Interactive comment on "The potential role of organics in new particle formation and initial growth in the remote tropical upper troposphere" by Agnieszka Kupc et al.

Anonymous Referee #3

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The paper investigates the origin of nucleation mode particles in the upper tropical troposphere and makes use of the extensive ATom dataset in the process. The authors use nucleation and growth box models evaluated along the trajectories of air masses reaching from the outflow of tropical convection until the air was encountered during ATom flights. They find that the nucleation parametrization of Riccobono et al. 2014 can describe the measured datasets best, which indicates organic involvement in nucleation. Also, if growth driven by organics is included in the model, the agreement with observations is enhanced as well, further pointing to the importance of organics in the upper tropical troposhpere.

The paper offers very interesting insights into the important, but notoriously hard to observe topic of upper tropospheric new particle formation. The authors corroborate their conclusions with an extensive set of measurement and model data that are nicely presented. The paper is well within the scope of ACP; it is well written and clearly structured. The methods used are described clearly as well as their limitations. I recommend publication in ACP and only have some minor comments to add:

The paper focusses on nucleation rates, which is very insightful, however, in the model also growth, especially driven by organics is incorporated. You state that the inclusion of organic growth enhances the agreement between model and observations in many cases. However, you do not give a range for the growth rates needed to do that. Can you add a figure and/or short discussion that indicate the range of growth rates used in your model?

Are you in Table 1 discussing the ion concentration as indicated by the given parameter name or the ion pair production rate as indicated by the given unit ($cm^{-3}s^{-1}$)? Please add a short description in section 2.4 that describes the origin of this quantity. The value 15 in Table 1 would correspond to the ion pair production rate in $cm^{-3}s^{-1}$ given in Dunne et al. 2016 for the upper tropical troposphere.

l. 322: You state "We have undertaken sensitivity studies that vary the pre-existing background aerosol used as input parameter (Table 1)", but in Table 1 there is no information on how this quantity was varied, as it is only named. Can you give more detail in Table 1 on how you varied the pre-existing aerosol and/or refer to the SI part where you discuss this in more depth?

l. 485: You state that "varying the scale factor for organics taking part in nucleation (F_{orgnuc}) did not change results significantly". Can you add a short discussion on why this is the case? You span one order of magnitude in [BioOxOrg] with the scaling factor values you use, so according to the RIC scheme, this should vary the nucleation rate as well by an order of magnitude. So in Fig S48 I would guess that you would need higher

organics in the F = 0.1 case to match observations than in the F = 1 case. However, the blue dots are practically identical in all cases. So what is the compensating effect for that?

Figure 7: It is a bit misleading that you place the black symbols for Aitken and accumulation mode at x = 0, as these originated from the measurement as you write and the measurement was not taken at x = 0. Even if you use them as starting point for the model, I would still place them at the point of actual measurement.

Figure 7: I would suggest to remove grid lines in between the panels.

l. 577: "5-20 size range": You mean nm?

Figure 2: Add an altitude axis on the right, as in Fig 2 in Williamson et al. 2019