

Interactive comment on “Importance of transboundary transport of biomass burning emissions to regional air quality in Southeast Asia” by B. Aouizerats et al.

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Atmospheric chemistry and physics,

To Reviewer#1,

We appreciate the constructive and helpful comments provided by Reviewer#1, which helped us to improve our manuscript. We have modified to paper to address

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these comments and a detailed reply is given below with referee comments in italics.

Regards,

Dr. Benjamin Aouizerats

Reviewer #1 (Comments to Author):

This study presents model simulation results about tropical biomass burning emissions affect air quality of Singapore. This study applied WRF-Chem model to simulate transport of biomass burning emissions in Jul.-Oct. 2006 and the model results are compared with measured PM10 and CO, also satellite measured AOD. The influence of biomass burning to the air quality of Singapore is also evaluated by turning "on" and "off" biomass burning emission. This work is important as it present how emissions from several hundreds km away affect air quality of a highly populous metropolitan. However, the analysis in the manuscript is weak at this point, I recommend major revision before it can be published in ACP.

1. *"This study compare WRF-Chem simulation with measured PM10 in Singapore, measured CO at a station in Sumatra. They also compared WRF-Chem results with satellite measured AOD, but it is kind of failed. The good agreements of PM10 and CO with measurements at two locations are somewhat convincing. But, they have no aerosol composition measurements at all. Good agreements of PM10 can arise from overestimating one species and underestimating the other species, or arise from overestimating primary emissions and underestimating secondary formation. The authors spent a whole section to discuss aerosol compositions in Section 3. If the authors can not provide some evidence to*

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validate their model, it is hard to believe the results. The sentence (P11228 L21-23) "The comparison of model outputs with observations shows that the WRF-chem model set-up is capable of representing quite accurately the evolution of the aerosol concentration for the 4 months of simulation is just too ambitiously."

We understand and agree with the point raised by Reviewer #1 stating that the good agreement in PM10 comparison does not necessarily lead to a correct representation of the chemical composition of the aerosol particles. We have added a sentence in that direction in order to put things into perspective concerning the comparison of PM10:

"While the PM10 comparison indicated the model was able to reproduce the measurements, we cannot conclusively state that the model managed to reproduce the aerosol chemical composition because no measurement information on the exact aerosol composition was available. However, given our efforts to accurately take into consideration the partitioning of emissions (including various Volatile Organic Compounds) as well as the use of one of the most accurate aerosol/chemistry reaction scheme available at the present time (VBS scheme), the good match between the total aerosol mass concentrations modelled and observed yields some confidence in these results."

2. "The authors also use aerosol compositions data from model to investigate secondary formation in biomass burning plume. Many related important studies are not cited in the paper, including several aircraft BB plume observation data and also laboratory data, such as Vakkari et al., 2014; Yokelson et al. 2009; Akagi et al., 2012; Cubison et al., 2011; Capes et al., 2009; Hennigan et al., 2011. Some of the studies show that OA formation can be very significantly in BB plume. The study of Yokelson et al., 2009 saw very fast (1.4 h) of OA

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enhancement of a factor 2.3 in tropical BB plume evolution in Yucatan, Mexico and the study environment is highly relevant in this study. This is contrast with the authors model results. Given that SOA is usually underestimated in models and very low SOC/POC ratio in this study, I would recommend the authors work more on this issue."

We thank Reviewer #1 for pointing out this issue. we have added a short discussion on thus using the suggested literature in the manuscript:

"The results in this study show a significantly lower SOA/POA ratio in the plume than the ratio reported by several studies mainly focused over northern America (Vakkari et al., 2014; Yokelson et al. 2009; Akagi et al., 2012; Cubison et al., 2011; Capes et al., 2009; Hennigan et al., 2011.). This difference may be attributed to several reasons. First, it is well known that due to the complexity involved in the chemical reactions, almost every numerical model tend to underestimate the secondary aerosol formation (Seinfeld and Pandis, 2006). However, it is more likely that the large difference between the SOA/POA ratios reported in the previously mentioned studies and the ones presented in this work are due to the very large concentrations of primary particles emitted by peatland fires. Indeed, the fact that Indonesia has the highest density of fire emissions leads to very large emissions of both primary particles and precursory gases responsible for the formation of secondary organic aerosols. However, the formation of secondary organic aerosols is a strongly non-linear process which depends on numerous and complex processes (such has the VOC concentrations, ozone concentrations, NO_x concentrations, water vapor, aerosol internal mixing rate, etc.) (Seinfeld and Pandis, 2006; Ng et al., 2007). Therefore its formation can quickly reach its saturation mixing ratio or a threshold due to a limiting factor. In our case we believe that the partitioning between the vapor and aerosol phase has quickly reached a saturation point due to the NO_x and ozone conditions, and despite the fact the VOC needed for the formation of SOA are still abundant."

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3. *"P11226 L13: How PM10 and CO are measured. How many sites do you have PM 10 data. Are they urban sites? Please provide the information."*

We have added information concerning the measurements. As previously mentioned in the manuscript, the PM10 data are averaged from 5 stations located over Singapore. We have added the urban qualification for more clarity.

4. *"P11247: Fig. 6 Please provide more explicit x-axis in the figure, e.g. latitude."*

We have added the distance in the transect caption as the latitude and longitude are not linear within the transect.

5. *"What is the different between POA and OCp. Please use a consistent terminology in the paper."*

We thank Reviewer #1 for pointing out this issue. The terminology has been corrected throughout the manuscript to be more consistent.