Summary of major changes

Figure 1 and 2. The *m* and *r* have been added to each panel, in addition RACMO2.3 and CESM1.0 in text are annotated to the first figure in colors corresponding to their scatter and lines. Y-axis is changed to show the same range for the same units.
Figure 2. Added an additional panel containing snowfall.
Figure 3. Added an additional row with SMB vs elevation for each of the lapse rates.
Figure 4. Added a spatial map of the RACMO2.3 reference data. Changed panel (b) to show EC-1K minus EC-6K.
Supplementary figure. A supplementary figure similar to Figure 1 has been added showing the incoming and outgoing solar and longwave components.

Reviewers comments in black
Author's response in red

Reviewer #2:

Summary: The authors present an evaluation of an elevation class scheme applied in the Community Earth System Model 1.0 (CESM1.0). The elevation class scheme allows CESM to simulate surface processes at a sub-grid scale, and allows for interaction between the surface and the atmosphere on the CESM gird scale after surface fluxes are integrated on the CESM grid. The authors mainly focus on comparing gradients of energy and mass balance components at the sub-grid scale with gradients from the RACMO2.3 RCM, a leading RCM used to simulate surface mass balance (SMB) over the Greenland ice sheet. CESM captures gradients of SMB effectively as compared with RACMO2.3 but SMB and surface energy balance (SEB) components are not captured as effectively. Biases in these components tend to compensate for each other, resulting in the effective simulation of SMB gradients. The authors also find that implementing the elevation class scheme influences the simulation of regional climate around Greenland in CESM.

General Comments
I feel that this paper represents an important contribution to our understanding of simulating ice sheet surface mass balance in global climate models. Implementing such simulation is essential for capturing SMB-climate feedbacks in future climate projections with earth system models, which the authors also briefly address in the study. The paper is well argued and the analysis and interpretation of results is straightforward and logical. Sometimes the text becomes a bit wordy and difficult to understand; some suggestions are provided below. I feel the paper can be excepted with relatively minor revisions. Some general comments:

Thank you for your positive feedback, we have addressed individual comments below.

1. There is a recent study by Alexander et al. (2019) that builds on work of Fischer et al. (2014), which evaluates the impact of elevation classes on simulation of Greenland SMB in the NASA GISS ModelE GCM. This study was similar in comparing GCM outputs to an RCM, but differs in that the EC simulation was not evaluated at a high resolution as is done here. These studies should be mentioned and the authors may be interested in exploring similarities or differences in the conclusions at the ESM grid scale.

Alexander, P., LeGrande, A. N., Fischer, E., Tedesco, M., Fettweis, X., Kelley, M., Nowicki, S. M. J., and Schmidt, G. A.: Simulated Greenland ice sheet surface mass balance in the GISS ModelE2 GCM: Role of the ice sheet surface, Journal of Geophysical Research, 124, 750-765, https://doi.org/10.1029/2018JF004772, 2019.

Fischer, R., Nowicki, S., Kelley, M., and Schmidt, G. A.: A system of conservative regridding for ice-atmosphere coupling in a General Circulation Model (GCM), Geoscientific Model Development, 7, 883-907, https://doi.org/10.5194/gmd-7-883-2014, 2014.

We include references to these papers in the revised manuscript.

2. The authors discuss some gradients for which scatter plots are not included. It would be helpful if the authors could provide additional figures for e.g. downward and upward shortwave and longwave radiation, snow accumulation and refreezing. These figures could be included as additional panels in Figures 1 and 2 or supplemental figures at the authors' discretion.

We agree that these figures would provide the readers with additional insight, and will add them to the supplementary material.

3. It is not clear in the text that the SEB terms are computed for JJA, while SMB terms are computed annually. The authors should make this difference clear in the methods and results sections.

In the model, both the SEB and SMB terms are computed every 30 minutes. However, our analysis focuses on JJA for SEB terms (as melt on the ice sheet is generated in summer) and annual SMB. As we see this is a point of confusion, we will clarify in the methods and results section.

Specific Comments

1. Figure 1: Though not essential, it would be helpful if the authors add text or a legend on one of the figures to show that black is RACMO2.3 and blue is CESM1.0. Also it would be helpful if the authors specify the sign convention (+ down) in the legend and text. Mention y-axis scale

differences in the legend. Also, what is meant by “several summer SEB components”. Is only a subset of years used to calculate the gradients? If so this should be made clear in the text.

