Atmos. Chem. Phys. Discuss., 14, C4609–C4611, 2014
www.atmos-chem-phys-discuss.net/14/C4609/2014/

Atmospheric Chemistry and Physics Discussions

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ACPD

14, C4609–C4611, 2014

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.

J. Reid

reidj@nrlmry.navy.mil

Received and published: 8 July 2014

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 transboundary 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.

2) Their domain is Sumatra and the Malay Peninsula. Borneo is absent, as is Java. We don’t 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.

3) Their statement that easterly winds for the Oct 2006 were light and variable is at odds with the Signapore RAOB site (http://weather.uwyo.edu/cgi-bin/sounding?region=seasia&TYPE=GIF%3ASTUVE700&YEAR=2006&MONTH=10&FROM=1500&TO=2500&STNM=48 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 (inluding the co-authors) that Jakarta is an important source for Singapore. Thus, I think there needs to be discussion on this point. A general 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.

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). See http://modis-

atmos.gsfc.nasa.gov/IMAGES/index_mod021km.html for this period

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.

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.

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. Best Regards, Jeffrey S. Reid & Edward J. Hyer, US Naval Research Laboratory

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 11221, 2014.