Review of “Description and Evaluation of a Secondary Organic Aerosol and New Particle Formation Scheme within TM5-MP v1.1”

This article describe the inclusion of a more explicit parameterisation of the formation of secondary aerosol and nucleation induced by extremely low volatile organic compounds and sulphuric acid and new emissions aerosol in M7 aerosol module embedded within the global chemistry transport model TM5-MP.

The work includes a description of these processes as well as a comparison with the previous parameterisations including emission changes

The authors have included a quite extensive comparison with measurements of both aerosol mass, number concentration, and aerosol optical depth. This output is given with a high temporal resolution of output in order to catch individual nucleation events. The relatively coarse resolution of the CTM make it however make a detailed comparison with station sites.

While none of the parameterizations are new, the more physical approach is a useful addition to aerosol modelling tools, in particular for M7 and derivatives thereof. The article is in general well structured although sometimes it is a bit unclear when the authors describe the results from the old or the new version. A somewhat more problematic issue is that the authors often conclude that this is caused by emission changes and this is due to secondary aerosol formation. While it may be obvious for most cases I think the authors should do a sensitivity test that only show the impact of the emission changes. The assumptions on yields is another parameter that should be discussed although the emission test will probably work as a first approximation on understanding yields.

It may beyond the scope of the article and or due to missing capabilities of the model I am wondering if the authors also considered comparing the Ångstrøm component in particular in light of the focus of aerosol size changes from the new parameterisaton.

The paper is suitable for GMD and I recommend that the papers should be published after the manuscript provides a better description on the effect of emission changes versus the microphysical / chemcial changes in the model. I have also added some minor issues below

Minor / technical issues.

The introduction splits the organic compounds between ELVOC, LVOC and SVOC, Later on it only use ELVOC and SVOC. My usual interpreation is that SVOC s reserved for thos compounds that can commonly re-evaporate. As I understand none of the VOCs described in this work is re-evaporating. I think this should be stated explicitly if the authors continue to use SVOC.

Abstract line 11. “clearly lower as is” new versus old ?

Line 32: Missing in first volatility word

Line 34. Numbers inside “vegetation”

Line 39: Two “rathers” are rather too much. Can one of them e.g. the first be replaced by a number / fractional estimate.

Line 76: “between” → “divided between” ?

Line 93, but this has also consequences for the remaining manuscript. Does “trace gases “include OH? If not the description of chemistry must include information on the OH concentration used in the model.
If OH is part of the chemistry. Will the SOA chemistry have noticeable impact on the OH concentration?
Figure 1: No evaporation of SVOC?

Line 121. Are isoprene and monoterpene emitted at ground level or follow the height distribution of the oldSOA.

Line 143: Unclear. “geographical distribution ...” As far as I understand the model reads regridded monthly mean emissions from a specific MEGAN set-up. TM5 does not do anything to the emissions themselves e.g. related to meteorological fields? so it is just how the emission look like from the model (except for the diurnal cycle)

Line 156. Choice of yield looks a bit arbitrary. Tuning ? A sensitivity test might be interesting although information from “initial tests” may be useful. Are the production and results linear with yields?

Line 164. Do you think these assumptions are important for the results.

Line 172: Sometimes the different properties of hygroscopicity is taken into account by a employing a bulk coefficient to the condensation term with an interval of [0 1]
This is not relevant here, or just too uncertain ?

Line 214. Refer back to section for readability

Line 220. Any reference to the entire model system (van Noije et al ?) Given a reference which SOA parameterisation is used in that work

Line 225. Is the resolution the same as in the referenced version of the model.

Line 250. overpresented is likely not the right word

Line 325: Very low dry deposition. This should be the same for sulphate and elemental carbon.?

Line 329 -335. Unclear description, can be made shorter. Can be describe it as a combination of increase (production aloft) and decrease (aitken vs accumulation) ?

Line 346. Ocean production: Any Ricco NPF above the boundary layer (outflow from continents?)

Line 354: No observation of NPF over ocean?

Line 359 Solitary “fig” word

Line 461 Increas → e

Section 3.4.2 Is the reduction in PM2.5 mainly due to reduction of SOA mass or are there other non-linear effects?

Line 501-506: Very unclear.

Line 514-520: Are the results from satellite and Aeronet consitent. I do not expectant large analysis here but nice to point out.?

Line 524. The magnitude of AOD ? → The increase of AOD varies by an order of magnitude of 0.003 to 0.03