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The Cryosphere Discussions

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

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This paper addresses the very important problem of determining the difference between amount of melt generated at the surface of a glacier or ice sheet (in most cases applied to Greenland) and the amount of water which is actually mobilized and available for runoff or action within the ice sheet hydraulic system. We are dealing with a comparison of meltwater retention parameterizations together with a coupled snow model incorporated into the regional climate model RACMO2. The paper makes a systematical and quantitative comparison of the impact from the meltwater retention parameterizations of the Greenland Ice Sheet and it also shows and discuss the significance of changes in critical model parameters (depending on which model parameterization used) to the overall spatial distribution of modeled water retention.

Overall I think this is a good piece of work, but I have two categories of suggestions for improving the manuscript, each of which is fairly easily addressed. The first relates to clarity of presentation, and the second to a discussion of details which the authors have not included in their model but whose potential influence should be discussed at least qualitatively. For the second of these, I'm not suggesting more modeling work (it might take a lot), but the authors should acknowledge the existence of the issues I'll mention and offer some discussion of how these issues might influence the situation.

1) Clarity of presentation: The paper deals with a comparison between different retention models.

a) The coupled snow model incorporated in RACMO2 is not described in the manuscript. Since it is compared to all the other parameterizations, I think, it would be essential to have a description of this sub model in order to clarify the differences for the reader without turning to another paper.

b) The figures are in general of good quality, but 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.

c) 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.

2) Unaddressed issues: There are a few details of the dry snow densification and retention model formulation and the discussion of the results from the parameterizations that trouble me a bit. I am not suggesting that the authors revise their model and start over, but they might consider these details and comment of their potential influence in the text:

a) Empirical densification models of the Herron and Langway type is based on the idea that the proportional change in air space is linearly related to the change in stress due

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to weight of the overlying mass. A redistribution of mass and energy happens when meltwater is introduced into the snowpack, which will in turn influence the melt, percolation, refreezing, densification of dry snow, etc. I find the energy (Equation 2 in Reijmer and Hock (2008)) and mass movement (Equation 3-7 in Reijmer and Hock (2008)) descriptions and their consequences well described in Reijmer and Hock (2008), but I cannot find the consequence of the redistributed mass on the dry snow densification (Equation 12 in Reijmer and Hock (2008)). For example, when you redistribute water by percolation from the top layer into the lower layers, the dry snow densification will change for the whole firn column due to the change in the mass distribution. Reeh and others (2005), Reeh (2008) and Li and others (2007) include this in a simple and straight forward way (limited by melting at the surface in 1 year is assumed to refreeze in the corresponding annual layer) in their densification models. As I read it based on Reijmer and Hock (2008), this is not accounted for in RACMO2. For example, the extra term in Equation 8 from Reeh (2008) could easily be included in the model by Li and Zwally (2004) (Equation 12 in Reijmer and Hock (2008)). 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?

b) The density of the surface layer rho_f is kept constant in the coupled snow model incorporated in RACMO2 and in the meltwater retention parameterizations as well. Reeh and others (2005) offers a distributed rho_f as a function of the mean annual firn temperature, which is based on observations. It would be interesting to see the spatial distribution of the meltwater retention from both the coupled snow model (RACMO2) and the parameterizations when this function 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?

c) The size of the thermally active layer d_ice is dependent on altitude. In the ablation zone d_ice would be close to C (accumulation), whereas above the ELA d_ice would

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be much larger. Using a constant d_ice for the whole ice sheet would introduce larger errors on E_r in the parameterizations. 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?

References:

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2. Reeh, N., D.A. Fisher, R.M. Koerner and H.B. Clausen, 2005. An empirical firn-densification model comprising ice lenses, Ann. Glaciol., 42, 101-106.

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