

**Review of “Haze in Singapore - Source Attribution of Biomass Burning from Southeast Asia”
by Hansen et al.**

This study investigates the sources of biomass burning from Southeast Asia for 6 years (2010-2015) using the Lagrangian dispersion model NAME. The tracer concentrations were evaluated using observations at two sites in Singapore. The authors also discussed the seasonal variations of emissions sources to Singapore. The topic of this paper is very interesting and important and I really appreciate the seasonal focus in this study. However, the conclusion and discussion are very confusing and repetitive. The paper lacks a coherent flow and the method section lacks significant information and clarity. I would only recommend this manuscript for publications in ACP only after substantial modifications to the manuscripts and figures. I also suggest the authors make higher quality figures for publication in ACP (Figs1, 4, ...). In general, it is difficult to interpret the results and the discussion is weak.

1- **Meteorology:**

Dispersion models are highly sensitive to their meteorological inputs. However, there is no analysis of the metrological values fed into the model. First, there is not clear statistical analysis or comparisons between modeled meteorology from UM and observations in that region. Without first evaluating the meteorological input, we cannot draw any conclusion from the Lagrangian models. For example, slight errors in modeled wind speed (and direction) and observations, makes the originating source region of the tracer very different.

Second, P4:L1-2 mentions that the metrology runs were different (resolution and settings?) for different years. Based on previous studies, the modeled meteorological values (especially wind speed) are sensitive to the model resolution. Considering that in this manuscript, the authors compared different years with each other, I strongly recommend use of consistent settings and resolutions for the NWP runs. Else the inter-annual difference between the sources of biomass burning can easily be attributed to the difference between meteorological model differences.

- 2- In general, there are large discrepancies between modeled and observed PM10 for all years and both stations. The authors assumed a constant 25 ug/m³ background concentration for all year. However, the emissions from various sectors (especially residential) have high seasonal variability (see Sobhani et al. 2018). Considering the same background concentration (meaning the same contribution of other sectors to your PM10) for all seasons may introduce large errors to your analysis.

Specific Comments:

Introduction:

- 1- This part lacking significant discussions and references. For example, the reference this part of P2, L2 is missing, “it is not caused by activities within Singapore, rather it is a transboundary problem caused by biomass burning across the wider region.” Maybe adding sample studies.
- 2- P2, L13: I would recommend adding more discussions here. I suggest citing and/or describing some source attribution studies with the focus on other regions or bigger domains using different methods (Eulerian, Lagrangian, Observation analysis). I am not sure why the very few studies in the next lines are cited. Few examples for source appointment in different region of the world (with Eulerian methods) are: (Ikeda et al., 2017; Sobhani et al., 2018; Wang et al., 2011; Yang et al., 2017), With both Eulerian and Lagrangian methods:(Kulkarni et al., 2015) Observation Analysis + Lagrangian: (Winiger et al., 2017).
- 3- P2, L 22: Any reference for this sentence or is it the result of the study? If it is this result of the study please mention so.
- 4- P2, L34 and P3, L3: Please add a reference for each sentence.
- 5- In general, I suggest restructuring this section a bit for cohesiveness by moving few first sentences of the 5th paragraph (P2, L28) in introduction before the 4th paragraph (P2, L20). It is not clear if some the sentences in the 4th paragraph are result of this study or previous studies.

- 6- It is not obvious why the focus of study is Singapore. Can you please add why the focus of this study Singapore?

Methods:

- 1- This section lacks a lot of details. Can you please add some information and a paragraph describing the NAME model? How are they numerically represented in the model? What kind of aerosol processes are accounted for? Are there any known biases? Why have you used this model for this study?
- 2- Can you please add some information and more description on the modeling setup for this study instead of just citing Hertwig et al. 2015. How are the wet and dry deposition processes calculated in the model?
- 3- Also, can you please describe your meteorological model (UM) and why this model is used to drive NAME?
- 4- Significant lack of clarity and explanations regarding observations: It is not clear at all where the locations of the observation sites are (maybe add them to all maps and include lat lon of the measurement sites)? The authors should add more information on the method of measurements in those locations. Also, it is not clear where these measurements come from (paper?, organization?)? Also, is this data available for public if so please include the link to the data either here or in the code and data availability section of the paper (or both).
- 5- Air history map?? Do you mean PM10 or all aerosols (air?) lumped together? What chemical species and aerosols are considered in air history maps? Or is it only PM10 or tracer? This term is very confusing.

Results:

- 1- The assumption of 25 ug/m³ for both stations is problematic. Could it be because the background value from another source is higher in the western station?? Also emissions from other important sectors are not accounted for which might cause the difference between the stations.

- 2- P14, L13: Can you add some figure (maybe to SM) to show the meteorology difference for 2013 and other years. In general, this sentence is vague. What do you mean by 2013 is a unique year in terms of meteorology and burning?
- 3- Are peaks concurrent with biomass burning incidents? Several other factors influence the peaks. For example, high residential emissions in winter in South East Asia can be attributed to the peaks.
- 4- There is a large redundancy between results (section 3) and discussions (section 4). I suggest merging section 4 into section 3 and conclusions.
- 5- P 15, L30: It seems like the model did not capture the observation contrary to the claim.
- 6- P16, L10: The model significantly underestimates the peaks (30/125) Please explain why?

