

Replies to Reviewer-I

Suggestions for revision or reasons for rejection (will be published if the paper is accepted for final publication)

I have a few technical corrections that should be considered before publication in ACP:

Reply: We thank reviewer for appreciating our efforts. We have incorporated suggestions given by the reviewer.

- Abstract: Remove the line break at p2 l26.

Ans: Above suggestion is incorporated at Pg2, L26.

- p5, section 2.2. A reference to table 1 is missing.

Ans: Since Table 1 is repetition of text in the manuscript, it has been removed.

- p7, l160ff. I would suggest to rephrase the paragraph. Delete the text in brackets "(dedicated by our computational resources)" and skip the begin of the sentence in line 162 "While we acknowledge the limitations of our relatively coarse vertical resolution". I would simply write "However, the model resolution of T42 L31 is still capable of reasonably simulating the general regional spatial pattern of precipitation and low-level circulation..."

Ans: Above suggestion is incorporated at Pg7, L159-162.

- p7, l160: I would rather write "probably" instead of "will likely" and then it should read "depends on model resolution and an increased vertical" --> add "s" to "depend" and include "an" before "increase".

Ans: Above suggestion is incorporated at Pg7, L158.

p10, l234: flank_of --> I guess the underscore is obsolete.

Ans: Above suggestion is incorporated at Pg10, L232.

p16, l375: Include "the" so that it reads "The authors....."

Ans: Above suggestion is incorporated at Pg16, L377

p25, l772: The opening bracket is obsolete or the closing bracket is missing.

Ans: Above suggestion is incorporated at Pg25, L773.

Replies to Reviewer-II

The authors have done considerable work to incorporate the comments of the referees, leading to an improved version of the manuscript. However, there are still major and minor points that need to be addressed before the paper can be published (see list below). Additional points mentioned below in comparison to the first referee comment result from the changes of the revised manuscript in comparison to the ACPD version.

Reply: We thank the reviewer for appreciating our efforts. We have incorporated suggestions given by the reviewer.

Comments to the authors:

Major Comment:

- You write that lightning NO_x is included in the simulations, which answers one major point raised by the referees. However, I do not agree with the statement (p.7 l.153-155) that: "Therefore NO_x anomalies obtained from the ... do not have an impact of lightning or soil emissions as they are same in all the simulations.". This statement might be true for the soil emissions, if they are simply fed into the model. But for lightning NO_x, which I assume is parametrized as in Fadnavis et al 2015, this is not true for a fully-coupled model. A changed circulation (initially due to different anthropogenic NO_x emissions) has an effect on convection (raised already in my first comment: General Comments first point) and hence on lightning NO_x production. An example of this can be probably seen in Fig.2 d) at the Equator and south of it at ~500-200hPa (blue blob), which is likely connected to less (convective precipitation) in that region (Fig. 9 d). This affects the discussion of the results at various parts in the manuscript.

- With respect to this. Please mention the parametrization scheme of lightning NO_x you are using as it is a key aspect of your simulation. Also the content of Table 1 could be stated in the text as it contains mostly repetitive statements. Instead - in my view - a table showing the VOC, CO, NO_x emission totals during JJAS (maybe split by sector) over India and China for the CTRL run would be appropriate.

Reply: We agree that there may be indirect impact of lightning NO_x. However, the impact of lightning NO_x may not be significant as it is same in both the simulations (CTRL and sensitivity). In Fig. 2, a blue blob at the Equator and south of it at ~500-200hPa is due to the descending branch of monsoon Hadley cell circulation. We have mentioned about this

subsidence in the discussion section. As suggested we have mentioned about indirect impacts of lightning NO_x (Pg7, L151-153).

We have now mentioned lightning NO_x parameterization scheme (Pg6, L123-124).

In the model we have used RETRO emission inventory. VOC, CO, NO_x obtained from the CTRL simulations are concentrations and not emissions. The concentrations of VOC, CO, NO_x obtained from the CTRL cannot be split sector wise. Since Table 1 is repetition of text in the manuscript, it has been removed.

Minor comment:

- Sometimes cross TP transport is indicated with arrows in the Figures, while the differences are really small and might not be important/significant (especially for the Ind38 simulation).- I was asking for significance of the results in the first comment. Unfortunately, I don't understand the contour lines in Figs. 3 and S6, which are supposed to relate to the 95% confidence levels (using black for the tropopause, as well, is not optimal). I was expecting some transparent shading out of not significant results, or are these contours to various levels. Also have you checked the significance with respect to the other figures? This relates also to the question about cross TP transport.

Reply: Contour lines in Figs. 3 and S6 indicates 95% confidence levels. As suggested we have marked the confidence levels by black hatched lines (Figs. 3 and S6).

- On p. 7 l156-159 you mention 4 additional "lightning-off" simulations. However, you only discuss the CTRL "lightning-off" simulation in the whole manuscript. Either only introduce this simulation or make a statement about the other simulations in the manuscript.

Reply: We agree there was some confusion about the lightning-off simulation. In CTRL, Ind38, Ind73 and chin73 simulations lightning parameterization is on. In addition to these simulations we have performed another CTRL simulation with lightning parameterization off. This experiment is described in Fadnavis et al. (2015). This is now made clear in the revised version of the manuscript (Pg. 7 L154-157).

- p. 15 l. 356-357: Please include a reference to the Figures that support your statement.

Reply: Above mentioned suggestion is incorporated in the revised manuscript (Pg15, L357-358).

Specific:

- p. 1 l.14: add "emissions" after "(NO_x)"

Reply: Above mentioned suggestion is incorporated in the revised manuscript (Pg1, L14).

- p 5. l106-108: You state that MLS data was interpolated to potential temperature levels but you are using MLS data on pressure levels (Fig. 1)?- Sorry about the mistake regarding the MLS kernels, I was thinking about Fig 1b) , i.e. the model data. However, I thought that the MLS AKs are defined for the MLS levels, which do not include 90hPa (https://mls.jpl.nasa.gov/data/ak/MLS-Aura_L2AK-O3-LAT0N_v04-2x_0000d000.txt).

Reply: This sentence related to interpolation was creating confusion and hence it is re-written. The procedure of application of MLS averaging kernel on model data is explained at Pg.8, L175-177.

- p. 9 l. 210: "demonstrated" rather than "quantified"?

Reply: Above mentioned suggestion is incorporated in the revised manuscript (Pg9, L209).

- p. 10 l 231-235: You attribute the enhanced values of NO_x in Fig. 2f to the southern flank of the Himalaya, however, the major sources over China are located east of the Himalaya (Fig. S1)...

Reply: Thank you for the careful reading. It is corrected now.

- In two Figures the description within the graphic and the description in the caption do not match (Figs. 7 and 9).

Reply: Thank you for the careful reading. It is corrected now.

- Please add the Tibetan Plateau in (one of) the map plots to make the discussion easier to follow. Also, including the outline of the AC in lat/lon vs. height plots, e.g. via winds or just a box, would be helpful to follow some of the statements (e.g. in Sect. 3.3).

Reply: As suggested, we have now indicated Tibetan Plateau in figure Fig S4(c) and outline of the AC in lat/lon vs. height in Figs. 2(a) and 2(d).