All SEB components plotted are the multi-year means from the full simulation (1965-2005). Several refers to showing more than one SEB component. Rather than mentioning the y-axis scale, we will update the figures to use the same y-axis where quantities are of the same unit. We will also include annotated text on the plot to show which is RACMO and which is CESM.

2. Figure 2: Again, include a legend on the figure if possible. Mention difference in y-axis scales in the legend.

3. Table 2: Are the standard deviation values annual values? Please specify.

Yes, the standard deviation are annual values, except for the prognostic temperatures where the standard deviations are JJA and DJF.

4. P. 1, Line 5: Change “from RACMO2.3” to “from the RACMO2.3 regional climate model.” The model has not been introduced yet.

We will change accordingly.

5. P. 1, Lines 11-12: The topographic smoothing affects the atmospheric simulation, while the elevation class technique cools the surface by another means. It seems the technique doesn’t really “correct” the bias, but rather “compensates” for it by correcting a bias associated with the coarse ESM resolution. Is this the case? Please clarify here.

Yes, compensates is a more appropriate word. This will be changed in the revised manuscript.

6. P. 2, Lines 24-25: Here the authors might mention that a benefit of the “online” approach is that it is able to capture feedbacks between the downscaled surface simulation and the atmospheric component of the ESM.

Indeed, we will highlight this more.

7. P. 2, Lines 26-30: As noted above, an elevation class scheme has also been implemented in the NASA GISS ModelE GCM in an “online” manner as discussed by Fischer et al. (2014) and evaluated on the ModelE grid by Alexander et al. (2019). However, I believe the authors are correct that the effects of downscaling on the finer resolution grid representation of SMB and SEB has not been evaluated in detail. These studies should be mentioned and the authors should make clear the distinction between evaluation on the coarse resolution model grid, and at the finer scale.

This is clearly relevant literature that should be reviewed in this section, so we will add references to the articles.

8. P. 3, Line 19: I believe the authors are referring to downscaling using elevation classes, but this is not entirely clear. Perhaps the sentence can be revised to read something like “A static ice sheet surface that corresponds to present-day observations (Bamber et al., 2013) is used to downscale SMB and other quantities through the elevation class scheme.”

This sentence will be added.

9. P. 3, Line 21 – P. 4, Line 21: This section is a bit confusing. The steps in the elevation class scheme are not entirely clear. I believe the steps are as follows: 1. A set of elevation classes is defined and for each grid cell containing ice. 2. Some atmospheric quantities are downscaled by elevation. 2. The surface model is run for each elevation class, forced with the downscaled quantities. 3. Surface model outputs are averaged to the ESM grid, weighting for the percentage of each elevation class within each ESM grid cell, and these integrated quantities feed back to the atmosphere. Perhaps these steps can be clarified in the paragraph on P. 3, lines 21-24, and this will make the following material clear, or the text can be revised to mention one step at a time.

We follow the reviewers suggestions, and have restructured the text in an attempt to make the steps in using the elevation classes clearer.

10. P. 3, Line 27: How is the weight of each elevation class within a grid cell determined?

The weight of each elevation class within a grid cell is determined by the area of the high-resolution topography dataset that lies within an elevation class. We will add this clarification to the revised manuscript.

11. P. 3, Line 28: I believe “average” is referring to the average surface to atmosphere fluxes, and outputs such as SMB and SMB components, but this is not clear.

This is correct and will be clarified.

12. P. 3, Line 33: Clarify to read “all ECs within a grid cell.”

Will be changed according to reviewers suggestion.

13. P. 4, Lines 2-8: It would be helpful if the authors can reiterate here which terms are common to all ECs within a grid cell, and which terms vary by EC as a result of downscaling. In particular it should be mentioned that albedo is calculated interactively within the model for each EC based on snow properties / snow depth over ice.

This is explained on p3, l31-33. We will add more information about the calculation of the albedo within each EC.

14. P. 4, Lines 11-12: Note that these quantities are calculated on the ESM grid. Also, aren't these quantities calculated by the atmospheric model CAM4 and not CLM4?

