Interactive comment on "A regional chemical transport modeling to identify the influences of biomass burning during 2006 BASE-ASIA" by J. S. Fu et al.

Anonymous Referee #2
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General comment:
This manuscript tries to show the influence of biomass burning in Southeast Asia on the air quality in both source and downwind regions by using regional chemical transport model for the spring in 2006. I agree with the importance to estimate the impact of Southeast Asian biomass burning on the air quality outside the source region. The authors calculate the biomass-burning-related contribution on CO, O3, and PM2.5 by performing two model experiments with and without biomass burning emission in Southeast Asia and taking the difference of both experiments. The method is not novel but adequate for the purpose of this study. The purpose and the methodology of this study seem to be plausible, but I don't think the manuscript as it stands is suitable for the publication in ACP. My biggest concern is obscure descriptions found in many places in the manuscript, which seriously degrade the value of the manuscript. The authors should carefully clarify those descriptions.

We appreciate the reviewer's time and comments for improving the manuscript. We have responded to the all the comments point by point.

1. Specific Comments:
(1) Abstract (P3072 L7), Concentrations of which altitude? surface? please clarify.

Response:
It is the surface concentration of gases and particles. We have clarified it in the revised manuscript.

(2) Abstract (P3072 L8-13), Please clarify how those values are deduced. Are those values the monthly means or based on the analysis of either episode?

Response:
The deduction percentage of these values was based on the episodic analysis. We have clarified it in the manuscript.

(3) Abstract (P3072 L14), What is "an impact pattern"?

Response:

"The impact pattern" refers to the areas that biomass burning could impact in both the source and downwind regions during the study period, which is visualized in Figure 7. We have revised the original sentence as " In March, the impact of biomass burning mainly concentrated in Southeast Asia and southern China, while in April the impact becomes slightly broader and even could go up to the Yangtze River Delta region".

(4) Abstract (P3072 L18), Is there “strong” upward tendency in the concentrations of CO, O3 and PM in the source region? At P3090 L3-4, the manuscript described that the concentration gradient was very small or had an increasing trend from the surface to high altitutde. For me, this description and “strong” upward tendency are inconsistent.

Response:

Yes, we did mean the strong upward transport of biomass emitted gases and PM but not tendency. We have revised it in the abstract that replaced the word “tendency” to “transport”.

(5) Abstract (P3072 L19-21), What is the contribution of subsidence? The values in this sentence can be found in section 4.4 (P3090 L15-17), but in this part, the values are contribution of long-range transport. Do you mean the same thing?

Response:

Yes, we did mean the same processes. Actually, we think that the subsidence was caused by the long-range transport from the source region, and we have revised the original sentence as “The subsidence process during the long-range transport contributed 60 to 70%, 20 to 50%, and 80% on CO, O3 and PM2.5”.

(6) Abstract (P3072 L23-24), Why you can say this is lower limit? Show the reason.

Response:
In this study we choose 2006 as the study year for biomass burning. In fact, we have another study focusing on 2007-2009 biomass burning impact. We also found the biomass burning emissions in 2007 is larger than 2006. Thus, we think 2006 may represent a lower limit in biomass burning emissions even though it may not be the lowest.

(7) P3074 L9, Where did those studies show the springtime high O3 events?

Response:

The study by [*Liu et al., 1999*] was a first observation study of highly enhanced ozone layers associated with biomass burning in continental Southeast Asia. The location of that study was Hong Kong, where they observed that the ozone concentrations at elevations of 2~5 km often exceeding 100 ppbv during February through April of 1994 to1996.

(8) P3075 L22 – P3076 L2, This sentence is quite difficult to understand for readers unfamiliar to WRF/CMAQ system. Remove it if unnecessary or at least rephrase it.

Response:

We have removed these sentences in the manuscript. Thanks for the suggestion.

(9) P3075 L3-4, which domain does Figure 1 show? Please mention it in the caption of Fig1.

