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The Cryosphere Discussions

Interactive comment on "The added value of high resolution in estimating the surface mass balance in southern Greenland" *by* Willem Jan van de Berg et al.

Anonymous Referee #2

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General comments In this manuscript the authors take a detailed look at the impact of varying the horizontal resolution of the RACMO2 model (from 60km to 2.2km) on the Surface Mass Balance (SMB) of the southern portion of the Greenland ice sheet. Furthermore, they incorporate the impacts of dynamical vs statistical downscaling on the SMB and its various components. Additionally, they investigate whether the resolution is the most important aspect in a modelling study by conducting sensitivity test by varying other factors such as bare ice albedo, firn initialisation and turbulent flux parameterisations. This paper is thorough and detailed, and it is clear that the authors have thought a lot about the contents of the study. However, as stated numerous times throughout the manuscript, RACMO2 is a hydrostatic model which should not be

run (or with caution/parameterisations) at resolutions higher than 5km, but the authors run the model at 2.2km resolution and results from this run are included throughout the study. It is a curious choice that the authors have chosen to run at such high resolutions regardless of the results being 'no longer physically correct', and the lower boundary on resolution in hydrostatic models is not a new finding. It is also a worry that future studies could use this paper as a basis for continuing to use high-resolution hydrostatic modelling despite their limitations, purely as they still provide a good comparison to observations. The authors should provide more information about the model set up, specifically for the convective parameterisations if they are to keep the 2.2km results. The specific comments are outlined below:

Major suggestions: Pg 3, Ln 60: Can you justify why you have used the 2.2km resolution run when hydrostatic models do not hold for such high resolutions? See general comments above.

Pg 3, Ln 46: Please provide examples of non-hydrostatic modelling of Greenland and the Arctic. Specifically, the Polar WRF model has been used quite frequently over Greenland for various climate studies (e.g Hines & Bromwich, 2008., Hines et al. 2011., Duviver & Cassano 2013., Turton et al in discussion with Earth System Science Data). Similarly, Niwano et al (2018) used a non-hydrostatic model coupled with a snow model, and Mottram et al (2017) used the non-hydrostatic HARMONIE model.

Section 2.2: What timestep was used in the model for each resolution? Later in the manuscript you mention 'similar' timesteps, but what are they? What is the height of your lowest model level for each resolution? Will these have an impact on your turbulent flux estimations?

Section 3.2.3 onwards: Taylor diagrams can be difficult to immediately understand, and there are lots of information on them, including shapes, colours and numbers. Whilst you give a good description on how to interpret them, a lot of back and forth between the figures, description and results is required to fully compare results and figures.

Help the readers by pointing out features of the diagram within the text. For example, Pg 17, ln 321 you say 'little to no gain in performance is seen between the results'. It would be useful to follow this with 'as seen in Figure 7b by the clustering of numbers 2,3,4 of the * shape'. Or similar. The authors should go back through the text to see where this information is required.

Section 3.4, Pg 17, Ln 332 onwards: Why is precipitation reduced in the 2.2km runs? Do you parameterise convection in RACMO2 for any of your runs? Or change any of the setup between runs? It would be useful to include a sensitivity test of convective parameters and how this affects precipitation as it is both an important and erroneous component of the SMB.

Pg 18, Ln 342 to 350: This section reads a little confusing. At the start you say that higher temperature and wind speeds contribute to the increased SHF. Towards the end of the paragraph you say that heat advection from the increased resolution is responsible. You should combine these lines of thought, as I assume they are all related to the resolution. Or was the initial sentence (increase T and WS) only related to melt events, and the latter to the full period? Do you have any AWS observations of SHF to compare with? As you could then provide a more definitive assessment than 'SHF fluxes near the margin are probably realistic'.

Pg 19, Ln 367-369: Why do you go down to 2.2km resolution then? See general comments.

Minor suggestions:

P1 Abstract, Ln 2: please add the time period that you cover, in brackets is fine.

P1 Abstract, Ln 13: '25km is sufficient'- where do you get this value from when you use 60km and 20km, and I struggle to find a reference to 25km in the manuscript.

Figure 1: The purple and red dots on Figure 1c and d are very similar looking. Could you try a different colour, or fill the dots on one? Also, later in the text you refer to the

purple ones as 'magenta' which is a better description than purple.

Pg 4, Ln 89: Unstable and convective conditions are regularly observed in summer. E.g Cohen et al 2007, Box et al 2001 (PhD thesis).

Pg 6: Ln 133: 'all methods are applied on time-averaged quantities and not on daily SMB fields'. What is the output frequency of your runs, and what time-average was used for output?

Figure 2: October 2008- September 2014 is correct? Or should it say October 2007? If you do not include 2007, why?

Figure 3: Please add a label/marker for Sukkertoppen as it is relevant for section 3.2.1. Just on one subfigure is fine.

Figure 4 caption: Write out the full caption, as there is a lot of information on this figure. The white line marking the ice sheet margin is missing.

Pg 11, Ln 228: Also highlight Figure 4f for numerical artefacts.

Pg 12, Ln 246/247: Refer to figure 5a.

Pg 13, Ln 257: 'significantly less variability'- have you tested the difference in variability with statistical tests?

Pg 13, Ln 270 onwards: can you include a spatial plot (like Fig 3 or 4) of the snow drift differences, as it would be easier to interpret line 270 to 280, especially the 'very different patterns' (Ln 275).

Figure 7 caption: 'all data in this Figure is' should say 'all data in this Figure are'. Add information about the dashed lines/REF in the caption.

Pg 17, Ln 320: in reference to figures, include that the * markers are what the reader should be looking at here. Similarly, what on the Taylor diagram is showing that the 60km resolution is 'insufficient'.

Pg 18, Ln 357: Remove ‘These waves may or may not be realistic’ here as you say it later.

Pg 19, Ln 362: remove ‘as already mentioned’ if you change the above sentence.

Pg 19, Ln 376: Why do you exclude the first year?

Pg 19, Ln 381: Why do you exclude the first 4 years?

Pg 21, Ln 406: Where do these soot content values come from? Literature or observations perhaps?

Section 3.5.5 is missing- or the subtitle is in the wrong place. It would make sense that this should go before Line 439, starting with ‘Figure 10 shows. . .’

Pg 22, Ln 461: Whilst I agree that a smaller fraction of Greenland is being improved, aren’t these some of the most important regions in terms of the SMB, such as the coast and low-elevation zones?

Pg 24, Ln 481: treating is spelt wrong.

References: Hines, K. M., & Bromwich, D. H. (2008). Development and Testing of Polar Weather Research and Forecasting (WRF) Model. Part I: Greenland Ice Sheet Meteorology*. MWR, 136(6), 1971–1989. https://doi.org/10.1175/2007MWR2112.1

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Niwano, et al (2018) NHM–SMAP: spatially and temporally high-resolution nonhydrostatic atmospheric model coupled with detailed snow process model for Greenland Ice Sheet, The Cryosphere, DOI: 10.5194/tc-12-635-2018

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