Figures:

- 1- Does Figure 1 show the entire domain? It seems smaller than the domain mentioned in the method section. Please correct the figure to include all the domain in this figure.
- 2- Please add the location of Singapore to Figure 1 and all other spatial figures. It is hard for someone who does not know the region's geography to find Singapore in each figure. Based on the caption's description, the reader might think Singapore is located in the Riau Islands.
- 3- Figure 2: Wrong caption. The second line of the caption of this figure is not related to the manuscript. Central receptor sites???? Inland and coastal sites? Are these sites discussed in this manuscript??
- 4- Figure 2: Please correct the label title.
- 5- Figure 2: Can you please add more discussions about this figure to the paper? It is confusing what these figures show.
- 6- Figure 2: It is very nice that you included figure A2 (Figure 2 for all years to the discussion). I suggest also adding similar plots for each season. (each season averaged over the years). The season-specific "Air history maps" would make it easier to understand the transport pathway in different seasons as discussed in P6, L10.
- 7- Figure 3: This is a good plot; however, it is difficult to compare different years because of the different scales. Also, the y-axis label denotes T as the unit for monthly emission, which is different from the caption.
- 8- Figure 4: This figure is very hard to read and should be modified before publication. First, it seems like hourly observations are plotted against (daily averages of model??). It is very

hard to distinguish any modeled data points. Please make different plots for this figure. One way is showing monthly averages for both model and observation similar to Figure 3. Or time series of the daily observations as points overlaid on top of the modeled output. I recommend area chart for modeled value. Please include sum multiple region as “the other regions” multiple of the regions in this plot. Please only include important regions with visible high impacts. Very few of the 28 regions are visible in this plot. Maybe another scale (e.g. a log scale) is better for the purpose of this plot. Please use the same scale for all years and denote the events discuss on these plots.

The scale for all the years vary significantly. I would suggest having all PM 10 for all years on the same scale 0-700. Quick look at the plots, one might think there are higher pm 10 concentrations in 2010 compared to 2013 or 2015.

- 9- Figures 5-8: Please add a title with the name of event to the plot. Please add the stations (locations of NTU and TP) and denote Singapore on the plots.
- 10- Figure A2: What are the colored squares overlaid on the plots? Please put the figure in order that is mentioned in the paper.

Minor Comments and Technical Corrections:

- 7- P2, L3: Fig 1 is technically not related to this sentence.
- 8- P2, L3: I would recommend adding reference for the second part of this sentence. (the reference for transboundary problem...)
- 9- P2, L25: This sentence is very vague. Two events in each of June 2014-2015 and FMA 2014-2015. Or one event in June (2014 or 2015?) and one in FMA (2015?). Also, why using FMA vs June. I would recommend either using month or season. Can you use the season instead of June for consistency?
- 10- P2, L 33: Can you please elaborate what you mean by north-east monsoon and south-west monsoon seasons?
- 11- P3, L 9: FMA acronym were explained last page and redundant here.
- 12- P3, L30: What does NAME stands for?
- 13- P3, L32: I recommend adding a figure to SM with the modeling domain. It seems like figure 1 does not show the complete modeling domain. (not extended 14 S or 23 N)

- 14- P4, L1-2: What do you mean? Is it different meteorological setup for each year??? Is the resolution of NWP runs different from 17 km to 40 km??
- 15- P4, L 14: Please add what GFED stands for.
- 16- P4, L 16: Redundant, very similar sentence in the above paragraph L10....
- 17- P4, L24-25: Can you please point to the pie charts?
- 18- P4- L30-35: Can you please explain why did you use these metrics instead of other metrics like R2 or RMSE and many others? I suggest adding few more metrics to the tables 1 and 2 (R2 and RMSE). Please provide references or descriptions of the metrics used.
- 19- P4, L35: I strongly suggest using daily averages instead of hourly averages.
- 20- P5, L7: Please add what NWP stands for.
- 21- P5-6: Air history vs air conc. percentile? Please clarify?
- 22- P7, L20: I would highly suggest including emission maps for each year.
- 23- P7, L29: What is the reference for this sentence?
- 24- P7, L32: Why did you assume constant 25 ug/m³ for background concentration? Is there any reference for that?
- 25- P8, L3-5: I suggest including the values for clarity and readability.
- 26- P9, L4: Please add the name of the western monitoring station here and throughout the manuscript
- 27- P14, L15-16: A sentence without a paragraph. remove the unnecessary line break.
- 28- P14, L 16-18: This sentence is very confusing. Please rephrase it.
- 29- P14: In general, adding the locations of the sites to the maps would make reading the paper much easier.
- 30- P14, L24: It is not obvious if the maximum observed and modeled are concurrent or the values indicate maximum observation and maximum modeled value occurring at different times?
- 31- P15, L25: Would you discuss FMA 2014 or February 2014 only. Earlier in the text you mentioned June 2013 and February 2014 as the haze events but discuss FMA 2014 as the haze event here. In general, there is a lack of consistency between using months and seasons.
- 32- P15, L30: Can you explain the reason why concentrations at TP is double of NTU?

- 33- P16, L1-2: Very unclear and vague sentence. Different meteorology for events or between the monitoring stations? The sentence implies that in spite of the clear dominance of one source region, there is a little variation in the source regions across the monitoring stations???
- 34- P16, L3: I suggest adding ASO to the title of this section. The inconsistency between using southwest monsoon haze and ASO makes it confusing.
- 35- P16, L4: Please remove the extra line break.
- 36- In general, not clear when the events are. I would highly suggest making a table including all the events discussed (and their corresponding figure) and also denoting each event on the time series plot.
- 37- P 19, L1-3: This sentence is extremely confusing. For ASO or for the seasons with the most significant haze events including MJJ, FMA, and ASO?
- 38- Please check the sentence constructions of discussions and conclusions sections carefully.
- 39- P19- 20: The discussions seems like an extended conclusion section and there is a lot of redundancy between results (section3), discussions (section 4) and conclusions (section 5), which decrease the readability of paper.
- 40- P20, L30: Please restate this sentence. It is very confusing.
- 41- Code and data availability: Please include the link to the observations used for this study.

References

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