Response:

Figure 1 shows the 27×27 km nested domain from a mother domain with a resolution of 81×81 km. We have added detailed description of the domain information in the caption of the figure.

(10) P3077 L13-15, Please explain in more detail how to interpolate GFED data by using FLAMBE data. In particular, how do you treat such case as there is GFED emission but is not FLAMBE emission?

Response:

As we indicated in the second paragraph of Section 2.3.2, The FLAMBE emission inventory is at hourly time resolution while GFEDv2.1's time resolution is much lower with an eight-day resolution. Thus it is necessary to distribute the eight-day emission into an hourly resolution which could be used in the CMAQ model if the GFED emission inventory is applied. In this study, we distributed the GFED emission by using the temporal profile from FLAMBE. From FLAMBE's hourly and daily emission variation, we can get the hourly biomass burning percentage of one day and also the daily percentage in one month. And then we could use these percentage profiles to allocate the eight-day GFED emissions into hourly profiles.

(11) Section 2.3.3, The title of this section seems no to fit. How about "Injection height of biomass burning emission"? The wording of "inject height" and "injection height" should be unified.

Response:

We have changed the section title as the reviewer suggested. And we have replaced the wording of "inject height" to "injection height" in the whole text.

(12) Section 3.2, Why do you pick up only Lulin site here? The data at Lulin is only shown in supplemental figures, and in those figures, the data at 4 sites in Hong Kong also shown. I recommend adding some descriptions on Hong Kong sites here or moving all this section to supplemental material.

Response:
We added the descriptions and citations on Hong Kong sites.

Four sites in the Hong Kong Environmental Protection Department (HKEPD) Air Pollution Index (API) network were used as observational stations for model evaluations.

Two of them are Tsuen Wan and Yuen Long on the southwest and northwest of Hong Kong, while the other two are Tap Mun and Tung Chung, which are remote sites located in the northeast of Hong Kong and Lantau Island. Other detailed information was described elsewhere (Kwok et al., 2010).

Kwok, R. H. F., J. C. H. Fung, A. K. H. Lau, and J. S. Fu (2010), Numerical study on seasonal variations of gaseous pollutants and particulate matters in Hong Kong and Pearl River Delta Region, *J. Geophys. Res.*, 115, D16308, doi:10.1029/2009JD012809.

(13) P3082 L7, Underestimation of 450 ppbv seems to be overstated.

Yes, the upper limit between the peak values of modeled and observed CO concentrations was about 300 ppbv. We have revised it in the manuscript.

(14) P3083, L5, In Section 2.3.3, there are no concrete descriptions about the two ways of emission allocation mentioned in the following sentence. In Section 2.3.3, the author mentioned SMOKE 2.6 was used in this study. Please clarify the relationship between SMOKE 2.6 and the SURFACE and INJECT methods.

Response:

We added the relationship between SMOKE 2.6 and SURFACE/INJECT method in the manuscript.

In this study, we adopted the methodology implemented in Sparse Matrix Operator Kernel Emissions (SMOKE) version 2.6 for calculating the plume fractions in different layer heights given a bottom (Pbot) and top (Ptop) of a plume.

(15) P3083, L9-17, These sentences are too complicated to understand. First of all, the definitions of 4 statistics (MNB, MNE, MFB, and MFE) must be show somewhere in the manuscript. Please unify the wording for those statistics in the manuscript and in the Figures (Fig 2 and figures in the supplement). What is the meaning of "cufoff" here, and why the values of cutoff for MNB and MNE are in ppbv, since the unit for MNB and MNE must be %? The description about the threshold staring from L13 is incomprehensible. Why do MFB and MFE abruptly appear here? What is the difference in the role of MNB/MNE and MFB/MFE? If these statistics are important to evaluate the model performance, the author should explain them in more comprehensively.

