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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Investigating the role of dust in ice nucleation within clouds and further effects on regional weather system over East Asia – Part I: model development and validation" *by* Lin Su and Jimmy C. H. Fung

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We deeply thank Dr. Thompson for his valuable comments and suggestions, which help us improve the quality of the manuscript. It took us some time to submit the response, as we decided to revise the manuscript to address most of the comments before drafting the response. The detailed responses to each comment are shown as following.

Scientific issues: Computational time needed for the new treatment.

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Response: As dust is produced by WRF-Chem for the new treatment of dust-ice interaction, it requires longer computational time than using standard WRF only. Generally, the speed ratio between standard WRF and WRF-Chem with the GOCART aerosol scheme is 5:3. In our opinion, it brings improvements either by directly incorporation a dust emission into WRF, or hooking up the GOCART aerosol scheme and microphysics scheme. It is more efficient in computing dust-ice interaction with the first option. However, there are several dust emission schemes in the GOCART aerosol scheme, and more will be added. These dust emission schemes might have different performances depending on the regions. Therefore, it allows users to apply different dust emission schemes to produce dust emission for evaluating dust-ice interaction by hooking up the GOCART aerosol scheme with microphysics scheme. Thanks very much for the information about your work, we will mention the extra computational time needed for the simulation, as well as the alternate possibility in the revised manuscript.

Statistics for the performance of the model in simulating AOD and PM10 concentration.

Response: The correlation coefficients for AOD is 0.46 and 0.65 for Dalanzadgad and SACOL, respectively. The statistics for the surface PM10 concentrations are displayed in Table 2, but we have also put the correlation coefficient of each station in Figure 3 and 4 in the revised manuscript.

Minor comments:

Comment 1: At line#106 and similar lines mentioning the "Thompson scheme," would you please change all relevant places to "Thompson-Eidhammer scheme?" I would like to ensure proper credit to the coauthor of the WRF aerosol-aware scheme.

Response: we have replaced “aerosol-aware Thompson scheme” with “Thompson-Eidhammer scheme” in the revised manuscript.

Comment 2: Line#118: you state: "In the current version of WRF-Chem..." then mention the number of water/ice-friendly aerosols. Actually, this is not technically correct

as the creation of those 2 variables does not occur within any version of WRF-Chem. Those new variables are created irrespective of WRF-Chem for correctness in the paper.

Response: We have revised the statement into “For the Thompson-Eidhammer scheme. . .”.

Comment 3: Lines 126-128: "... can hardly represent realistic aerosol level..." is a bit too harsh. Actually, the use of a climatological aerosol concentration is closer to observations more often than not simply because the times when aerosols are extremely high or low as compared to climatology are the rarity in the true world as it takes a major weather change to produce the more extreme values. Take for example a place in the world with consistent southerly winds and a large urban region to the south of a point in question. When the winds are from the north, from a region of very low aerosols, then the point in question may be experiencing a large departure from climatology. Or if a significant weather front moves through a region and pushes away a majority of the aerosols, then the departure from average is more significant than the days in which the "regular weather regime" is occurring. So I am only suggesting to you that the wording here could be an exaggeration that using climo aerosols is always wrong.

Response: We have modified this part to make a fairer statement in the revised manuscript.

Comment 4: Line 205 and 223: The internals of the Thompson-Eidhammer scheme contains a wet deposition removal by precipitation. Did you disable this internal sink of aerosols when putting in your own wet removal process or is there a double-counting of aerosol removal?

Response: Yes. The GOCART aerosol scheme calculates the number concentration of aerosol every time step, it makes no sense to update it in the Thompson-Eidhammer scheme, so we turned it off while calculating the wet deposition using a new scheme.

Comment 5: Fig. 6b: I believe the units label is incorrect if the plot is supposed to show cloud ice number concentration.

Response: Revised in the updated manuscript.

Comment 6: Fig. 7: Can you offer any explanation for the very very large increase in ice number concentration showing along the southern boundary of the domain? The increase looks tremendous.

Response: During dust season, the outbreak of cold high system over northeast Asia can bring quantitative dust aerosol down to the South China Sea or even further, a typical case on March 26, 2012 is shown in Fig. R1 attached at the end of this response. In such cases, strong northwestlies swept across the entire China, and brought large amount of dust from source areas to the south border of the domain. Besides, the water vapor mixing ratio over south China Sea can be over five times as that over north China as shown in Fig. R2. Large amount of ice nuclei transported by winds, combining with abundant water vapor, results in a significant enhancement in the formation of ice crystals over the area.

Comment 7: Line 433-434: If moist convection is too weak in the model and insufficient water vapor is not lofted high enough, then do you mean there is an *under* prediction of ice water path? The text says there is an over-prediction, but I do not understand this apparent contradiction.

Response: It was a mistake and should be “underestimation”, we have corrected it in the revised manuscript.

Comment 8: Fig. 11: Is the peak shown _4km coinciding with the melting level? If so, is it a data processing issue of falsely identifying ice clouds in CALIPSO data?

Response: The melting level is slightly below 4 km for the cases in Fig. 11, it is reasonable that ice clouds are observed at 4km, but the small peak at $\sim$2 km in the third case in Fig. 11 might be due to falsely identifying ice clouds in CALIPSO data.

