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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Spring and summer contrast in new particle formation over nine forest areas in North America" *by* F. Yu et al.

Anonymous Referee #3

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This paper compares modeled and measured nucleation events in spring and summer at nine locations in North America. Two nucleation parameterizations are used in the model: ion-mediated (IMN) and the CLOUD BioOxOrg empirical parameterization. The paper is very clearly written and the figures and tables are clearly presented. I recommend this paper should be published subject to a few revisions. I have some general recommendations below:

The authors need to clearly present what the novelty is of this study. In the current manuscript, this is not clearly stated. For example, it is well known that new particle formation occurs more frequently in the spring than the summer at these types of locations, and that models have varying degrees of success at accurately predicting this seasonality. Arguably, the large overprediction in nucleation events by the BioOxOrg

simulation is the more novel result, so this should be emphasized over the comparison with IMN. The paper seems to take for granted that the IMN mechanism should be considered the "base case" simulation. Also, one of the main objectives of the paper, model-measurement comparison of nucleation events, has been performed many times, including by this research group. I have not found any evidence that this model (APM) is used very widely in the community, so the authors need to justify the scientific significance of evaluating it.

The authors should also view there model with a more critical eye. They should specifically justify their SOA mechanism and nucleation mechanism. Have they evaluated their organic aerosols concentrations against AMS data in the past? If so, mention this. It is also possible that the modeled LV-SOG(alpha-pinene) is not representative of actual ambient low volatility organic aerosol, something that is briefly mentioned by the authors but warrants further explanation.

Specific comments: 1) p. 21274, line 4-5. The authors cite 80-95% total contribution of NPF to CN concentrations, and 50-80% contribution to CCN. Only a previous paper from this group, Yu and Luo 2009, is cited. A more exhaustive review of the literature on NPF contribution to CCN is warranted here. Also, the definition of "contribution" is also important here, as many of these papers are actually sensitivity studies.

2) p. 21274, line 10. Sulfuric acid should be mentioned here

3) p. 21275 line 10. The Egbert mention should include a citation of Pierce et al. (2014) ACP.

4) p. 21275 line 24 and p. 21276 line 3. Overuse of the phrase "state of the art"

5) p. 21277 line 26. Can the coarse resolution model grid (2x2.5) accurately represent nucleation at a specific point?

6) Results section, Fig 3 and 4. Is 10 days and up to one month enough data to make the conclusion that the BioOxOrg parameterization may not be applicable? If the data

for more spring and summer months are available, that analysis would make the paper stronger.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 21271, 2015.