



Interactive  
Comment

## ***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 J. Reid,

We appreciate the constructive and helpful comments provided by J. Reid, which helped us to improve our manuscript. We have addressed some of his concerns. We

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have spend considerable time to extend the spatial domain of the study but failed to do this within the allocated time due to technical restrictions. However, we have partly addressed the concerns by adding HYPLIT model results as well as adding more perspectives on uncertainties. A detailed response is given below with the referee comments in italics.

Regards,

Dr. Benjamin Aouizerats

*J. Reid (Comments to Author):*

*"We have a few comments on this paper, which as an outside poster the authors can take or leave. I held off on sending these in as I was waiting for the official reviewers. But as their comments have not come in, we thought we better jot these down. Our group has performed substantial research on the observability and predictability of atmospheric constituents in the region and have some input which we hope the authors find useful. The topic that they address is an important one, with significant scientific as well as political implications and is certainly suitable for ACP. The inclusion of anthropogenic emission simultaneously with burning does make it distinct from other studies and is a useful contribution to the community. However, there appear to be problems with the analysis presented in the manuscript that the authors need to take into account."*

1. *"The title gives a bit of a false impression. Really this paper is a case study on the 2006 burning season and its impact on Singapore. The title "Importance of transbound ary transport of biomass burning emissions to regional air quality in*

*Southeast Asia" implies an effort much bigger than what is presented. If they are looking at the partition between anthropogenic pollution and biomass burning in Sumatra for the biggest biomass burning event of the EOS era, they should simply say that"*

While we understand the concerns about the title not pointing that this work is based on a case study, it does reflect the main purpose of this work: a better description of the interactions between biomass burning and anthropogenic emissions at regional scale. We have changed the title to: "Importance of transboundary transport of biomass burning emissions to regional air quality in Southeast Asia during a high fire event" to indicate our work is based on a case study.

2. *"Their domain is Sumatra and the Malay Peninsula. Borneo is absent, as is Java. We dont agree at their supposition that these islands can be ignored. From Wang et al., 2013 (cited in the paper), transport across the Java Sea from Borneo is clearly occurring-just look at the satellite images."*

While satellite images may show transport occurring from Borneo to Singapore, such analyses provide mostly information about the column concentrations and do not necessarily yield information on what is going on at the surface in which we are interested mostly. Although we were not able to rerun the model with an extended domain including Borneo, we provided supplementary material with the HYSPLIT dispersion model results including deposition process which clearly shows that the 3-D dispersion of mass emitted in Borneo does not reach Singapore at the surface level during our period of interest. Clearly the concern of Reid is valid and we have added a section in the manuscript discussing the origin of the air mass reaching Singapore during the second half of October.

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3. *"Their statement that easterly winds for the Oct 2006 were light and variable is at odds with the Singapore RAOB site ([http://weather.uwyo.edu/...](http://weather.uwyo.edu/)) which shows consistent easterly PBL winds of 5-10 knots. Surface winds alone are not an adequate representation of regional transport. Further, based on the analysis of Atwood et al., (2013) there is likely a reservoir of smoke aloft being entrained into the PBL-something that models often represent poorly. Similarly, there have been many who have hypothesized (including the co-authors) that Jakarta is an important source for Singapore. Thus, I think there needs to be discussion on this point. A general Printer-friendly Version analysis describing the meteorology of Borneo and Java transport can be found in Reid et al., 2012, Atwood et al., 2013, and Xian et al., 2013."*

Concerning the wind fields, we had compared the WRF-Chem results with observations from the National university of Singapore (<https://inetapps.nus.edu.sg/fas/geog/ajxdirList.aspx>) which show for the month of October 5-meter winds oscillating from 0 to 5 m.s<sup>-1</sup> with associated directions from NE to SSW. This is in good agreement with our model results mentioned in the manuscript.

We agree that there is the possibility of the presence of an elevated residual layer of aerosols which may act as a reservoir for the PBL. However, we believe that WRF-Chem under the configuration we used is one of the most accurate models available to reproduce the reality as closely as possible.

