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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Biomass Burning Aerosols and the Low Visibility Events in Southeast Asia" *by* Hsiang-He Lee et al.

Anonymous Referee #2

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General Comments:

The paper provides a correlation of modelled particulate matter with low visibility days recorded at observation sites across South East Asia. Information is presented about the most likely source areas for biomass burning pollution for different cities and different seasons.

This is an interesting application of an alternative observation dataset for assessing the impact of biomass burning haze on the region and for validating CTM and dispersion models. However, the significant flaw in the way the results are presented is that the model is assumed to be correct and that all low visibility days that are not modelled are therefore specified to be due to other pollution contributions. The validity of this assumption is not demonstrated. It is quite possible that the model is over-estimating the biomass contribution at some sites and underestimating it at others. Fig 6 for example would suggest that the model may not be capturing up to 50% of the fire haze days, and Fig 4 would suggest that the model misses 50% of the VLVDs at Singapore. The references in the text to fire and non-fire LVD are therefore misleading. The authors need to reconsider how they interpret this data and present it in the paper.

The paper would benefit from some reorganisation of the sections and a reduction in the number of figures.

Specific Comments:

Following on from the general comments, I am concerned that no real attempt at model validation is made within this paper. An additional source of observed data, e.g. PM10 concentrations, from a minimum of one of the sites (ideally many more) is needed to demonstrate that the WRF-Chem simulations are correctly capturing the fire component. The data shown in Fig 5(a) is misleading due to the use of different scales and a more robust analysis of this data is needed earlier in the paper. In fact this data may reveal useful information about missing "background" PM from the model. There are statements on line 320 that the model is underestimating PM2.5 concentration by up to 30-50% in this comparison. This is a significant underestimation. What impact does this then have on the visibility and hence the LVD calculations? The authors also need to discuss in more detail the impacts of uncertainty on the LVD and VLVD estimates. Without this level of validation, the model results cannot be used to the level of precision that the authors present in e.g. Table 2.

I would also like to see some explanation as to why the modelled visibility distance for Bangkok in Fig 4 is significantly lower than that in the observations (and in comparison to the difference at other sites), and consequently what this means for the calculation of VLVDs.

The decision that the "other pollution contribution %" is "100% minus Fire pollution contribution %" is not appropriate for the analysis that is then presented. Statements

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such as those on line 336-338 and line 345-347 do not hold up. The authors need to present a justification for why the reader should assume that the model data is correct. Even so, all interpretation of non-fire LVD should probably be removed.

To aid the discussion of the changing number of LVDs further explanation of certain statements is needed. For example, Line 366-368, why is Kuching different to Singapore? Could this be because Kuching is within a fire area?

More information and explanation on the model set-up and analysis approach are needed to help the reader understand what has been done. Including (a) in section 2, further explanation about the "chemistry tracer module" is required – is there any chemistry at all? It doesn't appear so, so this is a bit misleading. It would be better to say "chemical tracer module" and be clear that the pollutants are being modelled as tracers only. The lines on p8 (163-164) describing the deposition processes could usefully be moved to this earlier point in the text. An explanation for why the domain extends so far west would also be helpful. (b) p9 line 180 – the authors need to clarify whether emissions have been injected at just 700 m or from the surface to 700 m. Is this asl or agl? (c) More detail (ideally the equations used) is needed as to how the hydroscopic growth is calculated on p11 line 232 and how this relates to the visibility calculation. Also where has the environmental relative humidity data that is used come from? This is fundamental part of the model data processing, and will introduced it's own uncertainties, but is rushed over (d) There is currently no information on how the model output has been produced for each site, so this needs to be added. For example, is it based on the modelled concentration in the lowest WRF-Chem layer for the grid box corresponding to each observation site? (e) A brief explanation as to how the runs have been conducted to identify the different source sectors is needed. Did these use labelled tracers?

The use of two different time periods for the analysis of the results for the FINN data vs the GFED data introduces differences in the outputs, which could be misinterpreted. It makes Table 3 particularly complicated to interpret. I would recommend that through-

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Discussion paper

out the paper the authors only present data for the same period for all 3 model simulations (i.e. 2003-2014) to avoid introducing additional uncertainty and confusion in their results and analysis.

