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Interactive Comment

Interactive comment on "Injection heights of springtime biomass burning plumes over the Peninsular Southeast Asia and their impacts on pollutant long-range transport" *by* Y. Jian and T.-M. Fu

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Review

This well written paper addresses the injection height of biomass burning emissions over Peninsular Southeast Asia. A model study is conducted to assess the impact of emission height on the large range transport and chemistry. Validation is performed with aircraft data measured in 2001 during TRACE-P.

Although the paper reads easily, I have some reservations about the approach.

A first point that is not questioned by the authors is the fact that the overpass time MISR and MODIS is 10:30 a.m.. How representative is this time for fire activity and the associated vertical rise of the biomass plumes? How could the situation change in the afternoon, when extra surface heating may have activated fires and convective activity?

On page 23788 it is written that vertical mixing in the boundary layer was assumed to be instantaneous. I assume this is within the planetary boundary layer. What is the reason then to have 8 levels in the lowest 1 km. Moreover, I wonder what is the impact of this assumption on the results. Given the subject of the paper, I think this issue should be addressed in more detailed.

Another point concerning the method is the treatment of biomass burning emissions in GEOS-Chem. On page 23789 it is mentioned that the annual biomass burning amounts are the same in each simulated year (actually they simulate only 2001) and that the distribution is based on fire counts in the years 1996-2000. Furthermore, emissions are applied in monthly amounts, with a strong peak in March. Given the subject of the paper this makes me wonder why the authors did not apply a more sophisticated way of distributing their emissions. Later in the paper it is concluded that vertical redistribution by convective activity is fast, but I would expect that convective activity (rain) and fires (no rain) are non-overlapping. By using a fixed pattern of burning each year, you might emit large amounts of biomass burning in convective regions during your 2001 simulation. This approach is particularly strange if one realizes that the authors have access to the specific locations of the fires measured by MODIS/MISR. With figure 1, the authors try to show that burning occurs in the same locations every year (there was no significant difference in the spatial distributions of the identified smoke plumes from year to year, page 23791), but I find the proof for that far from convincing. One could argue that given the model resolution used here, these details are not important, but again, given the subject of the paper, one expects at least an assess-

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ment of the uncertainty of the chosen approach. By showing monthly averages (e.g. figure 4), synoptic scale variations are averaged out. One might ask whether analysis on monthly output of this model (run with climatological biomass burning emissions and climatological emission heights, but with 3-hourly MERRA meteorology) does not underestimate the effect one could get with a more tailored model analysis.

Finally, based on the model output, interesting observations are made about the impact of the emission heights on chemistry of PAN and O3. However, these model results are not validated in any way. The validation only uses BC measurements of the 2001 TRACE-P campaign. I wonder if the authors looked in other ways to validate their results.

In conclusion, the authors did a great job in analyzing a huge number of smoke plume injections. In that respect, the contribution of this paper is valuable. The model analysis and validation, however, is done in a rather quick and simplified way and does not analyze the results on the spatial and temporal scales that are relevant the process studied. One can thus only conclude that the impact of the plume height (e.g. on modeled CO) is not significant in the model world.

Minor comments:

Technically: MINX is published, but the smoke plume detection seems rather subjective. It would be good (e.g. in an appendix) to verify some aspects of it. For instance, the derived wind speed: how well does the derived wind compare to independent data (e.g. de winds used in GEOS-CHEM)?

Page 23783: line 17: remove “local” before air quality. This is widespread transport, so local seems not adequate.

23784, line 15: Model . . ..2 km. Sentence does not run smoothly. Consider rewrite.

Line 27: I suggest to replace “strong convections” by “strong convective activity”

23787, line 22: ‘, where both were retrieved, were..’. Suggest to replace by: ‘in cases

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for which both values were retrieved, were..'

23790: line 21: All measurements were merged to a time resolution of one minute. This sounds strange after discussing time resolutions of 3-7 minutes, 1-3 minutes, and canister air samples. Please clarify.

Figure 2(a). I do not see the added value. Figure 2(b) is simply a split out of figure 2(a) if I understand correctly.

23795, line 6: the lifetime of two month sounds like a global average and not a lifetime for this tropical (and thus high OH) region.

Line 10: "we concluded": why not "we conclude"? This conclusion is drawn here I assume.

23786, line 3: "Part of those BC were", should be "Part of this BC was".

23799, line 21: Deep convections -> deep convective activity

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 23781, 2013.

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