Concerning the contribution of Jakarta to the PM levels in Singapore, we understand and agree that under certain circumstances, it can be an important source. In this study, we focused on the transport from Sumatra to Singapore, and while Java is not in the domain (just as Borneo), their respective contribution to the total PM concentration is still accounted for by including PM and gases injections from the boundaries of the domain based on MOZART model reanalyses. It is considerably lower than the contribution from fires in Sumatra during that time

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period.

4. *"This then leads to the verification data being a bit at odds with a simple review of satellite imagery. Their simulations suggest no fire influence just when we expect Borneo influence to be most important (Fig. 7)."*

Indeed, while most studies focusing on this region using on satellite data tend to attribute the total contribution of PM level in Singapore to fires, the goal of this study is also to point out the complexity of modelling aerosol particle evolution (transport as well as physical and chemical processes) and we think that high-resolution models are needed to accurately represent the processes involved and solve them online. When we interpreted our results, we were equally surprised that the coinciding poor air quality in Singapore and heavy burning in Kalimantan during the second half of October may have actually just been coincidence. But based on the good correlation of PM10 values between our model and measurements as well as the HYSPLIT results in the supplementary material we can be relatively confident in our conclusions.

5. *"Regarding verification data with satellite AOT data, we would like to point out the analysis in Reid et al., 2013, which clearly demonstrates that satellite AOT products underestimate true AOT values. Thus, the difference is even bigger than reported. This is due to two things. First, high AOTs are often flagged as cloud. Second, for moderate values of AOT, the assumption of single scattering albedo is far too high. While I appreciate there is little verification data out there for the 2006 event, I think the fact that the AOTs for some of the most significant events could be underestimated by more than 50% or more should be noted. Even though as the authors note that AOT is not a criterion pollutant, the fact that they have good results at the surface yet cant constrain total mass loadings has implications for the source function and the transport"*

We have added some discussion in the revised manuscript noting the uncertainties in the AOT values and that the AOT values may be underestimated, "Moreover, as shown by Reid et al. (2013), AOD measurements in this region are often underestimated by up to 50%". We agree that comparing model results only at the surface is not sufficient to fully validate the source function as well as the transport (and the aerosol physics-chemistry) of PM. We know our study is not the last word on this but we feel we have made progress by comparing to both surface CO and PM10 and column AOD values. Future research is needed to reconcile why the surface observations were matched reasonably well but the AOD not. To date, most studies focusing on only AOD indicated GFED-type bottom-up emissions were too low. We show this may be cutting corners; if we had done this (that way matching AOD) we would have overestimated the surface measurements. Clearly, reconciling this mismatch is an important research area.

6. *"For their source function, the authors should review Hyer et al., 2013 (as well as the commentary in Reid et al., 2009 and 2013). The fact of the matter is we don't know source functions to better than integer factors. Hyer showed that the GFED method favors larger fires. With all methods, we know there are countless small fires undetected by either burn scar or thermal anomaly. This should at least be mentioned."*

The manuscript has been updated to show some perspectives on the fire detection, and the method used by GFED3 to correct the omission of small fires. Moreover, we have compared the results from GFED3 (which includes a "boost" based on fire persistence to account for omitting fires in deforestation areas) and GFED4 where small fires are more accurately accounted for and for our region and time of study: [http://www.globalfiredata.org/\\_plots/timeseries\\_0.pdf](http://www.globalfiredata.org/_plots/timeseries_0.pdf). It appears that the differences are relatively small.

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7. *"Bottom line for us is that it is good to get the industrial pollution into the picture, and the authors can do everyone a great service by generating a solid simulation which they and regional researchers can mine. Qualitative verification of the model outputs against the observed meteorology is also a necessary step. Some discussion of satellite and model uncertainties needs to be incorporated in the study. I would strongly suggest rerunning with a larger domain. You will be glad you did (as will the community). That larger domain could then be used to apply this analysis for all of the major cities in the region, not just Singapore."*

We had hoped to update our results with a larger domain. Unfortunately we did not succeed in this within the allotted time but were strengthened in our conclusions thanks to the HYSPLIT results. We have validated our results with multiple sources of information (trace gases, PM10 measurement, AOD, and a quantitative comparison against meteorology) and have added more discussion on uncertainties. Clearly, more can and will be done in the future and our results are, just like most other studies, just a step in the -hopefully- right direction.

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