I would also recommend that Table 3 is modified to present the total number of days in the 12 year period rather than an annual average, as the latter significantly distorts the true year to year variability and introduces false precision.

The language needs some improvement particularly in the abstract and the introduction. The use of “particulate matters” rather than “matter” is somewhat unconventional.

The discussion of the role of precipitation jumps around the sections, so the authors are encouraged to see if this could be pulled together into one, shorter overview section. Some of the text regarding the precipitation in section 2.4 needs further explanation. For example on line 275 more detail and/or a citation is needed for the FDDA grid nudging. The use of mean monthly rainfall to compare the models and observations (lines 269-274) seems strange given that the authors have nicely demonstrated the large annual variation in rainfall timing and magnitude across the region. It would be useful to explore whether the models are better in some seasons than others in this region? On Line 281 the authors mention the temporal correlation, but also need to state over what averaging period this is, e.g. is this based on daily, weekly, monthly mean or total ppt data? Figure 3 is particularly hard to interpret. Difference plots would be more useful here, but this figure is a candidate for removal.

Section 4 would benefit from a broader discussion of the NWP datasets, for example there is currently no discussion of the wind fields, which are of higher order relevance than the precipitation, particularly for the source area identification. I also find it slightly surprising that given that the LBCs are a long way from Sumatra that WRF develops such a discrepancy in precipitation over the central region of the domain in the different runs. Is there a similar difference in the winds, which would therefore impact the transport? Has any verification of the WRF wind data been conducted? This section

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would benefit from being merged with the other sections on meteorology.

The attempt by the authors to use the data to assess the impact of the haze on populations in SE Asia is to be commended, but the approach taken is needlessly complicated. The units of the HED metrics are unclear and the dominance of population size on the HEDpw metric needs more careful explanation. What the results are showing are that the total number of LVDs in the region (based on observations at 50 cities) has increased over the analysis period. This conclusion could be reached without the HED and is easier to explain and understand for the reader. As explained previously the statements in this section about non-fire pollution are not justified by the approach.

The manuscript would benefit from fewer figures and I am not sure the supplementary material adds anything. The line thickness in many of the line graphs means that the bottom lines are often hidden, this is always a problem with this sort of graph, but a reduction in the line thickness would be beneficial.

Technical Corrections P2 line 45 – 99.1% is over stating the precision here. I would suggest using only 99% which is in line with the precision of other numbers given in the abstract

P4 line 66-73 – The discussion of radiative impact isn't relevant to the rest of this work, so seems unnecessary. Recommend deleting these lines.

Line 325-327 – it would be more helpful to the reader if these percentages were expressed as a number of days. The language at the end of this sentence could also be improved

Line 237 – Is the total population figure here correct? It is not clear if this the combined total, or if each city has more than 2 million?

Table 2 – The table would benefit from explanation that the VLD and VLVD for FNL_FINN and ERA_FINN are identical as they are based on observations, and that the data for FNL_GFED is different as it covers a shorter time period. However see

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comments regarding making the time period consistent.

Table 2 - The FNL_FINN LVD line for Singapore does not add up to 100%.

In Table 3, the caption states that "parentheses show the fire aerosol fraction in total PM2.5" – this is very unclear and confusing. It could be taken to imply that the model also contains non-fire PM2.5, but I don't think this is the case. I think the table would be more informative and cleaner if all of the parentheses data were removed.

Figure 2 – it would be useful to highlight in the caption that all of the plots have different axes scales.

Figure 5 – the use of different axis scales in (a) is very misleading. Both data sets should be presented with the same scale and starting from 0. Where is the data that gives the green areas from? This data could usefully contribute to the discussion in the text and the validation of the model.

Figure 6 – A better way to present this data would be to have the green data as the GSOD observed LVDs and the red data as the modelled fire LVDs. This would be a more robust comparison of model vs observations and start to address issues in the comments above.

Figure 7 – the S1 and S5 line colours are too similar in my copy, so can one of these be changed please.

Figure 9 – Need to specify that these are "fire" concentrations in the caption. In this and Fig 10, the purple contours on the right hand plots prevent the underlying colours from being seen and are so small that they are unreadable, so recommend that these are removed.

Figure 11 – To ensure that there is no unintentional bias, the plot would be better if it depicted data for only 2003-2014 for all of the data sources.

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