

## ***Interactive comment on “Impact of temperature dependence on the possible contribution of organics to new particle formation in the atmosphere” by Fangqun Yu et al.***

### **Anonymous Referee #3**

Received and published: 4 November 2016

Review of Yu et al. 2016, “Impact of temperature dependence on the possible contribution of organics to new particle formation in the atmosphere”

The authors explore temperature dependence effects on organic-sulfuric acid new particle formation in a chemical transport model with aerosol microphysics. This is an interesting and concise paper that addresses a significant missing piece (T-dependence) of current new particle formation research. However, I have a few major comments that need to be addressed before I can recommend this paper for publication. Once these are addressed, I believe the paper will be stronger and more impactful, and will be ready for publication.

Major comments:

1. There is a lot of uncertainty in the Nucl-OrgT parameterization. The authors do acknowledge this, discussing the uncertainty in the chemical identity of the stable cluster molecule and the subsequent uncertainty of the  $\Delta H$  and  $tT$  values. The authors mention explicitly on page 8 around line 150, suggesting that a change of 5 kcal/mol would result in a 40% difference in  $tT$  at 288 K, whereas a 20 kcal/mol change results in a factor of 3.5 difference. Although the acknowledgement of this uncertainty is helpful, why not actually implement this into the simulations? Since the uncertainty in the  $\Delta H$  values is that large, I would encourage them to explore the sensitivity of their results (nucleation rates, CCN formation, etc) to using different values of  $\Delta H$ . It would be very interesting and useful to the community to have a range (low  $\Delta H$ , high  $\Delta H$ ) of results for Tdependent organic-sulfuric acid nucleation.

2. The authors conclude that the temperature-dependent Nucl-OrgT parameterization is “likely” more realistic than Nucl-Org, which does not account for temperature. While it makes sense that adding something so fundamental to nucleation as temperature would improve a parameterization, this is not automatically the case. What I find more concerning is that I do not think the authors have appropriately justified this conclusion that Nucl-OrgT is better with the results presented in this paper. Besides the qualitative argument I mention above, the only evidence in this paper we have comes from Fig 5, where  $\sim 10$  nucleation events at one location (Duke Forest) in spring and summer are used to validate the model. Figure 4 and its associated text mention that the Nucl-OrgT does better against observations but there are no observations actually plotted in Fig. 4! Another paper is referenced, but to make this conclusion for this paper, the comparison needs to be explicitly presented. Thus, I suggest the authors do show this observational data and present statistics on how it compares to the Nucl-OrgT simulation, for more than only the Duke Forest site.

3. The authors tend to take their model and its configuration somewhat uncritically. For example, the introduction paints a clear picture of new particle formation being a “strong” and “significant” contributor to the aerosol indirect effect and CCN concentra-

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tions, citing mostly their own work. The authors should perform due diligence to other work which may not agree with their single model findings (some suggested citations below). Indeed, despite changing the mean nucleation rate by nearly an order of magnitude with Nucl-OrgT, CCN0.2 are reported to be changed by only around 10-20% globally, suggesting probably a weaker sensitivity than is introduced by the authors.

Spracklen et al. 2008, JGR, 35, L06808

Westervelt, et al, 2014. Atmos. Chem. Phys., 14, 5577-5597

Other comments: 1. Line 35-36, Page 3: While technically true, the impact of aerosols on the surface solar radiation balance and thus atmospheric circulation is a stronger control on the hydrological cycle than the indirect effect.

2. Line 67 Page 4: “In the recent study. . .” change to “In a recent study”. Also looks like there are too many spaces between “the” and “recent”. There are a few other instances of superfluous “the”, please proofread carefully.

3. Line 136, Page 7: Why is fT capped at 10? Seems arbitrary.

4. Line 132, Page 7: Along with major comment 1, explain why this  $\Delta H$  value is used.

5. Line 202, Page 10: “agree much better with the observed values”. As per major comment 2, need to actually show this.

6. Line 260, Page 12: “To our acknowledge”. This should say “To our knowledge”

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Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-812, 2016.

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