

# ***Interactive comment on “Modeling Regional Air Quality and Climate: Improving Organic Aerosol and Aerosol Activation Processes in WRF/Chem version 3.7.1” by Khairunnisa Yahya et al.***

## **Anonymous Referee #2**

Received and published: 15 March 2017

The study is extensive, and is suitable for publication in GMD. I particularly like the summary of existing SOA approaches, centred around the VBS. This is a useful addition to the literature for sure. I request the following issues are addressed prior to publication.

### General comments:

Page 4 line 80. I'm not sure the commonly held notion of computationally expensive SOA schemes according to the number of products should persist as a general discussion. Most, if not all, SOA models assume equilibrium absorptive mass partitioning which rests on Newton based methods requiring a small number of iterations. Is there a range of % contributions, for example, that display the relative cost of SOA schemes

versus, say, the gas phase chemistry?

The end of section 1.2 Would it be possible for the authors to comment on what conditions the activation schemes are initialised? Running at higher than 1km, presumably the assumption is to use the aerosol composition, both SOA and SIA, at a specific RH which is then fed into the ARG or FN schemes with regards to hygroscopicity? This might also impact the performance of any given activation scheme if the assumed mass is from a 'dry' SOA partitioning model, whereas SIA accounts for RH dependent partitioning.

Section 3.1: How does the new treatment of semi-volatile POA work with boundary conditions used to initialise simulations?

What is the impact of forcing different VBS profiles into one? I wasn't clear how this relates to, for example, the inputs required for the CCN schemes. Is it related to an inability to track separate sources through the simulation? Or is it related to how the emissions are convolved?

#### Section 4.3

Related to a previous point, the authors comment on how larger differences in CDNC predictions arise from different gas-phase mechanisms over VBS variants. I think it would benefit the reader, and the context of the sensitivity simulations to comment on how the VBS versus RH interactions feed into the CDNC parametrisations.

A more philosophical question, which doesn't require any modifications and isn't a critique of this study: I often wonder how much value we should place on assuming accurate ambient OA/OC measurements without going back to trailing the same model permutations in a controlled environment. Would the authors value smog chamber studies, on mixed VOC systems, using the same parametrisations but in a box-model configuration? It seems that, at least, this would be valuable from a high accuracy measurements perspective.

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Minor comments:

Abstract line 41: 'to 7.1%, it, however'. Please break the sentence here

page 14 line 316-317: 'based on a number of literature', should be 'based on a number of studies in the literature'

page 29, lines 661-662: 'simulated vs, observed' please replace this with 'simulated versus observed'

GMDD

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Interactive  
comment

Interactive comment on Geosci. Model Dev. Discuss., doi:10.5194/gmd-2016-288, 2016.

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