

**Reviewer 1:**

Overall, I am happy with the author's reply to my previous comments. Additionally, I suggest following corrections/revisions before publication.

**Response:**

*Thank you for your useful comments and hereby we present our responses for the queries below:*

1) The authors addressed the limitation of the study associated with 500-hPa top when aod is calculated. However, it is not stated in abstract and conclusion where it should be stated.

**Response:**

*Thank for your suggestion. We now add sentences to highlight this in abstract (lines 26-28, page 1) "AODEM (extended aerosol optical depth module) was used to calculate the total columnar aerosol optical depth (AOD) and BC AOD up to the top of the domain at 500 hPa (~5,500 m) which did not include the free tropospheric long-range transport of the pollution". Suggestion is added in the conclusion (lines 8-9, page 19) "Vertical model set-up should be extended beyond of 500 hPa (~5,500 m) in the future studies to better incorporate the free tropospheric LRT of aerosol".*

2) Page 7, lines 7-11: Rainfall patterns are shown, but similarity and/or difference between model and observation are never discussed. How does rainfall pattern affect simulated aod and/or pm?

**Response:**

*We actually have discussed the similarity and difference between the model and satellite observation in section 3.2.2 on "Synoptic scale model evaluation" page 10, lines 15-19 "The modeled monthly precipitations for two selected months (August and October, 2007) were compared with the TRMM-3B43 dataset in Figure 2 which showed good agreement in the distribution patterns but the model somehow underestimated the domain maximum monthly precipitation column, for example, that occurred over Myanmar in August 2007 or over the central part of Vietnam in October".*

*We now add explanation of the effect of precipitation on the simulated PM concentrations in section 3.4 lines 24-28, page 16 "Effects of precipitation on the PM levels were also seen, e.g. higher PM levels (Figure 7) were simulated over Indochina in January, October and November as compared to August because the latter was a rainy month in this part of the domain, i.e. less biomass open burning and more wet removal in principle. The opposite was actually seen in the Southern part of modeling domain, e.g. above Indonesia, where lower PM levels were simulated in October (more rainy month in this part) than other months".*

*Effect of precipitation on AOD is also added in lines 19-20, page 17 “Consistently with the PM results, the effects of precipitation on AOD were captured, i.e. higher in the dry months and lower in the wet months in the respective parts of the domain”.*

*Note that in this version we update the Figure 7 by adding results for the month of October 2007 and also Figure 8 by adding result of AOD in October 2007 for comparison with the monthly rainfall presented in Figure2.*

3) Page 10, 1st paragraph, & Fig. S4: Surface pressure fields from model are compared with observed Sea-level pressure. I suggest to use sea-level pressure for model as well.

**Response:**

*Thank you. In this version, we now compare the model results of surface pressure with the ERA interim reanalysis dataset and the figure S4 is revised accordingly. Discussion is added in lines 9-14, page 10 “Spatial distribution of surface pressure over the WRF domain is presented together with the ERA interim dataset in Figure S4 for three selected days (Jan 1, 2013; Oct 8, 2007; and Nov 7, 2007; 00:00 UTC). Both modeled and ERA data showed similar spatial distribution patterns of pressure but WRF appeared to produce slightly lower surface pressure over central Papua of Indonesia for all 3 cases presented. In fact, both datasets showed lower pressure zones over the high mountain areas of Himalaya, eastern parts of China and central Papua of Indonesia that indicated the effects of the topography”.*

4) Page 10, lines 11-17 & Fig. S5: Hard to compare two, but it seems that model and observation winds are quite different. Since WRF model has initial condition from NCEP reanalysis, can NCEP winds be used for observational counterpart. Also, “synoptic upper wind field” is not commonly used terminology.

**Response:**

*Thank you for your suggestion. We now compare the upper wind (850 hPa) with the ERA interim reanalysis dataset and Figure S5 is revised accordingly and add new upper wind result for 1<sup>st</sup> January 2007. We change the “synoptic upper wind field” to “upper wind field” in Figure S5 caption.*

*Discussion is added in lines 18-21, page 10 “The simulated wind fields at 850 hPa (~ 1,500 m) are compared with the ERA interim upper wind fields in Figure S5 that also showed a consistency of the two datasets and more in the center of the domain both for wind speeds and wind directions. A large discrepancy was seen at the NW corner of the modeling domain and this may be attributed to the boundary conditions (taken from NCEP FNL in this study)”.*

**Reviewer 2:**

I would like to thank the authors for their overall revised content. I thought that they carefully answered the points from each reviewer. I would like to note some points that I am concerned about, including some points I did not notice in the previous review.

**Response:**

*Thank you for the useful comments/corrections from the reviewers that helped us to refine the manuscript. The responses to the queries given by reviewer are presented below:*

<Abstract> The authors commented that this paper was intended to be composed of Part 1 and Part 2 but would handle it as two separate papers. However, Permadi et al.'s title on the reference has not been modified, and it remains as Part 2. Although it is not directly related to this paper, I think that it is better to modify it.

**Response:**

*Thank you for your correction and we revised the references accordingly to “Permadi, D.A., Kim Oanh, N.T., Vautard, R.: Assessment of emission scenarios for 2030 and co-benefits of black carbon emission reduction measures on air quality and climate forcing in Southeast Asia, Submitted to Atmospheric Chemistry and Physics , 2017a.*

. <text> p.3 L7 “The results of this Paper 1 are used in the follow up study which investigated the potential co-benefits of various emission reduction measures implemented in Indonesia and Thailand on air quality improvement, number of premature death reduction and climate forcing mitigation in 2030 (Permadi et al., 2017a).” It remains a composition of Part 1 and Part 2.

