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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 #1

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Remarks to the Authors

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

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General comments:

In this paper, the authors investigate and discuss dependency of the Greenland ice sheet (GrIS) surface mass balance (SMB) estimates by the polar regional climate

model RACMO2 on the choice of horizontal resolution set in the model (60, 20, 6.6, and 2.2 km). They highlight that setting a high horizontal resolution in the model is as important as making realistic initialization of firn physical conditions, simulating realistic ice albedo, estimating the accurate surface turbulent heat flux, and preventing positional displacement of precipitation systems in the model. This paper is very detailed, informative, and constructed well, so, this reviewer could enjoy reading it. However, I had a fundamental question about the authors' view on "the limitation of the hydrostatic assumption". I think the horizontal resolution of 6.6 km would be the maximum permissible limit for a hydrostatic atmospheric model like RACMO2, although I would set more than 10 km if I do the same things. In general, precipitating convective systems (its horizontal scale is not much wider than its vertical scale in general), which should be tried to resolve explicitly if an atmospheric model's horizontal resolution becomes less than about 10 km, cannot be simulated realistically by a hydrostatic atmospheric model due to "the limitation of the hydrostatic assumption". The horizontal resolution of 2.2 km for a hydrostatic atmospheric model is obviously out of the application range. Therefore, I thought presenting/discussing results from the 2.2 km run is not a good idea. An important but indistinct point of this study is whether the authors use a convective parameterization scheme in RACMO2 or not. Only if the authors use such a scheme and they have tuned it for high-resolution (less than 10 km) simulations around the GrIS well, the 2.2 km run would be worth trying. In the following part, this reviewer gives specific comments. Please note that page and line numbers are denoted by "P" and "L", respectively.

Specific comments (major)

P. 3, L. 45 $\sim$ 48: In recent years, several attempts to develop non-hydrostatic regional models that can calculate temporal evolution of the GrIS SMB have been made (Mottram et al., 2017; Niwano et al., 2018). Please consider indicating this point here.

Sect. 2.1: Do the authors use a convective parameterization scheme in RACMO2? If the authors use such a scheme in the model, do they use the same scheme/setting for all the simulations introduced here? This is an important point of this study, so please explain it here.

Sections 2.1 and 2.2: The authors recognize the limitation of the hydrostatic assumption (P. 3, L. 44); however, they apply a hydrostatic model (RACMO2) at very high resolutions of 6.6 and 2.2 km. The authors' intention here is not clear.

P. 8, L. 188 $\sim$ 189: Ettema et al. (2009) showed that precipitation rates over the GrIS can be increased for higher-resolution simulations with hydrostatic atmospheric models. However, in this study, the precipitation rate from the 2.2 km RACMO2 run is smaller than that from the 6.6 km RACMO2 run (Table 2). What is the main reason of this result? Please discuss.

P. 17, L. 332 $\sim$ 334: The result described here is interesting: more precipitation occurs over not GrIS but tundra in the 2.2 km run than the 6.6 km run (I feel the result indicated here is plausible). Could the authors discuss more about possible reasons for this (what happens in the model?) here? It would be informative for readers.

P. 17, L. 335 $\sim$ 336: Why does the not-small difference in precipitation rates from the 2.2 and 6.6 km runs (25 $\sim$ 100 mm w.e. a-1) occur also over sea? Is it related to activities of cyclones and/or frontal systems in the area? Please discuss.

P. 19, L. 366 $\sim$ 368: The authors' logic here is comprehensible basically; however, how can we understand the argument that RACMO2 can be applied at resolutions down to about 5 km? In general, precipitating convective systems (its horizontal scale is not much wider than its vertical scale in general), which should be resolved realistically if an atmospheric model's horizontal resolution becomes less than about 10 km, cannot be simulated/reproduced realistically by a hydrostatic atmospheric model: I understand it is a specific example of the limitation of the hydrostatic assumption. Is it OK to understand precipitations over Greenland are caused mainly by non-convective systems? I

think maybe no. In this regard, I assume the choice/setting of a convective parameterization scheme in RACMO2 plays a key role of the result (please also see my major comment at Sect. 2.1 indicated above).

Specific comments (minor)

P. 3, L. 52 $\sim$ 65: This reviewer understands that the RACMO2 simulation at the horizontal resolution of 60 km is assumed to be an ESM-equivalent run in this study. However, is it a reasonable assumption? Because RACMO2 is forced by the ECMWF Operational Analyses (P. 5, L. 117), its performance might be better than ESM simulations in general. Usually, ESM simulations do not refer to such spatially and temporally detailed and realistic information.

P. 4, L. 76: In RACMO2, does meltwater runoff occur only at the bottom of snowpack? If an ice layer formation is simulated in the internal snowpack, what happens in the model?

P. 5, L. 102: Is there any references for the empirical formulas (also constants listed in Table 1: Ek, kc, and ks)? If no, please describe more in detail about the basic/background science of the equations.

P. 6, L. 119 $\sim$ 120: It is better to indicate reasons why the 2.2 km RACMO2 run can be conducted without an intermediate RACMO2 simulation (double-nesting).

P. 6, L. 127: Please explain why "0.42" is chosen and set for the ice albedo in the model.

P. 11, L. 229 $\sim$ 231: At this point, the authors have not compared model simulation results with measurement data. Therefore, I think they should not use the words like "improve" and "underestimate" here.

P. 14, L. 283: This is a good explanation of the analysis conducted in Sect. 3.2.3.

Related to the above comment (P. 11, L. 229 $\sim$ 231), it is better to indicate at the beginning of Sect. 3.2.3 that the discussion in the subsection 3.2.3 is an “expectation” of the effects of the refining techniques.

P. 21, L. 406: Maybe, the soot concentration value of 0.10 ppmv is set by default in RACMO2 right? If so, what is the basis of the value?

Technical corrections

P. 1, L. 8: It would a bit difficult to understand the exact meaning of “resembled”. Can the authors replace “resembled” with another word?

References

Ettema, J., van den Broeke, M. R., van Meijgaard, E., van de Berg, W. J., Bamber, J. L., Box, J. E., and Bales, R. C.: Higher surface mass balance of the Greenland ice sheet revealed by highâĂŘresolution climate modeling, Geophys. Res. Lett., 36, L12501, https://doi.org/10.1029/2009GL038110, 2009.

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Niwano, M., Aoki, T., Hashimoto, A., Matoba, S., Yamaguchi, S., Tanikawa, T., Fujita, K., Tsushima, A., Iizuka, Y., Shimada, R., and Hori, M.: NHM–SMAP: spatially and temporally high-resolution nonhydrostatic atmospheric model coupled with detailed snow process model for Greenland Ice Sheet, The Cryosphere, 12, 635–655, https://doi.org/10.5194/tc-12-635-2018, 2018.

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