Response to comments from Reviewer 2

Brett Gantt et al.

Reviewer 2: *General: The manuscript describes the influence on the performance of the CESM/CAM5 model system of a new treatment of cloud droplet nucleation. Six different simulations are compared, one using the Abdul-Razzak & Ghan (2000) activation scheme, while the other 5 use the Fountoukis and Nenes (2005) [FN05] activation scheme. These latter 5 simulations differ in the degree of updates/modifications of the FN05 scheme, accounting for processes such as insoluble adsorption, the impact of giant CCN activation kinetics, the impact of dynamic entrainment, or all of those.*

This is definitely a worthwhile study. The uncertainty in current estimates of the aerosol indirect effect is still very large, and model improvements are urgently needed. To develop and implement new physically-based parameterizations for the underlying processes, and then to validate them, as done here, is a key step in moving forward in this area. Therefore, I would like to see this work published. However, before acceptance of the manuscript can be recommended, there are significant problems with the presentation of the results that need to be addressed. They are described under "Major comments" below.

Recommendation: Major revisions

Reply:

We thank the reviewer for constructive comments. Please see our point-by-point reply below.

Major comments:

1) The choice of data for validation A number of data sets of satellite-retrieved cloud properties is now available, with different strengths and weaknesses, e.g., CloudSat/CALIPSO (e.g., Su et al., 2013 in JGR), MODIS/CERES, SSM/I. Even for data from the same instrument, different algorithms give different results, especially for LWP, which is particularly high in MODIS data. A discussion of this is needed, e.g. in section 2.3, as well as in connection with the validation of the results in Table 2. In the discussion on page 32302, the authors give the impression that the underestimation of LWP is solely due to model deficiencies and 3-D effects in the MODIS retrievals, but if another data set had been used for LWP, these large underestimations might not have been present. The MODIS deficiencies are particularly severe over the polar regions, where very large values of LWP and COT are found. Such high values over the polar regions are inconsistent with in-situ measurements (e.g. McFarquhar et al., 2007 in JGR on M-PACE), and are clearly an artefact of the MODIS retrievals. That is not surprising given the fact that solar radiation is basically absent for half of the year in the Arctic, and in addition the wintertime atmosphere there is almost isothermal. This means that most of the MODIS channels in the visible, near-infrared and infrared are more or less useless for detecting clouds during the winter season (mid-September through mid-March). The most reasonable way to deal with that in Figure 1 would have been to only show the MODIS data at latitudes equatorward of 50 or 60 degrees.

Reply:

We agree with the reviewer that MODIS data is unreliable for polar regions, and have adjusted Figure 1 and the subsequent discussion in the updated manuscript to exclude polar comparisons.

*2) The selection of figures and their size The paper contains a large number of figures (45), and many of them are not informative. In addition, most of them are too small to be legible. Specifically: a) Figure 1 is OK as it is b) Figure 2 is too small, but otherwise OK. There is plenty of space on both sides, so it should be easy to expand the figures. c) Figure 3: Better to place the two panels side by side. d) Figure 4: Again the figures are too small. Another issue: Why is the difference in SWCF and surface incoming shortwave given in %? That is difficult to interpret. W / m**2 would be better. e) Figure 5: These figures are very noisy, and there is absolutely no need to show both absolute and percentwise changes for each of the four simulations. Please choose one of them and skip the other! With four panels, the figures can be made larger for clarity. f) Figure 6: Again, the 8 panels are too many and too small. I suggest skipping at least half of them, either showing 4 panels for only one of the quantities or only showing 1 panel from each of the quantities, from the combined experiment FN05/K09/B10/BN07. g) Figure 7: Too many and too small panels. In additions, the panels for precipitation are so noisy, that there's not much point in showing them. Solution: As in Figure 6.*

Reply:

We agree with the reviewer that the magnitude and clarity of figures could be improved. In the updated manuscript, all of the figures have either been rearranged to improve clarity or removed as suggested.

3) Figure captions The captions do not give enough information. For instance, in Fig.1, which variable is seen on what panel? Also, in Figure 4, "changes : : :. between the FN05 and AR-G00" could e.g. be reworded as "changes from AR-G00 to FN05" for added clarity.

Reply:

We have updated the figure captions in the updated manuscript to better describe the figures.

4) The conclusions drawn from the validation exercise The authors need to be more neutral and objective when they discuss the results. For instance, they argue (e.g. on lines 23-24 on page 32310) that the results are improved for CDNC, COT and LWP. However, looking at Figures 1 and 2, we see that concerning CDNC there are improvements in some areas (e.g. over the mid-latitude oceans), while the results have become worse in other areas (e.g. over SE Asia, Europe and N-Africa), where significant overestimations are evident with the new activation treatment. Globally, it is simply not true to claim, as the authors do, that simulations of CDNC have been improved.

Reply:

We agree with the reviewer that the neutrality of the results can be improved, and have made subsequent changes to the updated manuscript when describing the model validation.

5) Somewhere, the authors need to give information about the added computational cost of the FN05 schemes compared to AR-G00. This could e.g. be done in the introduction (cf. current text on lines 10-11 on page 32295).

Reply:

The updated manuscript now includes information about the computational cost of the FN05 scheme, which takes 10% more than that by AR-G00.

Minor comments: Line 8, page 32294: Typo: "signal" should be 'single'. Line 10, page 32294: Ambiguous sentence: "more consistent with that of : : :". Not clear what is meant. Please rephrase. Lines 11-15, page 32295: Were really all these aerosol nucleation formulations used simultaneously. Why? Line 19, page 32298: Right parenthesis should be moved to come right after "BN07". Lines 18-19, page 32298: "resulted" should be "resulting". Same place: What is meant by "chemistry feedbacks to meteorology through various direct and indirect effects"? Please rephrase. Line 10, page 32302: "underprediction" should be "overprediction". Line 11, pages 32302: "compensates" should be "compensate", "results" should be "result". Line 21, page 32305: "in the Tibetan plateau" should be "over the Tibetan plateau". Line 14, page 32306: "due to feedbacks from : : :". What is the nature of these feedbacks? Line 14, page 32307: "improved" should be "reduced". Lines 21-22, page 32307: "because of the influence of radiatively active snow on overlying cloud fraction". How do you know that this is the reason? One possible solution would be to precede with "possible partly" or something like that. Line 3, page 32308: "underpredictions of CF, COT, and LWP". In fact, there is no underprediction for CF by AR-G00. Line 10, page 32309: "where" should be "by which". Line 21, page 32309: In general, the text in the manuscript is very often too technical with widespread use of acronyms instead of words. This is one example where a reader that perhaps only has time to read the Abstract and the Conclusions will stumble over the unnecessarily cryptic language. Instead of "AR-G00" and "FN05", please explain in words. Lines 1-2, page 32310: "which may be explained by feedbacks ..". What is the nature of these feedbacks? Line 11, page 32310: "The more accurate prediction of CDNC : : :". The prediction of CDNC is not in general more accurate than before (see major comment #4). Line 12, page 32310: Again, the "acronym syndrome". Please spell out "NMB".

Reply:

All above suggested changes have been made in the updated manuscript.