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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Simulation of particle formation and number concentration over the Eastern United States with the WRF-Chem + APM model" *by* G. Luo and F. Yu

Anonymous Referee #2

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1 General comments:

The manuscript describes a detailed simulation of chemical transport model simulation, concentration on new particle formation over the North America. They compare the results with extensive field experiments based mostly on INTEX-A airborne measurements. I find the overall methodology valid, but there are still many details to be corrected and discussion to be added.

As I noticed that Dr Fast had already published very detailed and comprehensive referee comment , I will try to consider some points outside of this comments to avoid

overlap (although not completely succeeding).

I must however comment, that I do strongly agree with Dr Fast that the authors do have very optimistic view on their model performance, and especially performance of IMN as a mechanism. I recommend that they will change the wording of the manuscript accordingly. I also agree with him on the lack of discussion on the causes of the scatter in the model/measurement comparisons.

Some comments:

1. How do the model distinguish between secondary formed and primary CCN? I could not easily deduct are the particles handled completely externally mixed? What happens in mode-to-mode coagulation?
2. The APM explanation (2.2) needs to be reduced. After line 3 in 14665, there is very little relevant information for this paper, as this work should be considering what is done, not what is possible to do with APM
3. The authors claim VOC as one of the emissions considered (ln 4, pg 14666). How does the VOC oxidation take a part in the system? They claim that it does not take a part in the nucleated particle growth, but do SOA from SORGAM module any way influence the system?
4. What are the consequences of removing all anthropogenic sulphur emission in comparison to INTEX-A measurements? This is important issue, not only from direct nucleation point-of-view, but for the overall growth to CCN or removal by coagulation point-of-views. I would urge the authors to consider a small sensitivity analysis of this.
5. Why are the sectional bins so different in different particle types? How do the coagulation work between these types and do you consider that e.g. sea-salt par-

ticles covered with SOA or sulphate? Are the particles always externally mixed? The section 2.3.2 is rather difficult to read.

6. line 20, pg 14668: Are the sulphate particles ONLY scavenged by primary particles? Not at all by (quite numerous?) CCN sized grown particles from NPF? What are the consequences of such choice?

7. As the authors do not consider primary sulphate emissions, this might be of less importance, but do the emission rates of primary number emissions take into account sub-grid scale coagulation (e.g. Pierce et a, 2009). If not, some discussion on the importance of such processes are needed.

8. I think it is good that the authors considered the ability of the nudged CTM to actually produce the measured meteorology (3.2.1). However, as the BL processes are not well re-produced by the model (not surprising), the discussion on the NPF should also include BL evolution as and error source of NPF, especially as most of the nucleation seems to (?) happen near BL.

9. The CN10 discrepancies between measurements and model results could also be strongly affected by the growth/removal in the 1-10 nm part. Here the absence of organics can be crucial. Where does the NPF happen in the model and how would the increase of growth rates in BL from organics affect the results?

10. Figure 1 two leftmost columns are rather hard to distinguish where the changes actually are. One way would be to color code the scatterplots (right column) with measurement height.

11. as in the above point, figures 2 and 3 scatterplot (b) should be color-coded by altitude. Then (c) and (d) could be replaced with modelled top view (as in (a))

12. The resolution based comparisons (Fig 4 and 5) are interesting. I would like the authors to include (for direct comparison) an additional figure, at least on the map

figures: How would the WRF-CHEM-APM results look, if they would be averaged to the GEOS-CHEM-APM grid? This would also show if the "small" scale processes of WRF would actually have an important effect on the cloud processes in the GEOS scale. If we would then consider WRF simulations as "accurate", this would work as some sort of approximation on sub-grid scale variation effects of the processes in the global scale model?

2 References

J.R. Pierce, G. Theodoritsi, P.J. Adams, S.N. Pandis, Parameterization of the effect of sub-grid scale aerosol dynamics on aerosol number emission rates, Journal of Aerosol Science, Volume 40, Issue 5, May 2009, Pages 385-393, DOI: 10.1016/j.jaerosci.2008.11.009.

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