

## Review of Hansen et al. for Atmospheric Chemistry and Physics

### **General Comments**

In “Haze in Singapore - Source Attribution of Biomass Burning from Southeast Asia”, Hansen et al. use the NAME Lagrangian model to apportion modeled concentrations of a tracer from biomass burning emissions in Singapore to source locations. The modeled concentrations depended on meteorology, dry and wet deposition, and daily average emissions rates for biomass burning from the Global Fire Assimilation System (GFAS) v. 1.2. The tracer concentrations were compared with observed concentrations of particulate matter less than 10  $\mu\text{m}$  in diameter ( $\text{PM}_{10}$ ) at two sites on either side of Singapore. Other contributions to  $\text{PM}_{10}$  and any chemical transformation of the emissions from the fires were neglected. The literature review is helpful. NAME and the underlying meteorology have been used to study similar questions for specific episodes in the region.

The major difficulty in this work seems to be representing  $\text{PM}_{10}$  concentrations in Singapore except when biomass burning occurs very close by the sites (e.g., Riau or the Malaysian Peninsula) (Fig. 4). The two times this occurred during the modeled period are labeled as “Atypical haze” (Sect. 3.1). As the authors note, the  $\text{PM}_{10}$  concentrations in Singapore, especially in the other “haze seasons”, are likely from diverse sources; however, the time series (Fig. 4) and statistical analysis of the agreement between the model and observations (Tables 1, 2) do not support the notion that “Southeast monsoon season haze” (Sect. 3.2) is represented accurately enough by the model to claim attribution of these sources to biomass burning regions. The difficulty may result from the lines of source regions not being stacked in the time series plot (Fig. 4), but the correlation statistics are fairly poor for periods other than 2013 MJJ, which supports the interpretation of the time series that the model is not representing concentrations well. One reason may be that the  $\text{PM}_{10}$  from biomass burning from regions not adjacent to Singapore is not well represented by the modeled processes. Another reason may be, as noted by the authors, that much of the haze comes from biomass burning sources not detected by the model. Finally, it may be that the  $\text{PM}_{10}$  is not from biomass burning. The first two causes would indicate that this modeling framework is not appropriate for attribution of biomass burning contributions to  $\text{PM}_{10}$  concentrations in Singapore. The final potential explanation could be shown by more sophisticated analysis of the background  $\text{PM}_{10}$  concentrations rather than using a fixed value of 25  $\mu\text{g}/\text{m}^3$ ; if this explanation does not hold, it seems unreasonable to claim that these attributions are appropriate for episodes other than those in which the Riau and Malaysian Peninsula contributions dominate.

Given the difficulty of interpreting the results, the weakness of the Discussion and Conclusions sections seem reasonable. Specifically, the Conclusions seem to be very repetitive of the Discussion. Accordingly, I recommend this manuscript for publication in Atmospheric Chemistry and Physics once this major issue has been addressed. Additionally, specific comments have been included below that should also be considered in the revisions of the manuscript.

### **Specific Comments**

<b>Line</b>	<b>Comment</b>
p. 1, l. 8-13	The meaning of this interpretation of the results is unclear especially in the phrases “several and varying source regions”, “atypical haze episodes ... characterized by atypical weather conditions”, and “different set of five regions dominate”. Please refine.

- p. 1, l. 14 "climate" seems inappropriate when only 5 years have been considered.
- p. 2, l. 3 "Though the popular press often attribute" - grammatical error. Also, scientific literature has supported similar conclusions (Kim et al., 2015).
- p. 2, l. 17-9 Please insert a comma as "approach, and source" or divide these two thoughts into separate sentences. What type of source apportionment was applied by Lee et al. (2017) and Engling et al. (2014)? Please be more specific.
- p. 2, l. 20-2 Run-on sentence. Please correct here and throughout the manuscript (e.g., p.3, l.8-12, etc.)
- p. 2, l. 26-7 Poor sentence construction leads to a lack of clarity.
- p. 3, l. 13-4 "related haze events are linked" is redundant.
- p. 3, l. 23-4 A strong case for using dispersion modeling has not been made in the Introduction.
- p. 4, l. 31 Please provide a reference or equation for the MNMB.
- p. 4, l. 35 Please clarify here, as is stated later, that the emissions are emitted over a 24-hour period at the rate of 1 g/s. Please state how the same emissions rate results in different total emissions from a single fire (if it does).
- p. 5, l. 7 Please note the limitations of comparing a tracer with PM<sub>10</sub>.
- p. 6, l. 4-9 Please include an equation for the calculation described here. It is not clear how double counting in time is avoided given this description.
- p. 16, l. 1-2 Was it expected that the "atypical and different meteorological conditions" would cause variation of the source regions when those were dominated by nearby fires? It seems unlikely, so the sentence is unexpected.
- p. 19, l. 6 "Similarly to" should be "Similar to".
- p. 19, l. 15-7 Two statements contradict one another. The atypical haze events are said to have little variation between monitors, but then FMA 2014, one of the atypical haze events, is noted as having the largest difference in the next sentence. Please restate.
- p. 20, l. 3 Please eliminate the use of contractions here ("won't") and elsewhere.
- p. 20, l. 5-6 No effort was made to show data that support this conclusion. Please show that it is true as suggested in the General Comments or remove the sentence.
- p. 20, l. 9 The grammar and sentence construction in the Conclusions section of the document require careful revision.

- Table 1 Please include the meaning of the abbreviations for the statistical correlations in the caption.
- Figure 2 The caption states “air history” but the colorbar label indicates “Air Conc Percentile”, which seems to include information about emissions or concentrations rather than simply where the air has been. Please clarify.
- Figure 4 Please specify the meaning of the colored values. Are the lines for the source regions stacked? If not, they should be in order to show how they contribute to the total observed concentration.
- Figures 6-8 The design of these figures is nice. It is not clear why only some of the contributing regions are colored. Please be consistent between the “other”, which are grey in the legend, and the grey countries in the map. Also, please order the pie chart wedges in the same order as the legend names. The colors are too similar to be able to distinguish when the pie chart is not in the same order as the regions in the legend.

Kim, P. S., Jacob, D. J., Mickley, L. J., Koplitz, S. N., Marlier, M. E., DeFries, R. S., Myers, S. S., Chew, B. and Mao, Y. H.: Sensitivity of population smoke exposure to fire locations in Equatorial Asia, *Atmos Environ*, 102, 11–17, doi:10.1016/j.atmosenv.2014.09.045 , 2015.