (Figure 10). The 1981-2010 and 2005-2014 conditions are indeed close to 'temperature shifted' 1961-1990 conditions ($+0.6^{\circ}$ and $+1.6^{\circ}\text{C}$), but their precipitation is (slightly) different.

This has now been emphasized in the updated manuscript (see response to comments, where this addressed in detail).

Technical corrections

Page 1, Abstract, Line 24, suggest to replace "grow" with "thicken"

This is indeed a better wording. Was modified.

Page 2, line 5, delete "s" on exist

This was modified to *exist*

Page 3, line 15-17, sentence is confusing, how do inconsistencies arise with bedrock from Starzer and Reeh (2001) if the direct ice thickness measurements are not included in the Bamber et al (2013) dataset?

The wording was not well chosen. We mean here that there are differences between the bedrock reconstruction by Bamber et al. (2013) and the one by Starzer and Reeh (2001). In the updated manuscript the sentence is now:

Notice that the direct ice thickness measurements on Hans Tausen from the 1990s are not included in the Bamber et al. (2013) dataset and therefore local differences exist with the reconstructed bedrock from Starzer and Reeh (2001)

Page 6, line 31-32, make sure that the minus sign - sticks with the number

This is the case. In Microsoft Word '-10' is just displayed on two separate lines. But should not be a problem in final TC version (will double check it during typesetting stage).

Page 7, line 24, suggest to add "daily" before variability

This was added

Page 8, here the resolution of the T parameterization could be mentioned, ice flow model resolution?

The temperature parameterization is resolution independent. It needs the grid latitude and elevation, which can be calculated at any resolution. This is now added at the end of the first paragraph of section 4.1.2:

The temperatures are parameterized as a function of latitude and elevation, and they can therefore be determined at any model resolution.

Page 9, line 11, is the precipitation then further downscaled to ice flow model resolution?

Yes, it is. As mention above (see 'specific comments'). This was now added:
For all simulations this precipitation field is further downscaled to the model resolution

Page 9, line 20, what is “frontal SMB” - terminus ablation?

This was indeed not clearly formulated, as frontal SMB may refer to several forms of frontal ablation, including glacier calving. This was now modified:
...for Hare glacier the SMB at the glacier terminus is...

Page 9, line 27-32 – here the different forcing scenarios, shifted relative to 1961-1990 or other periods, could be presented and explained

This is the model evaluation section, and introducing different forcing scenarios does not seem to be justified here. In the response to related questions this issue is further addressed (see below) and the changes in the manuscript should make this clearer.

Page 10 line 6, suggest to replace “further” with “below”

This was modified.

Page 10, line 12, what does “imposed” mean here? is the SMB regridded to 250 or 500 m resolution?

The SMB is not regridded, it is directly calculated on the 250 m resolution. In the updated version of the manuscript this will now be clearer. It is now explained that the RACMO precipitation is in all cases downscaled to the model resolution (see also the two related questions above)

Page 10, line 21, edit, something strange in the sentence “but except”

Indeed. These are now two separate sentences:
...outlet glaciers are ice-covered. Except for this, the agreement is overall relatively good, especially given that there is no imposed constraint on ...

Page 10, line 31, is the grid not at 250m resolution?

Yes, the grid is at 250 m. But to make a direct comparison with RACMO, the SMB calculations were also performed on the GIMP 1-km grid (i.e. excluding SMB differences related to a different topography). In the new manuscript this should now be clearer. Notice that based on suggestions by Reviewer #1, this section was moved forward and changes were performed to avoid repetitions.

Page 11, line 10, suggest to replace “heating” with “heat flux”

This was changed.

Page 11, line 21 and onward, it is not clear what is discussed here, paragraph needs rewriting “With a value of $150 \text{ mW m}^{-2} \dots$ ” - is not clear

This sentence was reformulated as follows:

The modelled basal temperatures for Hare glacier are close to the pressure melting point (see Figure 6b), but nowhere basal sliding occurs

Page 12, lines 1-4, strange sentence, suggest editing

This sentence was modified to:

The observed ice thickness (Figure 2b) is well reproduced in the model (Figure 5) and so is the surface elevation (as the observed bedrock elevation is used in the model)

Page 12, line 17, suggest to replace “to” with “with”

This was modified

Page 12, line 17-20, too long sentence, suggest to split into two

The sentence was split in two:

For the outlet glaciers, many of the observed velocity patterns are closely reproduced. This is for instance illustrated for the main outlet glaciers at the eastern side of the ice cap...

Page 13, line 10, here it seems like SMB is calculated at the ice flow model resolution, is then T and P downscaled to this resolution?

