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Comment

Interactive comment on “Processes controlling the seasonal cycle of Arctic aerosol number and size distributions” by B. Croft et al.

Anonymous Referee #2

Received and published: 23 November 2015

The manuscript by Croft et al. 2015 explores the seasonal cycle in Arctic aerosol number and size distributions and the key processes controlling this seasonal cycle through model/measurement and model/model comparisons. Overall, the manuscript is very useful and well-written. I found no major scientific error, but I wish the authors would present the model/model comparisons a bit more carefully. I recommend this work for publication after the following comments are addressed.

Specific comments

Page 29081, line 7. The authors jump from the global aerosol to Arctic aerosol suddenly. The transition seems abrupt. Also, the motivation for studying Arctic aerosol seems a little weak to me. The authors should add a little bit discussion more on why particularly on Arctic aerosol. For example, the climate in Arctic is more sensitive to

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aerosol perturbations than other regions due to the complex positive feedback system there such as snow albedo feedback. This would make the transition more smooth and the motivation stronger.

Page 29091, line 18. What did the authors mean by “aerosol formation”? new particle formation? And by “reducing the condensation sink”? “condensation sink” on accumulation mode aerosols?

Page 29083, line 11. What is the difference between TSI 3776 CPC and TSI 3772 CPC?

Page 29084, line 7. “the same instrument configuration”? the same as what? Can the authors also clarify which year’s data they used at Mt. Zeppelin site and Alert site?

Page 29084, line 10. What purpose is the Ni-63 neutralizer used for?

Page 29084, line 21. “4 degrees by 5 degrees resolution”. Do the authors have any sense how this coarse grid resolution would affect the model results?

Page 29085, first paragraph. Can the authors briefly clarify how they treated the condensation growth and coagulation of particles in the model? I believe it would help readers to understand results. Did they consider the effect of nitrate or/and non-volatile SOA on condensation growth? on which size sections? Did they treat coagulation among all size sections? Or just between size sections that are next to each other?

Page 29085, line 25, Liu et al. (2001) is not appropriate for dry deposition, though it suits well for wet deposition.

Page 29087, line 8. Is there any justification for $1 \times 10^{-3} \text{ s}^{-1}$?

Page 29088, line 12. Pierce et al. (2014) is not seen in the reference list.

Page 29091, line 8-9. This is not consistent with what the authors stated on page 29090, line 9.

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Page 29091, line 20. Why does wet scavenging have less control on accumulation mode number in the non-summer seasons than the summer season? Because of less precipitations?

Page 29091, line 25. “Reduces the condensation sink”. the sink of sulfuric acids?

Page 29092, line 26. “Not enough material to contribute to new-particle growth”. Did the authors consider the condensation of SOA on it?

Page 29093, line 7-9. Do these volatile organic compounds come from ocean as well? Is that possible that the deposition of Aitken mode aerosol is underestimated at Alert site?

Page 29093, line 11. “at both sites and for all seasons”. I would say except for the summer season at Alert site.

Page 29093, line 12. This sentence is a digression. Also, the authors still discuss figures 4 and 5 in the following paragraph. I suggest removing this sentence.

Page 29093, line 19-21. Do the authors imply here that the overestimation of Aitken mode aerosol numbers can be explained by the errors in nucleation scheme (NPF)? This is in contrary to the statement in previous paragraph. In addition, is there any way to evaluate NH₃ in the model at Alert site? Did the authors think about the possibility that nucleation is actually associated with organic compounds while the model neglected this?

Page 29093, line 25-27. This is a misleading statement. The authors imply that aerosol nucleation is not important for non-Summer Aitken mode aerosol. This is not supported by Figures 4 and 5. The difference of Aitken aerosol numbers between the NEWSCAV simulation and the NONUC simulation is nearly as large as (or even larger in the Fall season) than that between the NEWSCAV simulation and the NEWSCAV+COAG simulation. This suggests that the nucleation may be as important as the in-cloud coagulation, at least a non-negligible process, for non-Summer Aitken mode aerosol. Also, the

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20-50 nm aerosol number concentration predicted by the NONUC simulation is even closer to measurements than the NEWSCAV+COAG simulation at both sites on Fall (SON). This paper do show the importance of the coagulation in clouds, but it also shows the importance of nucleation.

Figure 6. Please clarify the gray shaded region.

Figure 7. Can the authors explain why all simulations over-predict aerosol numbers for JFM?

Page 29094, line 13. This sentence is not accurate. The STD simulation captures measured N20 on JJA at Alert site better than the NEWSCAV+COAG simulation.

Page 29095, line 24. Did the authors imply that most of precursors for the nucleation in early spring are transported from the outside Arctic? Because the authors stated next line that in summer there are greatest local precursor emissions.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 29079, 2015.

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