We thank the referees for their helpful comments. We respond to each specific comment below. The referee comments are shown in red italics. Our replies are shown in black and the corresponding text are shown in blue.

1. 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?
We agree that the overpass time sampling is an important source of uncertainty that could not be resolved with our current methodology. We added text and cited Giglio (2007) in Section 3.2 to highlight this uncertainty.

Section 3.2: "The probability distributions of smoke pixel heights determined in Section 3.1 were susceptible to possible biases from sampling. The smoke plume heights detected at Terra satellite overpass time (approximately 10:30 a.m. local time) may not be representative of the smoke plumes during other times of the day, especially for fires associated with agriculture. Giglio (2007) showed that the diurnal cycle of fire activity over Southeast Asia peaks at 15:00 local time. Also, the MISR swath is 360 km wide while the MODIS swath is 2330 km wide. As a result, 85% of the fire pixels detected by MODIS were not within MISR footprints. In addition, fires and smoke plumes obscured by clouds were not detected by either MODIS or MISR. We assumed that the smoke plumes outside of the MISR temporal and spatial footprints or obscured by clouds had the same smoke pixel height distribution as the ones analyzed in Section 3.1. However, we acknowledge that these are important uncertainties that should be further investigated using in situ or remote sensing measurements that sample other times of the day."

2. 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.
Thank you for the comment. The results reported in this study were insensitive to the planetary boundary layer mixing scheme. We conducted a sensitivity simulation using a more advanced, non-local boundary layer mixing scheme but found no significant differences to the long-range transport of pollutants. We modified text in Section 2.4 to clarify this point:

Section 2.4: "Turbulent mixing within the boundary layer was assumed to be full and instantaneous (Bey et al., 2001). We conducted a sensitivity simulation using a non-local boundary layer mixing scheme (Lin and McElroy, 2010) but found that it had no significant impact on the results reported below in Sections 4 and 5."

3. 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 assessment 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.

Thank you for the comment. We used monthly-resolved emissions and compared model to aircraft measurements on a monthly-average basis for two reasons: First, Heald et al. (2003) showed that using monthly-resolved PSEA biomass burning emissions significantly improved the simulation of aircraft measurements downwind of the PSEA, while using daily-resolved emissions did not offer further improvement. Also, using the available MODIS fire counts to spatially allocate emissions on a daily basis would introduce a greater sampling bias due to patchy sampling. We added text in Section 2.4:

Section 2.4: "We did not resolve daily emissions for two reasons. First, Heald et al. (2003) showed that using monthly-resolved PSEA biomass burning emissions significantly improved the simulation of aircraft measurements downwind of the PSEA, while using daily-resolved emissions did not offer further improvement. Second, using daily MODIS fire counts (Section 2.1) to spatially allocate emissions on a daily basis would lead to more pronounced greater sampling bias by the satellite instruments (Section 3.2)."

4. Finally, based on the model output, interesting observations are made about the impact of the emission heights on chemistry of PAN and O_3. 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.

Thank you for the comment. We chose to focus on BC because its long-range transport was most sensitive to the injection height of smoke plumes of the PSEA. The impacts on the long-range transport of PAN and O_3 were small (at most 20% for PAN at 700 hPa, Figure 6). As a result, we found that similar comparisons between model and TRACE-P measurements were meaningless given the low density of measurements during TRACE-P. We also stress that the comparison of simulated and measured BC is not sufficient validation for our smoke plume height analysis. Rather, it highlights the point that BC transport is sensitive to the initial injection height. We modified the text in Section 5 to clarify:

Section 5: “Our model simulations in Section 4 showed the injection height of smoke plumes over PSEA had the largest impact on the long-range transport of PSEA biomass burning BC to the northwestern Pacific.”

Section 5: “Our analyses above showed that directly injecting 40% of the PSEA biomass burning pollutants in the free troposphere in the model led to a more pronounced BC peak at 3 km over the northwestern Pacific, which was in better agreement with the aircraft observations compared to the control simulation. We emphasize that this is not sufficient validation for the smoke plume injection height presented in Section 3. Other factors may contribute to the improved performance when using the MISR-constrained injection height, such as an underestimation of the PSEA biomass burning emissions, or an overestimation of the BC wet scavenging rate in the model.”

5. 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)?

Thank you for the comment. We completely agree that the MINX-derived smoke plume heights need to be further evaluated/validated, but that is beyond the scope and methodology of this paper. Comparison with the meteorological data used GEOS-Chem would not have been meaningful considering the great difference in resolution. While there has not yet been a detailed validation of MINX in the literature, the sensitivity of retrieved plume height to wind direction has been analyzed in Nelson et al. (2013), and we added reference to that.

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

Fixed as suggested. Thank you.

7. 23784, line 5: Model….2 km. Sentence does not run smoothly. Consider rewrite.

Thank you for the comment. We have rewritten the sentence.

Section 1: “Model simulations showed that the PSEA biomass burning pollutants were lifted from the surface near their source region by deep convection. These pollutants were then transported along the warm conveyor belts ahead of cold fronts to the northwestern Pacific at altitudes above 2 km (Carmichael et al., 2003; Liu et al., 2003; Miyazaki et al., 2003; Lin et al., 2009).”

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

Fixed as suggested. Thank you.

9. 23787, line 22: ‘, where both were retrieved, were..’. Suggest to replace by: ‘in cases for which both values were retrieved, were..’

We modified the text to be more precise:

Section 2.2: “The differences between wind-corrected stereo-height and zero-wind stereo-height were almost always within 500 m. Only 2% of all the analyzed pixels had wind-corrected stereo-heights and zero-wind stereo-heights differing by more than 1 km, and these pixels were not excluded from the analysis.”

10. 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.

Thank you for the comment. We modified the text to clarify the data processing steps.

Section 2.5: “BC, CH_3Cl, and C_2Cl_4 measurements were synchronized to a time resolution of one minute to identified the samples heavily impacted by biomass burning (CH_3Cl > 550 ppt and C_2Cl_4 < 3 ppt) following Kondo et al. (2004). Measurements and model results were both averaged to the temporal (15 minutes) and spatial (2.5 ºlongitude × 2 ºlatitude) resolution of the model for comparison (Section 5).”

11. 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.

Thank you for the comment. We combined Figure 2(a) and 2(b) as suggested.

12. 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.

Thank you for the comment. We changed the lifetime to 1-2 months.

Section 4.1: “Therefore, CO, which has a lifetime of 1-2 months …”

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

Thank you for the suggestion. We chose to keep the past tense (“We concluded …”) to maintain consistency of the verb tenses throughout the paper.

14. 23796, line 3: “Part of those BC were”, should be “Part of this BC was”.

Fixed as suggested. Thank you.

15. 23799, line 21: Deep convections -> deep convective activity

Fixed as suggested. Thank you.