Reply to Leo van Kampenhout

We sincerely appreciate Leo van Kampenhout for taking the time to review our paper. Below we describe our responses (in blue text) point-by-point to each comment (in black text). In addition, we indicate revisions in the updated manuscript together with new line numbers. Please also refer to the revised marked-up manuscript uploaded in the discussion board.

I agree with Xavier Fettweis that this work would be a welcome addition to the literature and the wider RCM modelling community. Some questions came up while reading the manuscript, in particular about the spinup method and the effect of percolation.

Thank you for the comment. We agree with the reviewer's point that we should detail more about the model spin-up and the effect of percolation. Please check our answers below.

L 238-240: I searched Dumont et al. (2014) for their spin-up procedure, but failed to find information on this. Did the authors obtain the method details through personal communication?

I am sorry the original description was old and incorrect. We have revised the sentence as follows:
"The initial snow/firn/ice physical conditions for the entire GrIS on 1 September 2011 were prepared by performing a 30year spin-up of the NHM-SMAP model following the procedure of Dumont et al. (2014)."
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"The initial top 30 m snow/firn/ice physical conditions for the entire GrIS on 1 September 2011 were prepared by performing a 30year spin-up of the NHM-SMAP model." (P. 7, L. 260-261)

L 238-240: I was wondering whether 30 years is sufficient to get a 30-m snowpack into equilibrium with the climate. Was there any remaining drift in e.g. the bottom layer temperature? What climate years were used to forced the spinup?

First, before starting model spin-up, we attempted prepare realistic initial profiles for snow/firn/ice physical conditions in the GrIS as much as possible. Thanks to this, we did not encounter any drift at the beginning of model spin-up. The performed procedure to prepare the initial conditions before the model spin-up has been described in the revised manuscript as follows:
"Before performing the model spin-up, the initial profiles for snow/firn/ice physical conditions in the GrIS were given following the procedure presented by Lefebre et al. (2005) and

properties for snow/firn microstructure (e.g., optically equivalent grain size and grain shape) were given from the firn core analysis at SIGMA-A (Yamaguchi et al., 2014) equally in the GrIS." (P. 7, L. 261 – P. 8, L. 265)

As for climate years used to force the model spin-up, we used the data during the 2010-2011 mass balance year. First, we performed JMA-NHM stand-alone simulations forced by JRA-55 during the period. Then, the simulation results for surface atmospheric conditions forced SMAP 30 times cyclically (off-line calculation). Of course, it is ideal to perform continuous (not cyclic) 30year spin-up; however, it was not realistic due to computational costs. In the revised manuscript, it is described as follows:
"From the state, surface atmospheric conditions from September 2010 to August 2011 simulated by JMA-NHM forced by JRA-55 were used to drive SMAP for 30 times cyclically." (P. 8, L. 265-267)

Reference:

Lefebre, F., Fettweis, X., Gallée, H., Van Ypersele, J.-P., Marbaix, P., Greuell, W., Calanca, P.: Evaluation of a high-resolution regional climate simulation over Greenland, Climate Dynamics, 25, 99, doi:10.1007/s00382-005-0005-8, 2005.

L 242: It reads like you started with zero snow depth at the beginning of the spinup period. The zero heat flux is then assumed at the bottom of the snow pack, not at 30 m, which is almost never reached? (which you mention in 245-246)

We "did not" start with zero snow depth at the beginning of the spin-up period as mentioned above. During the simulation period, the thickness of snow/firn/ice is always constant: 30 m. It is mentioned in the revised manuscript as follows:
"The thickness of snow/firn/ice is always set to constant (30 m) during the calculation. In case snow accumulation or ablation is simulated, the thickness of the bottom model layer is modified accordingly." (P. 7, L. 258 – L. 260)

L484-485: Figure 10 shows that runoff is larger for larger IWC value (6%), so the "piping" effect must be dominated by something else. Otherwise, the 2%-bucket model would have produced the largest runoff value.

We agree with the reviewer's point that 2%-bucket model setting can heat snow/firn effectively, then result in earlier onset of melting, which can produce large runoff. However, in the

sensitivity tests, we did not consider feedbacks that have more than a year time-scale due to our test setting. In the revised manuscript, we have added the following explanations regarding the setting of model sensitivity tests that changed water percolation schemes:
"In the sensitivity tests, profiles for snow/firn/ice physical conditions were reset at the beginning of the 2011–2012, 2012–2013, and 2013–2014 mass balance years by referring to the simulation data from the on-line version of NHM-SMAP. It means that feedbacks, which have more than a year time-scale, are not considered." (P. 15, L. 539-542)

L 497-502: The authors do not supply any proof of their statement that the formation of ice layers is the reason for the increased runoff. In particular, they do not present melt and refreezing as separate terms. After the formation of (sub-surface) ice layers, one expects the melt to stay roughly the same order of magnitude, yet see a drop in refreezing due to the added effect of lateral runoff.

On the other hand, an increase in runoff could also occur due to increased melt. The reasoning is that when you have higher IWC and more refreezing, warmer snow will result which leads to stronger metamorphism and larger grains that lower the albedo. The warm snow also will persist throughout winter and helps to bring snow to the melting point in spring. This behaviour is also seen in other models. It would benefit this paper if light could be shed on the exact processes that are dominant in this study.

Thank you for the insightful comments and suggestion. Following the suggestion, we have included a figure showing melt and refreeze rates, which are monitored in NHM-SMAP operationally. In the revised manuscript, it is discussed as follows:
"To confirm the discussion, the GrIS-area-integrated daily melt and refreeze rates were investigated (Fig. 9). In the figure, results for the 2011-2012 mass balance year are shown, whereas results for other mass balance years are depicted in Fig. S3. During the 2011-2012 mass balance year, simulated daily melt rates were almost the same among the results from Richards equation scheme and two bucket schemes (Fig. 9a); however, refreeze rates from the control Richards equation scheme were much lower compared to other results (Fig. 9b), which is an evidence for the above-mentioned more impermeable ice in the results from Richards equation scheme. The same characteristics could be found in other mass balance years (Fig. S3)." (P. 15, L. 554-561)

What we found are basically the same as the reviewer's recognition.