Atmospheric
Chemistry
and Physics

Discussions

Interactive comment on "Detection and attribution of aerosol-cloud interactions in large-domain large-eddy simulations with ICON" *by* Montserrat Costa-Surós et al.

Anonymous Referee #2

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Review of the manuscript numbered ACP-2019-850

Title: "Detection and attribution of aerosol-cloud interactions in large-domain large-eddy simulations with ICON" written by Montserrat Costa-Surós et al. Manuscript number: "acp-2019-850". Decision: "Major revision"

In this study, the authors conducted numerical simulations using large-eddy simulation mode of ICON (ICON-LEM) covering a large calculation domain (whole area of Germany) with fine grid resolution (156 m). They evaluated the ICON-LEM through the comparison between the results of satellite and ground-based observations and those of ICON-LEM. They also tried to detect and attribute the signal of the aerosol effects

on the cloud properties through the sensitivity experiment with changing aerosol. From their analyses, the authors indicated that the signal of the cloud aerosol interaction is only seen in the cloud number concentration and liquid water path larger than 200 g m-2. I think that the nesting simulation using the LES model covering such large domain has never conducted, and this is one of the unique points of this study. This study can be a basis of the numerical weather prediction with such fine grid resolution, and a basis of the numerical studies targeting on aerosol-cloud interaction by "real-case (nesting) simulation" with fine grid resolution. So, I evaluate the authors' efforts to conduct this study. However, most of the analyses conducted in this study can be done by the results of the simulation with "coarse" grid resolution. So, the manuscript has room to be modified as described below. Based on the descriptions shown above, my decision is "major revision", and I encourage the authors to modify the manuscript.

General Comment:

1: As I mentioned before, I evaluate the author's efforts to conduct simulation with fine grid resolution covering such large calculation domain. However, most of the analyses conducted by this study can be done by results of the simulation with coarse grid resolution. The analyses, which can only be done by the results with fine grid resolution, are required. Such analyses extend the value of this study. Entrainment around cloud edge, supersaturation and therefore CCN around the cloud base, and turbulence structure are examples of such analyses (Please do not misunderstand, entrainment, supersaturation, and turbulence are examples).

2: The author concluded that the signal of the aerosol-cloud interaction is difficult to be detected in terms of the cloud cover, cloud top height, cloud bottom height, liquid water path smaller than 200 g m-2. However, is this conclusion applicable for other cases? Based on the previous numerical simulation like Khain et al. (2008), the impacts of the aerosol perturbation on the clouds is dependent upon the meteorological field. I understand that the simulations for other cases using ICON-LEM require huge amount of computational resources, and it is not necessary to conduct the simulations.

However, the author should add comments about whether the conclusion of this study is applicable for other cases or not with referring previous studies.

3: The description about how to couple the aerosol and clouds in the ICON-LEM is not enough. The coupling of the aerosol and cloud is sensitive to the aerosol cloud interaction simulated by the model. In my understanding based on the manuscript, the number concentration of CCN calculated through the results of the COSMO-MUSCAT and the parameterization of Abdul-Razzak and Ghan (2000: AD2000) was given to the microphysical model of Seifeld and Beheng (2006: SB06) in ICON-LEM, and feedback of the cloud to the aerosol field was not calculated like off-line coupling in this study. Is this right? Or is the feedback explicitly calculated? The feedback of the cloud to aerosol (e.g., wet deposition) can reduce the aerosol and CCN number concentration. So, there is a possibility that one of the main conclusions of this study: "signal of the aerosol cloud interaction is limited to the number concentration of clouds (Nd) and LWP larger than 200 g m-2" could be change when the aerosol coupled on-line. Of course, I understand that off-line coupling is good as a first step, but I suggest the authors to add more detailed description about how to couple the aerosol and cloud in ICON-LEM (e.g., how to use CCN number concentration by AD2000 in SB06 with equation).

4: The discussion about the radiative forcing is poor. The authors discussed the radiative forcing for global scale through the scaling of the radiative forcing over the Germany. However, this discussion is unreasonable for the estimation of the global radiative forcing. I think that the discussion about the global radiative forcing is not necessary for this manuscript.

Major Comment:

Line 14 of page 2: Start writing of abstract and introduction are exactly same... I suggest the author to change the start writing of the introduction.

Line 9-10 of Page 4: There are no information about the vertical grid spacing. As well as the horizontal grid spacing, the vertical grid spacing is highly sensitive to the

activation of the cloud around the cloud bottom. The author should add the information about the vertical grid spacing.