These quantities, as mentioned here are calculated by CLM. Take for instance the temperature. It is first calculated in CAM (taken into account e.g. advection), then passed to CLM. CLM simulates exchanges of moisture and heat with the surface, whereafter the temperature is passed on again to CAM.

15. P. 4, Line 30: Although the details of the setup are described by Vizcaino et al., the authors should mention briefly what forcing is applied (e.g. sea ice/ocean temperatures/ atmospheric nudging).

Have added that the simulations are fully coupled, and all components are allowed to vary freely.

16. P. 5, Line 15: Make clear why it is necessary to subtract the average CESM grid value from the RACMO2.3 grid cell values. I think this is to only capture gradients within grid cells, and not at the coarser resolution.

This was to illustrate the scatter as deviations from what is simulated at the grid cell mean, due to different climate realizations in the two models.

17. P. 5, Line 19: "mean elevation" is confusing. Perhaps use "on the CESM grid".

Changed to on the CESM grid.

18. P. 5, Line 23: "..comparison of the downscaled SEB components via EC and RCM" is confusing. It is the gradients that are being compared. Revise to something like "…comparison of SEB component gradients for CESM1.0 ECs and the RACMO2.3 RCM."

Changed accordingly.

19. P. 6, Line 10: Change "opposite gradient" to "opposite elevation gradient" for clarity.

Changed accordingly.

20. P. 6, Lines 16-18: The difference in sign here makes this a bit confusing. Including the longwave components in Fig. 1 or in a supplemental figure would help the reader to easily visualize this.

We will include this in a supplementary figure.

21. P. 6, Lines 22-23: Suggest changing "null gradients of incoming radiation in the model and weaker albedo gradients" to "a null gradient of incoming radiation in CESM1.0 and weaker albedo gradients than in RACMO2.3, leading to a smaller gradient in net shortwave radiation." Also, I believe this sentence is only referring to shortwave radiation, but this is not mentioned. Please clarify.

We do refer to both.

22. P. 7, Lines 11-12: What is the value of the gradient for CESM1.0?

The value of the refreezing gradient in CESM1.0 is 62 mm yr^{-1} km^{-1} as mentioned on p7, l3.

23. P. 7, Lines 14-15: Again, it would be interesting to see the figures for snowfall and refreezing.

Figure for snowfall will be included, figure for refreezing is already there (Fig. 2b).

24. P. 8, Lines 1-3: Not sure what is meant by "non-null variations". It would be clearer to simply note that the albedo gradient increases with increasing lapse rate, as shown in Figure 3.

It means that more EC points respond to the forcing in form of albedo change.

25. P. 8, Lines 14-15: Any idea why there is a reversal for the 9.8K/km case?

It becomes opposite in the 9.8K km^{-1} case as this forcing leads to higher amounts of energy being transferred to the lowest areas. Added this text to clarify this.

26. P. 8, Line 19: This is the first time interannual variability is mentioned. Perhaps introduce this with a separate sentence, explaining why interannual variability is interesting in this case and not elsewhere in the study.

We would argue interannual variability could be interesting in the first part of the study as well. On the other hand, we find the gradients and correlation to be more informative for analyzing the downscaled fluxes, as interannual variability is connected to atmospheric variability which is somewhat taken out when the grid cell mean is subtracted from each EC.

27. P. 9, Lines 13-15: This is confusing and should be clarified. I think the authors mean that the mean grid cell elevation is lower than the elevation of the ice sheet, so without ECs, the

simulated ice sheet is higher in elevation. This effect was also observed by Alexander et al. (2019).

Yes. Within each CESM grid cell, there is a range of elevations from the 5 km ice sheet topography. The mean of the elevations from the 5 km topography dataset is higher than the elevation that the CESM atmosphere “sees” both due to smoothing of the atmospheric topography, and that the atmosphere can “see” both ice sheet and vegetated parts of a grid cell. As the lapse rate correction is only applied to elevations corresponding to the ice sheet, this causes a higher areal weight of ice sheet points being forced with a temperature that is lower than the atmosphere simulates. The result of this is cooling of the grid cell.

28. P. 9, Line 27: Any idea as to why incoming longwave radiation changes?

Not only does the near-surface heat up, the entire lower part of the atmospheric column warms which is probably leading to the increased incoming longwave radiation.