Response:

We added the definitions and formula of MNB, MNE, MFB, and MFE in the manuscript. Also, the Mean Fractional Bias can be referred as Fraction Bias or MFB, and Mean Fractional Gross Error can be referred as Fractional Gross Error and MFE. It is now consistent between the descriptions and figures. We deleted the cutoff values and rewrite the explanations of MFB and MFE. We also incorporated the relationship between MNB/MNE and MFB/MFE.

Mean Normalized Bias (Normalized Bias; MNB) and Mean Normalized Gross Error (Normalized Gross Error; MNE) are shown in Equations (Eqs.) (1) and (2) below, where C_m is the simulated model grid value which correspondences to observational site i, C_o is the observed value at site i, and N is the total number of observational sites. However, as is shown from the Eqs. (1) and (2), normalized bias and gross error can become extremly large when the observation data is quite low. On the contrary, Mean Fractional Bias (Fraction Bias; MFB) and Mean Fractional Gross Error (Fractional Gross Error; MFE) have the advantage of limiting the maximum model/observation bias and error. The formula of Fraction Bias and Fractional Gross Error are shown in Eqs (3) and (4). The reason is fractional bias and fractional gross error take the average of normalizing the bias and error by the average of model and observational data.

$$\mathrm{MNB} = \frac{1}{N}\sum_{i=1}^{N}\frac{C_m - C_o}{C_o} * 100\% \qquad (1)$$

$$\mathrm{MNE} = \frac{1}{N}\sum_{i=1}^{N}\frac{|C_m - C_o|}{C_o} * 100\% \qquad (2)$$

$$\mathrm{MFB} = \frac{1}{N}\sum_{i=1}^{N}\frac{C_m - C_o}{(C_m + C_o)/2} * 100\% \qquad (3)$$

$$\mathrm{MFE} = \frac{1}{N}\sum_{i=1}^{N}\frac{|C_m - C_o|}{(C_m + C_o)/2} * 100\% \qquad (4)$$

(14) P3083 L20-22, I cannot confirm this sentence, because the values of benchmark for O3 MNB and MNE only are given in the manuscript and the values of MFB and MFE, not of MNB and MNE, are shown in the supplemental figures. The author should provide sufficient information in order to say that most of parameters are within or close to the benchmark.

Response:

We added the benchmarks for O_3/$PM_{2.5}$ MFB and MFE.

Benchmarks of fractional bias and fractional gross error for O_3 are 15% and 35% , and for $PM_{2.5}$ are 50% and 75%, respectively (USEPA, 2007; Boylan and Russell, 2006; Morris et al., 2006; Tesche et al., 2006).

(15) P3083 L24, NMB and NME should be explained.

Response:

We have added the explanations by replying question 13.

(16) P3083 L25, The overestimation of CO: when and where?

Response:

The overestimation of CO was at Lulin, Taiwan. As shown in Fig. S3, there is some overestimation during the Julian day around 104 to 115. We have revised and stated it more explicitly in the manuscript.

(17) P3083 last sentence, when and where?

Response:

Yes, we still refer to the Lulin site. As shown in Fig. S3, the underestimation of O3 occurred during the Julian days around 109 to 115 and 130 to 134. We have revised and stated it more explicitly in the manuscript.

(18) P3084 L3, What are the supplemental tables? I cannot find any tables in supplemental material.

Response:

We changed the sentence because we did not show the table in the supplement.

In fact, we still found that the INJECT method performed slightly better than the SURFACE method (not shown).

(19) P3084 L27, I don't think the hot spots are well simulated. They are apparently overestimated.

Response:

The inconsistence between the modeled AOD and satellite observations in some regions could be caused by multiple reasons. First, the satellite scan over a specific region could only last several mineral, while in the model the time resolution is hourly, thus it is usually difficult to match the time periods of the two datasets. On the other hand, the satellite signals are usually masked due to clouds and wet precipitation, which are indicated by the blank areas in Figure 3 & 4. While model outputs are usually not affected by these factors at the low layers, which usually show higher values than the remote sensing results. And in this study, the overestimation mainly occurred in the source regions of biomass burning, i.e., Burma, northern parts of Thailand, Vietnam and Laos, while in southern parts where biomass burning was not intense, the comparison between modeled and observed AOD and NO2 was relatively good. This means that the biomass burning emission was an important cause for the model performance as it had a large uncertainly.