**Response:**

*Sentence is revised in lines 31-34, page 1, abstract ” The results of this paper are used to calculate the regional aerosol direct radiative forcing under different emission reduction scenarios to explore potential co-benefits for air quality improvement, reduction in number of premature deaths and climate forcing mitigation in SEA in 2030 (Permadi et al., 2017a)”.*

p.3 16L There is a description of spatial distribution, but the description of OB is followed in this part. I think that it would be better to add a description on spatial distribution of OB or to put the description of spatial distribution after the OB description.

**Response:**

*Thank you. As suggested we moved the information of the spatial distribution after the parts explaining the OB emission in lines 4-13, page 4.*

p.3 L32 and p.4 L1 Explanation of GFED3 abbreviation is coming after the first appearance. An abbreviation should be added after the first case the term is used.

**Response:**

*Thank you for the correction and we moved the abbreviation of GFED3 to lines 21-22, page 3 when it appears for the first time.*

p.4 L5 The description of the ship is in the paragraph of CROB emission.

**Response:**

*Thank you and now we move this after the paragraph of biogenic emission in lines 1-2, page 4.*

p.4 L10 The listed URL (<http://glcf.umd.edu>) did not reach the GFCL site. Please check it.

**Response:**

*Thank you it is now updated to <http://glcf.umd.edu> in line 34. Page 3.*

p.4 L11 Is the expression "NO<sub>x</sub> emissions from natural vegetation." correct? "NO emissions from soil"?

**Response:**

*Thank you for your suggestion and we correct in line 1 page 4.*

p.6 L13 The authors compared boundary concentrations of 2007 and 1998-2002, and the difference was described as being in the range of 0.98 - 1.23. However, the authors' consideration about this value is not shown in the text. As in the "authors' response to reviewer", it needs a comment such as "This implies that basically the two datasets were almost similar. The impacts of the aged boundary conditions on the simulation are expected but with a small magnitude".

**Response:**

*Thank you and we add sentence in lines 16-18, page 6 as suggested "This implies that basically the two datasets were almost similar. The impacts of the aged boundary conditions on the simulation are expected but with a small magnitude".*

p.10 L14 Why do the authors show examples in October and November to compare the wind situation of the upper layer with observations?

**Response:**

*We would like to take snapshots of one period of dry season for the areas in the upper part of the equator line (in November) which includes Thailand and other countries located in the continental SEA and below the equator line (in October) which includes Indonesia and Timor Leste. We add now a selected day in Figure for the 1<sup>st</sup> January 2007, 07:00 LST for comparison to be consistent with the discussion on surface pressure and precipitation.*

p.10 L20 Please write the approximate altitude of 500 hPa as shown on p.10 L12.

**Response:**

*Thank you we add the approximation of the physical height of 500 hPa in (~5,500 m) line 22, page 10.*

p.13 L4 I think that it is better to explain this part “EC was measured using thermal optical method while BC was measured using light absorption method.” at the measurement method (p.12 L29 or L30). In addition, please specify that there is no problem about comparison of EC and BC directly.

**Response:**

*Thank you. We move the explanation of EC and BC at the measurement method part in lines 32-34, page 12. We also add sentence to explain comparison between modeled BC and observed EC for AIT site in lines 4-5, page 13 “This is because for PM mass closure, EC seems to be better while BC is suitable for radiative transfer budget analysis (Gelencsér, 2004)”.*

p.15 L10 Is the value shown in Table S3 the average value of the entire model domain? Please write it clearly.

**Response:**

*Thank you we revise the annual average in the Table S3 to be “Max annual avg” and add footnote “One maximum value simulated in the whole modeling domain for the considered period”. The title of Table S3 is revised to “Summary of simulated domain maximum ground-level concentrations PM<sub>10</sub>, PM<sub>2.5</sub> and BC for different periods”.*

p.17 L13 The position of 10 AERONET stations can also be added in Figure S1. Or I think that it is good to put them in Figure 8.

**Response:**

*Thank you for your suggestion, we add in Figure S1 to be placed together with other ground-based observation.*

<Reference> p.24 L5 Is 4 of "UNEP-C4." a superscript?

**Response:**

*Thank you and you are correct it should be a superscript for The Center for Clouds, Chemistry and Climate (C<sup>4</sup>). Corrected accordingly in the reference list "UNEP-C<sup>4</sup>.: The Asian Brown Cloud: Climate and Other Environmental Impacts, UNEP, Nairobi, 2002".*

<Figures> Figure 1 The letter CO in b) has disappeared.

**Response:**

*Thank you for your correction. Figure 1 is revised accordingly.*

Figure 6 I am sorry I could not point it out in the last review, but I think that "PBCAR" in the legend should be "BC".

**Response:**

*We revised the legend of "PBCAR" to "BC" in Figure 6 accordingly. The word of PBCAR comes from the output of CHIMERE model.*

Figure 8

Legend numbers are too small to read.

**Response:**

*Thank you. Legend in Figure 8 is enlarged accordingly.*

Figure S4 The calculation result and the weather chart are difficult to compare. Since the calculation result is drawn with surface pressure, the information of the terrain in the figure is highlighted and it is difficult to compare the pressure pattern. Please draw to sea level pressure uniformly.

**Response:**

*Thank you. We now compare the modeled surface pressure with the ERA interim dataset and figure S4 is revised accordingly.*

Figure S7 As in Figure 7, it is better to write "PM 2.5 January" in the figure.

**Response:**

*Thank you. Figure S7 is revised accordingly. It is actually annual average of PM<sub>2.5</sub>.*