Comment 9: Line 531: "...once the coagulation makes the relative humidity..." I cannot determine how the word coagulation is being used here. Can you please re-phrase this sentence?

Response: It has been revised in to " freezing of droplets" in the updated manuscript.

Comment 10: Figs. 12 and 13: Is the ice water content computed from a combination of cloud ice mixing ratio and snow? I strongly advise combining both since cloud ice species in Thompson is literally only the smallest ice crystals (generally mean sizes below 40 microns) whereas the snow is much larger (mixing ratio as well as mean size).

Response: The ice water content shown in Fig. 12 and 13 contains only cloud ice. The reason we did not use a combination of cloud ice mixing ratio and snow is that, the ice water content in CALIPSO products represents only the amount of cloud ice, as the CALIPSO instruments are more sensitive to ice clouds and liquid clouds composed of small particles or droplets, and the LIDAR signal emitted from CALIPSO attenuated rapidly in optically dense clouds (more detailed description can be seen in section 5.2.2). Therefore, for comparison of the vertical profile of ice water content with the CALIPSO products, we only apply the mixing ratio of cloud ice. The combination of simulated cloud ice mixing ratio and snow is compared to MODIS products in Fig. 8, but only for a spatial comparison, due to the limitation of observations.

Comment 11: Line 567: "A new microphysics scheme..." Calling this entirely a 'new' scheme is probably a stretch. The microphysics scheme is still much more than dust nucleating as ice, so a 'new treatment' for a source of dust used in the existing scheme is perhaps a more fair description.

Response: we have revised it into "a new treatment" to avoid overstating the implementation work.

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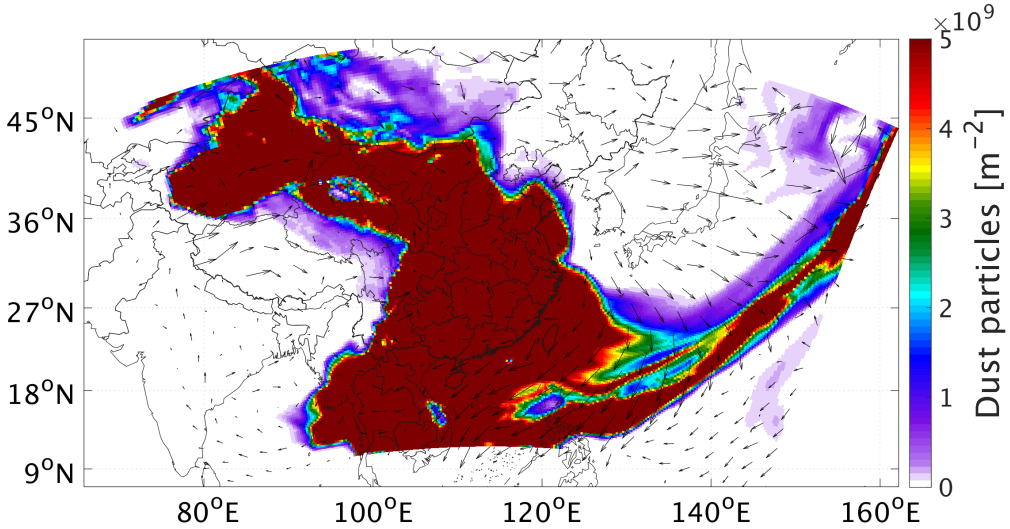

Fig. 1. The spatial distribution of dust particle number density over East Asia at 04 UTC, March 26, 2012.

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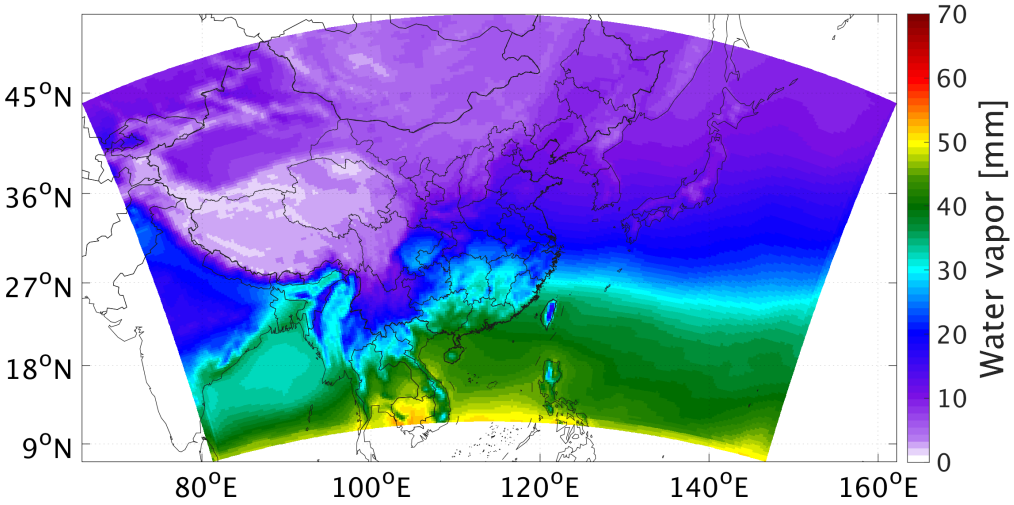

Fig. 2. The spatial distribution of the mean column sum of water vapor over East Asia during the simulation period.

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