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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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General comments:

I was aware of this work through various discussions with the first author while she was visiting Boulder, Colorado. Overall I find the manuscript reads quite well and has ample new material to warrant publishing. The results are clearly presented and the comparison to available observations is sufficient, although still a little less than I would prefer; however, this part of the world can be difficult to obtain high temporal and nu-

merous spatial observations. Due to the nature of dust storms there are many times when a big event occurred but the observed data contain outages (noted in Figs. 3 and 4, for example). Ultimately I believe the paper requires relatively minor revisions at this stage.

Scientific issues:

In general, please state how much extra computational time is needed to run WRF-Chem as configured here as compared to a WRF simulation with all else the same including the aerosol-aware microphysics scheme but not utilizing WRF-Chem at all.

The reason I am asking is because we were also working on direct incorporation of an existing GOCART dust emissions scheme directly into WRF, but completely independent of using WRF-Chem. I conveyed this to the author in person a couple years ago. Since then, we also have the same contribution to the ice nuclei variable but simply combine 4 distinct dust sizes into the single category, final variable (e.g. QNIFA), external to WRF-Chem. The result is a far lower computation cost, probably by a massive amount.

As a test of our own newly added capability and due to seeing this manuscript, I ran the WRF model configured very similarly to what was done by these authors from 12 March to 30 April 2012. Solely as a quick-look graphical comparison to Fig. 4, I created a time series at the 2 AERONET sites for AOD obs versus the model. The resulting figure is attached to this review. The final AOD shown in the attached figure appears better at tracking the "background" value when dust storms are not present as compared with Fig. 4 in the manuscript, which has a model-predicted value that is nearly always lower than observations. When combining the data from both AERONET sites, the correlation coefficient of AOD for my WRF run was 0.59.

Besides stating directly in the paper what is the added cost of running WRF-Chem as compared to 'regular' WRF, would you please include the correlation coefficient for each data location shown in Figs. 3 and 4? A single value per panel would be nice to

see.

A point this test raises is whether or not the use of WRF-Chem is at all needed to gain a large computational efficiency while still predicting dust outbreaks and resulting changes to AOD, coverage of ice clouds, etc. Perhaps a few sentences in the revised manuscript to point out this alternate possibility could be made clear.

Minor comments:

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.

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.

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.

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?

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

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.

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.

Fig. 11: Is the peak shown $\sim$4km coinciding with the melting level? If so, is it a data processing issue of falsely identifying ice clouds in CALIPSO data?

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?

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).

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.

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