

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.

<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.

<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.

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.

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.

p.4 L5

The description of the ship is in the paragraph of CROB emission.

p.4 L10

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

p.4 L11

Is the expression "NO<sub>x</sub> emissions from natural vegetation." correct? “NO emissions from soil”?

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".

p.10 L14

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

p.10 L20

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

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.

p.15 L10

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

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.

<Reference>

p.24 L5

Is 4 of "UNEP-C4." a superscript?

<Figures>

Figure 1

The letter CO in b) has disappeared.

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".

Figure 8

Legend numbers are too small to read.

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.

Figure S7

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