Yes, the temperature is directly calculated at the model resolution (through the elevation-latitude parameterization), while the precipitation is downscaled to the model grid. This was addressed in earlier comments by this reviewer, and the manuscript was updated accordingly (see above for more details)

Page 13, lines 18-21, long sentence, suggest to split or rewrite

This sentence was indeed long. It is now split in two:

Notice that treatment of the ice mask in the downscaling approach has an important effect on the modelled geometry at a 250 m resolution. It is important that the area of the ice cap and ice-free regions is the same at both resolutions in order to ensure that the large-scale dynamics, which are determined by the overall mass balance, are similar.

Page 13, line 25, “which results in smoothing...”

This was modified

Page 14, line 2, something is missing in title, “of”?

This was also suggested by reviewer #1 and was modified.

Page 14, line 23, text is confusing “(vs. “ do you mean relative? Is it only temperature shift?, what are 1981-2010 conditions, do you mean in the RACMO model?)

Yes, by this we mean relative to the 1961-1990 conditions. This was modified in the text. It indeed only concerns a temperature shift.

The 1981-2010 temperatures are obtained by applying a bias on the 1961-1990 temperature field (bias is based on RACMO output, which suggests that the period 1981-2010 is 0.6°C warmer than 1961-1990). Precipitation is directly taken from RACMO over the 1981-2010 period. To clarify this, an additional sentence was added:

The 1981-2010 climatic conditions are simulated by applying a +0.6°C bias compared to the 1961-1990 temperature field (see eqs. 9 and 10), while precipitation is directly derived from RACMO2.3 for this period.

Page 14, line 28, “build up faster than under 1961-1990 condition” – does not make sense, what do you mean by faster?

The sentence was changed and should now be clearer:

Compared to 1961-1990 conditions, less time is needed for the ice cap to build up under 1981-2010 conditions, as there is no interaction between the northern and southern part of the ice cap

Page 15, line 5, replace “parts” with “part”

Was modified.

Page 15, line 15, what does “largely similar” mean?

The 1981-2010 RACMO precipitation is very similar to the 1961-1990 RACMO precipitation. Over the Hans Tausen ice cap, the 1981-2010 mean precipitation is on 4% higher than the 1961-1990 mean precipitation. This is now also mentioned:
(ca. +0.6°C vs. 1961-1990, 4% higher precipitation than the 1961-1990 mean)

And further in the text also for the 2005-2014 climatic conditions:

...which are around 1.6°C warmer than the 1961-1990 average conditions over Hans Tausen (6% higher precipitation than 1961-1990 mean), and...

Page 16, line 15, needs editing, text is unclear

This sentence was modified and now reads:

For the +4°C scenario and by maintaining the precipitation at the 1961-1990 level, the ice cap entirely disappears within 350-400 years (i.e. before 2400) (Figure 11a), disregarding whether the forcing is immediately applied at present or incrementally until 2100

Page 16, line 19 “slightly different transient geometry” needs editing, not clear

This was modified:

...or a somewhat similar geometry

Page 17, line 20-22, text needs editing, two times “potential” in sentence

The sentence was edited:

An in-depth comparison between our modelling study and these observations is again not possible given the differences in timing and the model setup, but our simulations show the potential to reproduce the observed trends and the implications this can have on the future ice cap evolution.

Page 18, line 4, suggest to replace “was” with “is”

This was modified.

Page 19, line 1 “Both” – and then three things are mentioned, needs editing

This was edited by removing “both”:

Englacial temperature measurements, modelled ice thickness and temperatures in outlet glaciers suggest that there is an important heating mechanism related to infiltrating meltwater in the ablation area of the ice cap.

Figure 3 Annual and mean July are in different sequence in the figure and in the caption; (a), b) vs c) d)), suggest to move the precipitation figure to the right column so that RACMO fields are all in the same column

The discrepancy between the figure and the caption is well spotted by the reviewer. This was now modified. The precipitation figure was also moved to the right column (the “RACMO column”).

Figure 5, suggest coloring dotted line white, it is hardly visible as it is now

This was modified: the line is now white and was additionally thickened.

Figure 6 b) is not clear, suggest less vertical exaggeration?

The vertical exaggeration of the figure was diminished in the updated manuscript.

Figure 9 C is the period 1961-1990? Or 2010?

In this figure the steady state obtained from the 1981-2010 climatic conditions is shown (when starting from the 1961-1990 steady state geometry). To emphasize this, the fact that these are steady state geometries (9b-e) has now been added in the figure title.