The Cryosphere
Discussions

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Interactive comment on "Refreezing on the Greenland ice sheet: a comparison of parameterizations" *by* C. H. Reijmer et al.

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Author response

We would like to thank the four referees for their constructive comments. Below are responses to the issues raised by R. Fausto, the anonymous referee, T. Pfeffer and I. Janssens. The revised manuscript is added as supplement.

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Response to comments from R. Fausto

1a. *The coupled snow model incorporated in RACMO2 is not described in the manuscript.*
We incorporated a more detailed description of the snow model in the revised manuscript (section 3.1).

1b. *I would like to have the different plots in each of the figures 7, 8, 9 and 10 on the same scale in order to compare the spatial differences more easily.*
We have changed the color scales of figures 7, 8, 9, 10 and 11.

1c. *Section 4.2 is called sensitivity experiments: changing a parameter value a few times does not explore the full or at least a large part of the parameter space. Maybe the authors could just call the section "Experiments" and adjust throughout the text.*
We have changed the termology where necessary.

2a. *What would be the consequence for meltwater retention in the percolation zone when the redistributed mass in the firn is taken into account in the dry snow densification?*
Reeh et al. (2005); Li et al. (2007); Reeh (2008) separate in their densification models the effect of refrozen water and densification of the dry snow layer. Including such a description would result in an enhanced densification, reducing pore volume space and thus the amount of retention and refreezing in RACMO2. Reeh et al. (2005); Li et al. (2007); Reeh (2008) apply their models on annual snow or complete firn layers over time periods of months to years not taking the effect of temporal variability in the snow temperature and melt on shorter time scales on the densification into account. In our model these are taken into account (see section 3.1).

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2b. *It would be interesting to see the spatial distribution of the meltwater retention from both the coupled snow model (RACMO2) and the parameterizations when the Reeh (2008) function (rho_f as a function of firn temperature) is used. Are there any differences in the comparison between the model parameterizations? Also, there should be a bit more discussion on the altitudinal/temperature/accumulation dependence of rho_f and rho_pc. How is the optimal choice made for these parameters?*

In section 4.2.5 we added an experiment in which the function presented by Reeh (2008) is used to describe rho_f. In addition, we derived a similar function based on RACMO2 results, tested it and discuss the results as well in section 4.2.5.

Additional possible dependency of rho_f as a function of altitude/accumulation are tested using RACMO2 output and discussed in section 4.2.5. Note that rho_pc is per definition a constant value. The values used for rho_f and rho_pc and the range taken in the experiments are taken from literature. A comment is added in section 2.2 and section 4.2.5.

2c. *A bit more discussion on the point of how to "choose" the best d_ice for a model parameterization. For example, would it be possible to make a parameterization of d_ice which depends on air temperature? Altitude?*

Unfortunately, the maximum depth at which refreezing occurs in the snow model was not stored. It is therefore not possible to derive such a function based on available RACMO2 output. A comment is added in section 4.2.3.

Response to comments from the anonymous referee

The weakness of the paper is the lack of scientific significance. What is the contribution to scientific progress here? Janssens and Huybrechts (2000) made the same study and reached the same types of conclusions more than a decade ago. Sensitiv-

*ity experiments have been performed and differences are evaluated. Are there some
clear results which should be conveyed to the scientific community or are the tested
parameterizations oversimplified?*

We obviously do not agree with the statement. Janssens and Huybrechts (2000) limit
their analyses to comparing the presented parameterizations, then focussing on the
sensitivity of the total mass budget to climate change. In the absence of observations
we use the RACMO2 model, which includes a comprehensive snow model, as a ref-
erence in the comparison of the different parameterizations. Besides the reference
refreezing field, RACMO2 provides us with more realistic input fields to the parameter-
izations. We then perform a more detailed analysis of the sensitivity of the parameteri-
zations to their input parameters. We will stress the additional value of this study in the
introduction and conclusions.

Based on the found spatial differences between the parameterizations it is likely that
none of the parameterizations captures the spatial variability in actual refreezing. This
indicates that not all processes are included or described adequately.

Response to comments from T. Pfeffer

*A more detailed description of RACMO2 would have made the whole thing more
understandable, but perhaps this paper isn't the place for it.*

A more detailed description of the atmospheric part of RACMO2 is indeed beyond the
scope of this manuscript and can be found in Ettema et al. (2010). A more detailed
description of the snow model is added in section 3.1.

*The fact that everyone's in agreement in the comparisons described here isn't a
guarantee of validity – everyone might simply be modeling the same hypothetical
situation that never actually occurs.*

We added a comment along these lines in the conclusions.

In particular I am concerned about the absence of any consideration of heterogeneous infiltration (i.e. piping) in the downward transport of water.
We have added comments throughout the text regarding the absence and its implication of piping in the parameterizations and model.

That would make a good conclusion to this paper: it's great that everyone's model is in agreement, but do they agree on the important processes? Where do we go next?
We have added an extra paragraph to the conclusions to discuss possible next steps.

