

Review for revised version of:

“Multiphase MCM/CAPRAM modeling of formation and processing of secondary aerosol constituents observed at the Mt. Tai summer campaign 2014”

by

Y. Zhu et al.

Submitted to Atmospheric Chemistry and Physics

General Comments:

I thank the authors for their detailed answers about my concerns. However, I have still some remaining concerns on the revised manuscript detailed below.

Overall recommendation:

I recommend that the paper should be accepted for publication in Atmospheric Chemistry and Physics after major and specific revisions listed below.

Major Comments:

I disagree with this statement in author’s response: “MEGAN-MACC includes 21 biogenic species, such as ethane, propane, propene, toluene. However, most of the treated compounds are mainly from anthropogenic sources, especially in China.” For instance, emissions of methanol (acetone) from biogenic sources are of the same order of magnitude than emission of total alcohols (ketones) from anthropogenic sources in the vicinity of Mt Tai for June 2014. This can be verified using the ECCAD database comparing inventories CAMS-GLOB-BIO and CAMS-GLOB-ANT.

Specific Revisions:

Part 2.1: About my comment 13: “Does SPACCIM include aerosols? If yes, it should be specified how: which processes are considered for aerosols: microphysics processes (nucleation, aggregation, sedimentation), chemical aging, nucleation and impaction scavenging by cloud particles? Also the method to represent their size distribution should be indicated. Do you use thermodynamics equilibrium to partition inorganic and organic species between gas phase and particles?” The author did not answer to this point. I knew that exchange of soluble gases between gas phase and liquid phase is considered following Schwartz (1986) in SPACCIM. My point is about the treatment of physical and thermodynamical processes concerning aerosols.

Part 3.1.1: About my comment 18: “Whereas, the text page 6 line 187 indicates that obtained OH and HO₂ gaseous concentrations are compared to available measurements, it is mainly modelling studies that are used for this comparison. Moreover, no details are given on these studies: which model, which conditions (period of simulation, chemical mechanism used for instance). The only observations cited show discrepancies with results, in particular for OH.”, now simulated studies are well detailed but, in my opinion, the sentence (page 7, lines 240-243) should be rewritten. Indeed, I didn’t find that maxima of simulated results are comparable with available measurements, especially for OH. Moreover, you should explain that these results are discussed in comparison to measurements, but also to previous modelling studies.