In the revised manuscript, we have changed the word "well simulated" to "moderately well simulated".

(20) P3085 L1-2, Why don't you choose CO concentration or column by TES or MOPITT instead of NO2 column? If you consider the consistency with previous figures, model comparison with remote-sensed CO or/and O3 should be added.

Response:
In fact, since we have observational comparisons in Phimai, Hong Kong (4 sites) and Lulin for CO and O_3, it should be enough for the evaluations for these species. We want to include NO_2 is because it is an important precursors of O_3. So we would like to keep NO_2 in the evaluations.

(21) P3085 L17-19, this statement is valid for the first episode, since both satellite and model date well captured the wide spread region of high AOT. However, for the second episode, you cannot say so, because no satellite data is available in most of China and Taiwan region.

Response:

Yes, we have made this statement only for the first episode. And we have revised the original sentence as "Both satellite and CMAQ model show heavy aerosol loading over the biomass burning region in Southeast Asia and in the downwind areas during the first episode (March 28), suggesting strong biomass burning activities and substantial long-range transport during the two episodes. As for the second episode (April 13), although no enough satellite data was available in downwind areas, the model still simulated a large scale transport of aerosol. However, the ability of the long-range transport during the second episode was not as strong as the first one.".

(22) P3085 L19-22, Which episode does this sentence describe?

Response:

This sentence described the first episode.

(23) P3086 L5, Is the phrase "such as depositions" here correct?

Response:

We have corrected this sentence as "NOx (NO+NO2) were highly reactive and quickly converted to the particulate phase which was easily subject to deposition".

(24) P3086 L21, Concentrations of which altitude? surface? please clarify.

Response:

The two figures refer to the surface level. We have clarified this in both the text and the caption of the figures.

(25) Figure 6: Figures for both days are necessary? Only 13 April would be enough.

Response:

There are still some differences between two days in the distributions of CO, O_3 and $PM_{2.5}$. We feel it is good to show two days variations instead of only single day results.

(26) P3089 L28, Figure 8 shows the concentration. So, we can find the gradient of concentration, not that of emission. There are two other sentences using "emissions" which should be replaced by "concentrations"; P3090 L1 and L13

Response:

We have replaced them as the reviewer suggested.

(27) P3089 L28- P3090 L1, There is blank area around 103E where not over the ocean. Why?

Response:

The area around 103°E at the cross section of 15°N was at the central part of Thailand. From Figure 5, 6, and 7, we could find that the impacts of biomass burning were limited in Burma, northern parts of Thailand, Laos and Vietnam. While from the central part of Thailand (around 17°N) to its southern parts, the impact of biomass burning was negligible. We assume that this pattern of pollutants distribution could be due to the prevailing winds during the study period. As illustrated in Figure 7, the prevailing winds mainly blew from west to the east, which pushed the pollutants more northward and eastward and accumulated at the foot of highlands around the border of Burma and Thailand. That's why we didn't observe high concentrations at the cross section of 15°N.

(28) P3090 L3, Please clarify the direction of "gradient". Do you mean vertical gradient, or zonal gradient? There are two other "gradient" that should be clarified; P3090 L9 and L15

Response:

As this section discussed about vertical distribution of biomass plumes, the directions of gradient were all vertical gradient. We have clarified these in the revised manuscript.

(29) P3090 L11-12, Which longitude and altitude should we look at to find these values of percentage contribution? Near surface? or around 1-5 km? Please clarify. The same for other sentence; P3090 L15-17.