Response to comments from I. Janssens

The good introduction is promising but some experiences and conclusions are a bit disappointing because a large part of this work is descriptive without reaching a clear goal
We have added comments throughout the text in order to make the goal of this research more clear.

The scientific value of this work could be, in the absence of enough observations, to produce a more comprehensive retention model, built in a Regional Atmospheric Climate Model with an Energy Balance Melt Model, serving as reference for simpler parameterizations and opening the way to develop new parameterizations of a higher level, capable to reproduce a good local approximation of the retention in spite of a reduced number of parameters.
Unfortunately, at present the model output does not provide enough information to develop a new parameterization, see our comment above. Especially knowledge of

the depth to which water penetrates (i.e. d_ice) is lacking. We have added a comment along these lines in section 4.2.3 and the conclusions.

I tried to detect how the coupled snow model works. I had to go from one reference to the other. It should be easier for the reader to resume and clarify the key retention mechanisms of the coupled snow model in this paper and to direct the discussion about different parameterizations in this context.
A more detailed description of the snow model is added to section 3.1.

If I understand correctly, refreezing is only possible for the water percolating through the snow or firn, but if it reaches an impermeable layer no refreezing occurs? Nevertheless, for some regions, for instance in the neighbourhood of the run-off line at Central West Greenland, the reference retention is higher than all of the parameterization ones (see Fig. 7). What is it that I don't see? It is correct that in contrast to Greuell et al. (1997); Bougamont et al. (2005); Reijmer and Hock (2008) RACMO2 does not allow a slush layer to form. This is described in more detail in section 3.1. The slush formation was omitted to prevent unrealistic large slush layer formation compared to observations on the western ice margin.
The fact that the model produces considerable amounts of refreezing around the runoff limit is due to multiple cycles of refreezing. The lower amounts in the parameterizations can be explained by the specific formulation of those parameterizations and the fact that they do not explicitly account for multiple cycles of melt and refreezing.

The discussion about Pmax (4.2.7 Pmax) reveals other surprising facts. As pmax = 0.6 corresponds less or more to the maximal value of possible retention (theoretically and observed) we may expect that the total amount of retention should be overestimated by Re1991. Surprisingly not, as the Pmax has been tuned to 0.65. This suggests that the reference retention could be overestimated.

Re1991 does not take refreezing of rain into account and we have therefore not included rain in the tuning process as illustrated in table 4. The resulting higher Pmax compensates for not taking Rain into account. We have added a comment in section 4.2.7 to clarify this and also repeated the tuning process including Rain. The results are presented in table 4 and discussed in section 4.2.7.

It is also remarkable that the ice sheet average of Er/C reaches the high value of 0.28. Unless the "result of multiple cycles of melt and refreezing of the same snow/ice" (page 2742, line 1) plays a dominant role and exceeds the loss of retention caused by omitting formation of superimposed ice. If this is true, it should be interesting to investigate this new scientific issue.
The value of refreezing over snowfall (Er/C) is reasonably high as a result of multiple cycles of melt and refreezing. Given the lack of observations it is difficult to say whether these multiple cycles are overestimated, compensating for the omission of slush and super imposed ice formation. We have added a comment in section 4.2.7. High Er/C indicates heating of the snowpack over multiple years and is indeed an interesting feature. We have stressed this point more in section 4.2.7. The areas with high Er/C are subject of an additional study into indicators of climate change in RACMO2 output.

Figures 1 and 7 are very important but too small for a good analysis.
We have increased their size.

The different scales of figure 1 make it hardly possible to compare the maps. It would be more appropriate to show a map of available water (melt and rain).
We have changed the scales of panels a, b and d. Panel c has a different scale in order to visualize the gradients best.
We have chosen to show melt and rain separately because some of the parameter-

izations do not take rain into account. The figure therefore serves to illustrates the potential additional amount of possible refreezing. To limit the total number of figures we omitted the figure of the sum of rain and melt.

Figures 13: The Pmax fraction is a relative retention potential, while Er/C is the relative effective retention. Don' t use Pmax here. Of course you could speak about the tuned Pmax (= 0.65) producing the same area averaged effective retention (=0.28).
We have rephrased the caption in the revised manuscript.

Page 2735 line 26. I should replace "the difference goes to zero" by "the difference is zero, by definition". If the rain is accounted for, where the water is the limited factor, you have Er = min(Pr,Wr) = Wr = Melt+Rain = Wr_ref = Er_ref . Is it not possible to reserve the yellow color in Fig. 7 for the zero value alone?
We have rephrased the sentence in the revised manuscript. Unfortunately, the plotting programme does not allow for a single color to be chosen for one value. We have reduced the range of it to 0-1 instead of 0-10.

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/5/C2013/2012/tcd-5-C2013-2012-supplement.pdf

Interactive comment on The Cryosphere Discuss., 5, 2723, 2011.