Line 10-11 of Page 4: The detail information about the computational resources is not necessary.

Line 12-13: The authors describe the weather condition of target day at this part. The weather map of the target day is helpful for readers to clarify the location of high pressure and frontal system.

Line 15-16 of Page 4: In my understanding, the resolution of ECMWF analysis data is much coarser than ICON-LEM, and it is not suitable for the initial and boundary condition for the simulation with fine grid resolution. The author should be added the detail information of the initial and boundary condition (e.g., resolution, temporal interval, the physical variables used for the initial and boundary condition). In addition, if the initial and boundary condition is much coarser than ICON-LEM model, how do the authors drive the sub-grid scale turbulence? Was the small-scale turbulence, which can be resolved by ICON-LEM but cannot be resolved by ECMWF data, reasonably reproduced after the spin-up time (after 8 hours)?

Line 5-6 of Page 7: As I mentioned in the general comment, the detail descriptions of about how to couple the COSMO-MUSCAT's aerosol and ICOM-LEM are necessary. The detail information about the treatment of the CCN using equations is helpful for readers.

Fig. 2 and Table 2: The AOD simulated with CCN of 2013 is smaller than that observed by satellite. What is the reason of the underestimation of AOD?

Line 21-22 of Page 9: What is the reason of the overestimation of aerosols above the boundary layer? Is the overestimation affects the conclusion of the manuscript? I require the authors to add some comments.

Line 14-15 of Page 10: The authors indicate that graupel number and mass simulated

by clear case are higher at height of 3 − 4 km, but the difference between solid and dotted pink line in Figure 4 is too small to be identified.

Line 2 of Page 11: I think that "Distributions of liquid water path" should be "Probability density frequency (PDF) of liquid water path". Is this right?

Figure 5 and Line 6-7 of Page 11: The authors suggest that the difference in PDF between the model and MODIS is originated from the sensitivity of the MODIS. However, the geographical distribution of cloud simulated by the models are largely different from that observed based on Fig. 9. I think that such difference in the geographical distribution has impacts on the PDF shown in Figure 5.

Line 8-7 of Page 16: "simulated value of reflectivities fall into the range of the observations of MOL-RAO radar" should be "mean simulated value of the reflectivities fall into the range of the observation. . .".

Line 12-13 of Page 16: The author said the small reflectivity values of for the precipitation observations are due to noise by insects. If the authors know the signal is not originated from the precipitation, the author should remove the noise data.

Line 14-18 page 16: I think this paragraph is not necessary.

Line 20 of Page 16. How did the authors determine the cloud base height and CC simulated by the model? Was this the output of COSP? Usually, the edge of the cloud in the model is determined by a threshold value of LWP or ql. The threshold value is sensitive to the cloud cover and cloud base height. The results in Figure 8 is also sensitive to the threshold value.

Table 6: The authors indicate that the ICON simulate less cloud than observation and CBH is lower than that observed (even though the simulated CBH is included the range of 25-75th of the observation). In my understanding, such difference in the simulated and observed one is usually not originated from the problems in the model used by inner nested domain (i.e. ICON-LEM), but from the data used for initial and boundary

condition (i.e. ECMWF model). So, the author should check the data used for initial and boundary condition or results of outer domain (simulation with the grid spacing of 625 m and 312 m).

Figure 9: As I mentioned in the comment for Figure 5, the difference in the geophysical distribution of simulated cloud and observed one could have some contribution to the difference in PDF shown in Fig. 5.

Section 3.8: As I mentioned in the general comment, the discussion in this part is too rough. Of course, I understand the importance of the estimation of radiative forcing, but the estimation of global averaged ERFaci by the scaling of the results of regional model make readers misunderstanding.

Minor Comment:

Figure 1: The color scale (color bar) is helpful for the readers.

Line 2 of page 5: Reference and detail information of ECMWF analysis data should be added in the list of the reference.

Figure 4 left: For me, it is difficult to identify Black and blue line below the height of 6 km.

Line 6-9 of Page 14: The authors removed the data of the 15 stations because these stations are too close to other stations. I think that the averaged value of the close stations is better for the comparison with the model. The representativeness of the data of selected station is not always confirmed.

Reference:

Khain, A. P., N. BenMoshe, and A. Pokrovsky, 2008: Factors Determining the Impact of Aerosols on Surface Precipitation from Clouds: An Attempt at Classification. J. Atmos. Sci., 65, 1721–1748, https://doi.org/10.1175/2007JAS2515.1.

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