Review of "Regional grid refinement in an earth system model: impacts on the simulated Greenland surface mass balance"
by L. van Kampenhout et al.

Summary: The authors discuss the impact of spatial resolution on simulated Greenland ice sheet surface mass balance in a variable resolution version of the community earth system model (CESM). They find that enhancing the spatial resolution of the model over the Greenland ice sheet improved the spatial distribution of accumulation and reduced a positive bias relative to airborne remote sensing accumulation measurements at high elevations. The increased spatial resolution did not have much impact on patterns of atmospheric circulation, but led to decreased runoff along the east coast of Greenland. The authors attribute these changes to decrease in cloud water path, which led to changes in surface energy balance components, notably reduced longwave radiation. The authors also discuss potential reasons for an apparent positive bias in snow cover over tundra in northern Greenland in CESM which worsens with increasing spatial resolution.

General Comments
The paper is well-written, and the work is relevant, timely, and worthy of publication in *The Cryosphere*. I also feel that the authors have adequately addressed the concerns of the previous reviewers of the manuscript. I do have a few minor comments about the authors' interpretation regarding the role of clouds, and about the organization of sections at the end of the manuscript:

1. I feel the authors may be overemphasizing the role of clouds in the changes in runoff that occur along the Greenland east coast. The fairly large changes in longwave radiation that occur with increasing resolution extend far inland in southeast Greenland, while the net surface radiation anomalies are confined to the coast. Perhaps the longwave anomalies are balanced by similar downwelling shortwave anomalies. The spatial patterns of net surface radiation change actually seem to be better correlated with maps of surface albedo differences. I suggest the authors look at all components of the energy balance (e.g. for Figure 9) to see what the largest changes are and what might be contributing the most to the observed differences between simulations. (Less interesting figures could be included in the supplemental section.)
2. Surface albedo is discussed only briefly in the manuscript even though it is an important control on the surface energy balance. I suggest the authors consider the possibility of surface albedo changes while discussing some of the results.
3. The section labeled "discussion" seems a bit out of place, since it is mostly discussing the anomalies in snow cover over North Greenland tundra, and these differences are less relevant to the overall Greenland ice sheet mass balance, which is the focus of the rest of the paper. I suggest shortening this section, renaming it to reflect the snow accumulation issue, and including it as a subsection of Section 3. Perhaps Section 3 can also be renamed "results and discussion".

Specific Comments

1. P. 1, Line 5: Suggest mentioning precipitation or snowfall here to make clear what "wetting" and "drying" refer to.
2. P. 2, Lines 6-16: The following recently published paper might also be interesting for the authors and could be mentioned here:
   Alexander, P. M., Legrande, A. N., Fischer, E., Tedesco, M., Fettweis, X., Kelley, M., Nowicki, S. M. J., and Schmidt, G. A. (2019) Simulated Greenland surface mass balance in the GISS ModelE2 GCM: Role of the ice sheet surface. *Journal of Geophysical Research: Earth Surface*, 124. https://doi.org/10.1029/2018JF004772.
3. P. 5, Line 12: Does the model include multiple layers? Briefly mention this here.
4. P. 5, Line 16 to P. 6, Lines 26-28: This part is a bit confusing. Initially it seems that the authors are saying that snow height was reset everywhere to 100 mm w.e. Perhaps revise to read something like: "Below 1774 m in elevation (which is roughly the present-day GrIS equilibrium line altitude), the initial snow amount was set to a minimum value of 100 mm w.e. to avoid snow cover hysteresis resulting from errors in the interpolated initial conditions." Also, if possible, please include a reference for the ELA being at 1774 m.
5. P. 9, Line 11: Where is the location of increased cyclonic flow?
6. P. 13, Line 3, Section 3.5: It could be mentioned here that elevation classes are used in all simulations, and therefore there is already "downscaling" occurring with respect to the surface model, which helps to explain why changing the spatial resolution does not substantially impact the comparison with ablation sites.
7. P. 13, Line 10: Suggest adding "for all model versions" after "right locations" for clarity.
8. P. 14 Lines 2-3: Could changes in surface albedo due also produce these changes in runoff? Does albedo vary across elevation classes? If not, a lower resolution could result in a lower coastal grid box elevation, increasing chances of bare ice exposure and lowering surface albedo (see Alexander et al., 2019). Perhaps the authors should mention this possibility, although the fact that changes are seen primarily along the east coast rather than the west coast suggests that albedo may not be so important.
9. P. 14, Line 17: This is a bit unclear. What is the melt underestimation a consequence of?
10. P. 15, Line 6: Is it being implied that lower snow temperatures are resulting in increased refreezing despite lower melt? Please clarify. Also suggest adding "higher" within the parentheses: '(higher "cold content")'.
11. P. 16, Line 13: What about surface conditions (e.g. surface albedo) or surface elevation differences for different resolution simulations? I suppose with the elevation class scheme active across all simulations, surface conditions may play a less important role in the differences, but the authors should mention this if this is the case.
12. P. 21, Lines 16-17: Indicate where in the ablation zone the biases developed; revise "too little ablation" to "too little ablation with increased resolution".

Technical Corrections

1. P. 4, Line 18: Add “were” before “scaled with horizontal resolution”
2. P. 6, Line 3: Change “off Figure 6” to “on Figure 6”
3. P. 6, Line 6: “SMB is the main focus…” This sentence seems out of place. Perhaps remove or move it?
4. P. 7, Line 33: Change “seasonal estimates” to “seasonal accumulation estimates” for clarity.
5. P. 8, Line 28: Should this read “one of the minima” instead of “one of the maxima”?
6. Figure 3: If possible include “anomaly” on the colorbar labels, e.g. “Z500 anomaly”.
7. P. 8, Line 33-P. 9, Line 1: There does not appear to be any hatching in Fig. 3a-c. I think the authors can simply mention the hatching with regard to Figure 3f, and note that none of the other cases show any statistically significant difference. This can also be done for the caption of Figure 3.
8. P. 9, Lines 5-7: Define “JJA” and “MAM”.
9. P. 9, Lines 9-10: There doesn’t appear to be any hatching in Figures 3d and e. Revise to note only VR-CESM28 in Fig. 3f.
10. P. 10, Line 8: Remove “so” before “dominant”.
11. P. 11, Line 7: suggest revising “the IceBridge radar data support” to “the comparison with IceBridge radar data supports”.
12. P. 11, Line 17: Clarify that the r^2 value is for the spatial correlation.
13. P. 11, Line 18: Revise to “regional model RACMO” or remove “regional”
14. P. 11, Lines 14-19: Change units to mm w.e. yr^{-1} for consistency here and throughout the manuscript.
15. P. 14, Line 5: Suggest adding “In this section…” before the start of the sentence for clarity.
16. Figure 6 caption: Mention that the shading shows the distribution of the differences.
17. Table 1: Again, change units to mm w.e. yr^{-1}
18. P. 15, Line 19: Change “makes that” to “means that”.
19. P. 16, Line 10: Add “in response to changes in resolution” after “summer” for clarity.
20. P. 17, Line 16: Suggest revising “negative longwave radiation” to “negative downwelling longwave radiation” for clarity.
21. P. 19, Line 23: I think “permanently increasing” should read “permanently decreasing”.