Response:

The longitude that we discussed was indicated in P3090 L9, which was around 115-122E. The altitude is the whole column, which is from the surface to about 10km. We have clarified this in the manuscript.

(30) P3090 L21, The center of plume layer exists higher than 5km.

Response:

We have checked the vertical distribution of gases and PM and found that the plume layer did exist between 1km and 5km as shown in Figure 9.

(31) P3090 L27, Which longitude and altitude should we look at to find 160 to 200 ppv of CO? Near surface? or around 1-5 km?

Response:

The locations we were looking at were the two cross sections at 15°N and 20°N and The altitude is also the whole column, which is from the surface to about 10km.

(32) Conclusions (P3091 L10) Please give the specific year of examined.

Response:

We have added the year of this study.

(33) Conclusions (P3091 L10) Comparisons of what?

Response:

We have revised the original sentence as “Through comparisons between the base case and scenario case without biomass burning emission”.

(34) Conclusions (P3091 L21), Please give the day of second episode.

Response:

We have added the day of the second episode.

(35) Conclusion (P3091 L22), “to for” ?

Response:

This is typo, and we have omitted the word “to”, thanks for pointing out this.

(36) Conclusions (P3091 L24-25), Please clarify the specific area which you mentioned in this sentence.

Response:

The region includes the source region areas in Southeast Asia, and southern parts of China. We have clarified it in the text.

(37) Conclusions (P3092 L11 and L13-15), As mentioned above (the 4th and 5th specific comments), please clarify these sentences.

Response:

We have clarified these sentences in the text.

2. Technical Comments:

(1) P3037 L73, uncertain ! uncertainty

Response:

We have revised it.

(2) P3075 L6, Is “could” necessary here?

Response:

That’s a typo. We have deleted this.

(3) Table1 (P3103), Reference style is wrong for Wesely (1989) and Walcek and Aleksic (1998)

Response:

We have corrected the style for the two references.

(4) Figure 1 (P3017), It is necessary to note that this figure show the domain of the model in the caption. Typo: Fiver ! Five.

Response:

We have revised the caption of this figure and added the information of the model domain. And the typo is also revised.

(5)P3076 L23, MEGAN: Here is the first appearance of this acronym.

Response:

We have added the full name of MEGAN (Model of Emissions of Gases and Aerosols from Nature) in the text.

(6) P3079 L4, Please show the place of Phimai in Figure 1. Also Lulin and 4 sites in Hong Kong, if necessary.

Response:

Thanks for the suggestion. We have added the observational sites Phimai and Lulin in the figure. Since we have introduced Hong Kong sites and there is another reference (Kwok et al., 2010) which has detailed information about the location of Hong Kong sites, we did not plot Hong Kong sites over there. Another reason is if we plotted four sites inside Hong Kong, it is very hard to see.

Kwok, R. H. F., J. C. H. Fung, A. K. H. Lau, and J. S. Fu (2010), Numerical study on seasonal variations of gaseous pollutants and particulate matters in Hong Kong and Pearl River Delta Region, J. Geophys. Res., 115, D16308, doi:10.1029/2009JD012809.

(7) P3081 L3, Asian ! Asia

Response:

We have corrected this.

(8) Figure 2 (P3018), Please mention what this figure show in the caption. Only “Model Performance of „„” does not inform any proper things. The same can be said for the captions of Figure 5 to 9. The captions of Figure 2, 5, 6, 7, 8, and 9 must be rephrased to better explain the substances of each figure.

Response:

We have rephrased all the descriptions of figure captions as the reviewer has mentioned above.

(9) P3084 L3, typo: in fact

Response:

We have corrected this.

(10) Figures 5,6,7,8, and 9: Red contours are indiscernible. Please use other color or other line type. As mentioned above, caption of each figure must be rephrased. Explanations

for color contour and red contour should be included in the caption.

Response:

We have redrawn the contours to make it more clearly, and also added detail descriptions in the captions.