

## ***Interactive comment on “The modelled liquid water balance of the Greenland Ice Sheet” by Christian R. Steger et al.***

### **Anonymous Referee #1**

Received and published: 4 July 2017

Review Steger et al. (2017). “The modelled liquid water balance of the Greenland ice sheet”. The Cryosphere Discussion.

The manuscript addresses the important problem of determining the different contributors to the liquid water balance applied to Greenland. The study highlights the amount of water in the near-surface, using the subsurface scheme from SNOWPACK, which is actually mobilized and available for runoff. SNOWPACK is forced with the regional climate model RACMO2.3, while the manuscript makes a systematically and quantitative comparison of the impact of the different components in the liquid water balance on the Greenland Ice Sheet. The study shows and discusses the significance of changes in some of the critical model parameters to the overall spatial distribution of modelled water retention. However, it fails to sufficiently discuss the implication of the improved

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water balance on the surface mass balance.

Overall, this is a decent piece of work, but the manuscript has room for some substantial improvements.

Major points:

1. Highlight differences between Steger et al. (2017) and this study. There seems to be a lot of overlap. Fx fig 4 in Steger et al. (2017) looks almost identical to fig. 5 (e) of this study. It should also be specified if the SNOWPACK model versions and simulations are identical. Also, the firn aquifer description and discussion is very similar.
2. It is stated on p. 6, line 10-11: “At higher elevations in western Greenland SNOWPACK does simulate a pronounced warming of the firn but there are no in-situ observations available to constrain the magnitude of these changes.” The authors should have a look in the extensive GC-net archive of in-situ subsurface temperatures to validate simulated temperatures.
3. Compare simulated refreezing with available firn cores in the literature. However, I believe, that this was done to some extent in Steger et al. (2017)? Please highlight the main outcome of this analysis. How good is the model performing?
4. Define “skin temperature”.
5. Quantify statements whenever it is possible throughout the manuscript. For instance statements like “...good model performance...”, “...increase in surface melt...”, “...indicate positive trends...” or “...temperature increases are highest...” in the Abstract should be quantified. Please have a look at the other sections in the manuscript to quantify similar statements. Please have a look at the Conclusions.
6. The chosen spin up period seems to highly influence crucial subsurface parameters like density (fig 10). This will influence the interpretation of LWB results. Maybe use a different spin up method. Fx the year of 1960 could be used or a mean of the used period (1960-79).

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7. Several periods are used for comparing the different components in the liquid water balance. Please highlight/argue why all these periods are used.

8. You should state when the results are presented and discussed.

9. Since you state in the introduction that a better LWB contributes to a better estimate of surface mass balance, how did your experiment modify the surface mass balance? It would be an important point, which is not addressed/discussed sufficiently. Fx Paragraph 4.4 : The description is useful to understand figure 14 but it misses explanation on why those changes in snow/firn/ice melt are relevant for the SMB.

10. In the comparison with grace what is due to the new liquid water balance? How is the liquid water balance influencing the surface mass balance?

11. Basin scale GRACE comparisons with surface mass balance could improve our understanding.

Figures.

Fig. 1: Nice illustrative figure.

Fig. 2: Nice illustrative figure.

Fig. 3: It does not make sense to state a correlation value in Fig 3 unless the time series have undergone a high pass filter, which allow the analysis of the variability of shorter time scales when compared to annual cycle. I.e. the annual cycle should be removed as it will dominate the correlation. Here I would also recommend giving a seasonal R-squared then the mean of that and its standard deviation, which would give a good overview on the mean performance and on its variability.

Fig 4: please add RACMO2.3 surface accum. (1994-2013) to illustrate if changes in accumulation is responsible any temperature changes.

Fig 5: I wonder if the quantities of figs. 5d-f are influenced by the spin up method. Also, the title of (e) and (f) should not be "refreezing" because, I suspect, that the figures

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show values of both refrozen and liquid water being retained in the firn. Fx basin 4 is where the perennial aquifers exists seems to be a lot of refreezing there.

Fig 6: Same concern with refreezing vs retention as in Fig. 5.

Fig 7: Same concern with refreezing vs retention as in Figs. 5 and 6.

Fig 8: Please explain in the text, why do you show differences between the periods of 1960-1989 (30 years) and 1990-2014 (25 years). These results could also be influenced by the spin up period.

Fig 9: This is a nice plot, as (a) shows where the firn in 2012 lost its capacity to retain water compared to the reference period. However, I suspect it should be a retention anomaly.

Fig 10: Again, this figure clearly shows the influence of the spin up period. This evident on the western side of the ice sheet with three highly identical subsurface features in the density.

Fig 11: Again, spin up problems?

Fig 12: Nice plot!

Fig 13: Nice plot!

Fig 14: Please explain the implication of changes in the liquid water balance on the modelled runoff pattern in more detail.

Tab 1: I would like to see all trends even if they are not significant.

Specific points:

Line 27 p.3: Which bucket scheme? Please more details and references.

Line 1 on p. 4: It should be mentioned if the two model setups are identical. If not, the differences should be highlighted.

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Lines 9-15 on p. 4: More detail is needed for this description of observational data and what is it being used for? Also, many dataset are available (remote sensing, station data, historical and contemporary SMB measurements. . .) for further validation of the forcing data and of the model output. This part could be improved, which would make the conclusions more solid.

Line 6 on p. 5: How is the tundra hydrology dealt with?

Line 24 on p. 5: It is not only the subsurface temperatures that may be bias but also the density profile.

Line 4 to 7 p. 6: Here and later at KAN-U you mention the overestimation of bare ice zone. A quantification of the spatial extend of this bias would be useful (comparison with remotely sensed bare ice areas?). It would go hand in hand with the many observed SMB available in western Greenland (K transect EGIG line): how does the model compare to them.

Line 6 on p. 6: Who are "they"?

Lines 10-12 on p. 6: Fx, please have a look at GC-Net data.

Line 13 on p. 6: Near-surface snow density depends mostly on wind and subsurface vapor fluxes.

Line 22 on p. 6: Compaction here is mostly due to wind and vapor fluxes. Line 29 on p. 6: Please quantify this inaccuracy?

Line 19 on p. 7: Describe the results from Table 1 in more detail.

Lines 12-16 on p. 8: Should be assessed using observations

Lines 21-25 on p.8: Please explain in more detail the asymmetrical retention pattern and the consequences of this.

Line 10 on p. 9: Again, GC-Net firn temperatures can be used here.

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Lines 6-7 on p. 10: How good is this threshold? Where does it come from? Please more support for this is needed.

References:

Steger CR, Reijmer CH, van den Broeke MR, Wever N, Forster RR, Koenig LS, Kuipers Munneke P, Lehning M, Lhermitte S, Ligtenberg SRM, Miège C and Noël BPY (2017). Firn Meltwater Retention on the Greenland Ice Sheet: A Model Comparison. *Front. Earth Sci.* 5:3. doi: 10.3389/feart.2017.00003

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Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2017-88>, 2017.

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