Response to editor's comments

Brett Gantt et al.

Editor comments:

Furthermore, it should be indicated in the manuscript, whether the changes in CDNC and the radiation feedback cause modifications of the circulations and how strong these changes are among the sensitivity simulations to get a rough estimate on the resulting uncertainty originating from the different activation schemes or feedbacks with meteorology. Please provide also a quantification whether the differences in the analyzed quantities are statistically significant, i.e. how large they are compared to the internal variability of the model, e.g. seasonal standard deviation compared with the differences of the mean of the two model simulations, as this would help to estimate the strength of the effect and discern it from numerical noise.

Reply:

We agree with the reviewer that meteorological feedbacks and internal variability within the model are important aspects of this study which are difficult to constrain due to the coupled nature of the model and short simulation time period. We have added the following statements to the updated manuscript to address these issues:

1) Although the climate impact of aerosol activation cannot be determined from our one-year coupled atmosphere-ocean simulations, the overprediction of precipitation and underprediction of 10 meter wind speed from AR-G00 were slightly reduced (by ~2%) in FN05/K09/B10 due to small modifications of meteorology from the different activation schemes.

2) With the exception of polluted regions in China, eastern Europe, and eastern U.S., these changes in CDNC are greater than the internal model variability as determined by the seasonal standard deviation from the AR-G00 simulation.

3) Because the various Earth System components of CESM interact in our simulations, these predicted changes in cloud properties (which are statistically significant with a probability value from a student's t-test << 0.05) cannot be entirely attributable to aerosol activation.

The manuscript has been much improved. However, the following clarifications need to be made before publication can be recommended:

1) Abstract, line 15: It seems pretentious to use the term 'realistically' here. Is there hard evidence to back up such a wording? I doubt it. Therefore, I suggest skipping 'realistically'. The sentence reads fine without it.

Reply:

We agree with the review and have removed the term in the updated manuscript.

2) Computational cost: In the previous round, I requested some information on this. That has now been added on page 5, line 98. That is good, but not sufficient. First of all, the sentence says "In our global simulations, the FN05 scheme increased computational time by ~10%.", but it doesn't say what the computational cost is when all the updates are included, i.e. comparing FN05/K09/B10/BN07 to AR-G00, rather than just comparing FN05 to AR-G00. Please add this information. Furthermore, the computational cost is crucial information to anyone who might wish to use these schemes. Therefore, that information needs to be (briefly!) mentioned also in the Abstract and the Conclusions.

Reply:

We agree with the reviewer that computational cost is an important factor in climate modeling, and have added information about the increased computational time for the FN05 scheme and updates (the updates to FN05 add negligible computational time) in the text and to the Abstract and Conclusions.

3) Figure 1 caption: The caption needs to explain what clouds are shown in panel b). Only low clouds over ocean? Is the Bennartz (2007) algorithm used for the retrieval? Without this information, panel b) has little meaning.

Reply:

This information has been added to the figure caption in the updated manuscript.

4) Figure 3 caption: It would help the reader if 'observed' were clarified, e.g. by adding "(from field campaigns)" or something like that.

Reply:

This information has been added to the figure caption in the updated manuscript.

5) Figure 4 caption: It is very confusing that "the increase (warm colors) in the shortwave cloud forcing absolute change represents more negative values." SWCF is almost invariably a negative quantity, so an increase (decrease) in its W m-2 value must surely mean that it is weaker (stronger). This needs to be changed!

Reply:

We agree with the reviewer that having negative values for the shortwave cloud forcing creates difficulties when graphically showing the changes between simulations. The caption has been changed in the updated manuscript to try to clear up this issue with the following: "Because the shortwave cloud forcing typically has negative values, the absolute change map (bottom left) uses |SWCF| so that the warmer colors represent an increase in the forcing even though they are more negative values."

6) Figure 5, left panels: These incredibly high numbers are basically useless, because the reader can't even count the number of zeros at the color bar. There are two easy alternative ways to solve this: a) Express values as multiples of 10**6 as in Figure 4. or b) Replace the unit cm-2 by

m-2, which would imply a factor 10**4 scaling. In either case, the same scaling and notation should be used for 'column CDNC' in Figures 4 and 5.

Reply:

We agree with the reviewer that the high numbers on the color bar were difficult to use, and have changed numbers the same notation as in Figure 4. However, we have kept the scaling in Figure 5 to show spatial detail of the changes that would have been difficult to see with the scaling from Figure 4. The updated manuscript now includes the following statement in the caption: "Note that the color bar range for the left column is a factor of 5 less than that of Figure 4 (top left) to better show spatial details."