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Atmospheric Chemistry and Physics Discussions

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 #2

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This manuscript tackles a globally important research topic: formation of new aerosol particles in the tropical upper troposphere. The paper is essentially a sensitivity study, aiming to give new insight into which nucleation mechanisms and aerosol precursors, coupled with the initial growth of newly-formed particles, best explain the observed ultrafine particle number size distribution. The conducted analysis is based on box model simulations and statistical analyses of the simulation results. The paper is scientifically sound and relatively well structure. I do have, however, a few issues that should be addressed before the paper is ready for publication.

My major criticism is related to the treatment of aerosol processes in convective outflow

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Discussion paper

regions. As the authors state, they simulate outflow regions of deep convective clouds using a box model. This is fine as long as both nucleation and growth occur well beyond the region abound cloud boundaries where most of the mixing between cloud outflow and upper troposphere air take place. This may not be the case, as it is quite possible that nucleation and early particle growth take place in the mixing region, or event inside the convective clouds. This would seriously bias the results obtained in the paper. I understand that including the cloud and its immediate outflow region in a box model is almost impossible, and therefor outside the scope of the current paper. However, the authors should bring up this issue more honestly as done in the present paper (brief mentioning on lines 628-629). Furthermore, there are a number of both modeling and observations studied conducted on new particle formation in cloud outflow regions. The authors should better acknowledge such studies when discussion their results, their implications and the associated uncertainties.

Other, minor issues:

The right parenthesis is missing from line 432.

The text on lines 538-543 is not logical. When discusses nucleation mechanism not involving NH3 at all, it is incorrect to say "regardless of NH3 oncentraions", as the outcome of such mechanisms does not depend on NH3 concentrations. Please correct.

The statement on lines 546-548 sounds a bit strange. Is really so that the concentration of both SO2 and organic precursors need to be smaller than some upper limit values to reproduce the observations? Please check out this statement and modify if needed.

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