Review of the Manuscript
"Regional Grid Refinement in an Earth System Model: Impacts on the Simulated Greenland Surface Mass Balance"
by Kampenhout et al.

In their paper, Kampenhout and collogues investigate the impact of regional grid refinement on the Greenland surface mass balance. They compare three CESM simulations with regionally refined grids to a CESM simulation with a uniform grid. Further, they evaluate the model performance using remote sensing data, in-situ measurements as well as regional model simulations. They find, that a grid refinement improves accumulation patterns, however biases develop in the ablation zones. In order to explain differences in the ablation between the simulations they investigate differences in the atmospheric large-scale circulation and clouds. The manuscript is very interesting and well written and discusses a very relevant topic, which after major revision can provide a step forward in understanding resolution dependency of the surface mass balance.

Major Comments:
Results and Limitations section (P.8-13 and 18):
It should be appreciated that the error in the land cover input file is reported in the manuscript. Given the relatively short simulations in combination with the acceptable performance and throughput per day I was wondering why the authors did not consider to rerun the simulations. Although the error might not change the overall conclusions, a lot of absolute values are presented throughout the results section which are very likely to become different if the error was fixed.

Results and Discussions:
I am very skeptical towards not using the same ice sheet mask for a direct comparison between RACMO and the CESM simulations. The ice sheet extent has a significant influence on the Greenland integrated mass balance and specifically melt. Hence, I do not believe that a "fair" comparison can be made without using the same mask. As the authors focus on the low bias of melt in the VR-CESM simulations the authors should reconsider using an identical mask for the comparison; there is a good chance that the bias is a result of the smaller CESM ice mask. The mass conservation argument, as stated in their response to the editors comments, does not hold in my opinion. If the authors decide not to change the overall analysis in the result section, such an analysis should be included somewhere in the manuscript in order to estimate the effects of the mask on the integrated values.

Minor Comments:
Abstract: "The SMB in the accumulation zone is significantly improved compared …" – It should be added that the refinement leads to such improvement.

Introduction/Model description: It is not clear to me which version of CESM is used. This should be pointed out more clearly (CESM2 or CESM1) from the beginning.

Page 4, Line 8: Are these chosen constants a result of model tuning or used in other studies? Please add references. See also comment on the lapse rate below.

Table 1: The values for RACMO should be included for reference, as it would increase the readability and a direct comparison.

Page 12, Line 29: Please hypothesize which counteractive effects are acting.

Page 14, Line 12: ". ." – remove one .

Page 14, Line 23: Please state that a 0.25º mesh is chosen here.

Page 17, Section "Latent Heat": Regionally there are large differences and the response seems to be very dependent on the topography. The authors should be a little more detailed in their analysis.

Page 17, Section "Subgrid downscaling": Could this be tested by an additional simulation in which the lapse rate during the downscaling is slightly changed? Or can the authors think of another method to test this? See also the lapse rate comment above.

Page 18, Line 23: "&" to "and"

Page 14, Line 32: Have you investigated the blocking frequencies in the different simulations? Or is this a hypothesis?