29. P. 9, Lines 29-34: It seems ERA-Interim is used for locations outside of the RACMO2.3 domain? Please make this clear. Also specify here which fields are compared.

Correct, this is used as the lateral forcing for RACMO2.3 is given by ERA-Interim. We have changed in according to the reviewers suggestion

30. P. 9, Line 33: change “as with RACMO2.3” to “as differences with RACMO2.3” for clarity.

Changed accordingly.

31. P. 10, Lines 4-5: Other studies (e.g. Vizcaino et al., 2013; Alexander et al., 2019) have evaluated the EC method at the coarse resolution but not at a higher resolution as done here. This should be clarified here.

Will add this for clarity.

32. P. 10, Line 6: “Linear fits” of what? Please clarify.

Linear fits of these components with deviations of sub-grid elevation from the grid cell mean.

33. P. 10, Line 22: Change “enables to explore the interaction with” to “enables exploration of the interaction between the high-resolution surface simulation and…”

Changed accordingly.

34. P. 10, Line 25: Change “to RCM” to “to the RACMO2.3 RCM”.

Changed accordingly.

35. P. 11, Line 25: It could be that improving representation of physical processes at the elevation class scale will allow for a better identification of the optimal lapse rate. This could be mentioned here, if the authors agree.

Yes, that is a good point, and we will add this to the discussion.

36. P. 11, Line 32: Clarify “for radiation”, e.g. “for apparent biases in gradients of net radiation”

Changed accordingly.

37. P. 12, Line 11: Clarify “more adequate”.

By adequate we mean to represent snow compaction, firn, refreezing etc. more adequately. Will clarify.

Technical corrections

1. P. 1, Line 2: Remove “the” before “surface mass balance (SMB) modeling”

2. P. 1, Line 4: Change “elevation dependent” to “elevation-dependent”

3. P. 1, Line 20: Change “leading” to “which would lead”.

4. P. 1, Line 21: Change “is losing mass” to “has been losing mass”.

5. P. 2, Line 9: Perhaps change “seem required” to “are likely required”?

6. P. 2, Line 12: The van Kampenhout paper year can be changed to 2019, with the reference to the final revised paper updated in the reference list

7. P. 2, Line 17: Revise “Statistical downscaling, which uses elevation corrections on…” to “Statistical downscaling uses elevation corrections to…”

8. P. 5, Line 23: Change “r-value” to “r-values”.

9. P. 5, Line 25: Change “…solar radiation is not downscaled so that all ECs receive…” to “…solar radiation is not downscaled. As a result, all ECs within a grid cell receive…”

10. P. 6, Line 4: Change “more correlated” to “better correlated”.

11. P. 6, Lines 20-21: Change “The net radiation gradients…” to “The net radiation gradient in CESM1.0 is 5.4 W m-2 km—1 and in RACMO2.3 is -22.6 W m-2 km-1 (Table 1).”

12. P. 7, Line 5: Change “low ECs” to “low elevation ECs”

13. P. 7, Line 24: Change “compensates the biases” to “compensates for the biases”

14. P. 7, Line 26: Change “compensates this” to “compensates for this”

15. P. 8, Line 27: Change “therefore not only reflecting” to “therefore reflect more than just”

16. P. 8, Line 30: Change “high ECs” to “high elevation ECs” and “low ECs” to “low elevation ECs”

17. P. 8, Line 34: Change “lower than the magnitude of the respective” to “lower in magnitude than the respective”

18. P. 8, Line 35: Change “gradient is less” to “gradient is also less”

19. P. 9, Line 27: Change “overcompensated by” to “overcompensated for by”

20. P. 10, Lines 11-13: Suggest revising the sentence to read: “However, one of the limitations of comparing with an RCM is that unlike an ESM, the RCM is laterally forced with reanalysis. Also, there are fundamental differences in the physical schemes and simulated climate components between the ESM and RCM compared here.”

21. P. 10, Lines 13-14: Change “net longwave” to “net longwave radiation”

22. P. 11, Line 1: Change “although it varies” to “despite the fact that it varies”

23. P. 12, Lines 2-3: Change “efficient to generate” to “efficiently generates”

Thank you, these suggestions have been taken into account for the revised manuscript.