

Interactive comment on “Does the POA-SOA split matter for global CCN formation?” by W. Trivitayanurak and P. J. Adams

Anonymous Referee #4

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The manuscript by Trivitayanurak and Adams describes two rather distinct scientific topics. The first, which does not appear in the title of the paper, is the implementation of carbonaceous aerosols in the TOMAS microphysics, hosted by the CEOS-Chem chemistry/transport model. The second is a set of experiments that try to quantify the primary-secondary organic aerosol split from the microphysical point of view, assuming that only primary aerosols affect aerosol number. The study is of potential interest, but has some significant problems and limitations that do not make it suitable for publication in ACP in its present form. In addition, given the two different topics covered, I would suggest to split the paper in two; one will properly describe the carbonaceous aerosol implementation and will do a thorough comparison with measurements (probably a GMD paper), and the other will study the POA/SOA split.

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One important aspect is that the manuscript is not citing all relevant literature. In addition, the few references that are used are mostly old, with only about 10 of them being from the last 5 years. The same applies to the data used for the model evaluation; they are very few and rather old. Numerous datasets have become available from more recent campaigns and data compilations. The authors should seriously consider updating their datasets for a more detailed model evaluation. Comparing only a dozen European stations for carbonaceous aerosols and about that many for CCN mostly over Europe and the USA is far from sufficient.

Specific comments

CCN(0.2%) is quite long. I suggest using CCN, while mentioning that for the whole manuscript a 0.2% supersaturation is implied, unless otherwise noted.

p. 10568, l. 14-17: Is this a guess, or there are references that support it? How sensitive is the model on the choice of these numbers?

p. 10568, l. 20: I do not understand what the point (1) means.

p. 10568, l. 26: anthropogenic aromatic organic compounds have a small contribution; IVOC have been shown to produce much more.

p. 10570, l. 1: There is no externally mixed OC? If not, why? Externally mixed primary OC can evaporate and reduce the total aerosol number. What fraction of EC is externally mixed, on average?

p. 10570, l. 11: How sensitive is the model on the choice of 140nm for activation?

p. 10570, l. 11-15: where do these numbers come from? Are they best-guess, or found in the literature? What do other models use?

p. 10571, l. 20: Why neglect SOA distribution? The different POA/SOA splits can be justified due to different POA aging timescales, but monoterpenes SOA should always be present. Neglecting this source will strongly affect the aerosol microphysics, hence

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aerosol lifetime and CCN. In the idealized experiments the biogenic SOA should always be present. How about SOA from isoprene?

p. 10574, l. 12-15: Why not use the data mentioned at p. 10572, l. 15-17 as well?

p. 10576, l. 5: "predict" is a very strong word, since the experiments presented in the manuscript are idealized and are not based on real SOA calculations.

Table 3: Why does nucleation (J10) change? You use organics in the binary nucleation? Also, the microphysical growth is in reality the amount of aerosols that cross the 80nm size due to growth alone, right?

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