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Atmospheric Chemistry and Physics Discussions

Interactive comment on "What caused the extreme CO concentrations during the 2017 high pollution episode in India?" *by* Iris N. Dekker et al.

Anonymous Referee #1

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This study uses new high-resolution satellite measurements in combination with ground based in situ measurements and regional modeling to diagnose the contributions to extreme CO over northwest India for four days in November 2017. The authors analyze modeled meteorology differences between a polluted period and a cleaner period as well as identify sector-based contributions to the pollution event. The results are timely and interesting, with two very important outcomes – that India now experiences higher pollution than China, and that anthropogenic emissions are important contributors to the high pollution event in India.

The manuscript is well written and fits within the scope of ACP, in particular by analyzing new remote sensing measurements and using regional modeling. The general and broader implications of the study could be discussed to a deeper level, perhaps

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with more emphasis on the differences between India and China. Analysis is systematic and thorough, although some areas need clarification. Below are several specific comments, roughly in order of importance. All other comments relate to technical corrections.

Specific Comments:

1. Missing emissions
a) Please provide motivation for increasing GFAS emissions by 5 or 10 times. For example, are there reasons to believe these are the bounds on fire emission uncertainty? Add in to methods on page 5 or work into the analysis on page 8.
b) Low emissions are not the only way an inventory can be wrong. With small fires, such as agricultural fires, the issue is that fires could be completely missed (as discussed on page 12, lines 8-12), meaning that increasing emissions will not help (i.e. 10 x 0 = 0). This might be what is occurring for GFAS in this study, especially for 11-14 November. TROPOMI shows many more CO hotspots in NW India between 75 E and 80 E (Fig. 5c), compared to the 5 $\times$ GFAS plot (Fig. 5e). In comparison, NW India between 70 E and 75 E seems better captured by the modeled hot-spots. This discussion is currently missing in the manuscript. Potential missing emissions could be further analyzed by spatially comparing the 5 $\times$ GFAS to the 1 $\times$ GFAS simulation to determine how much the emission increase resulted in CO column increase in the underrepresented area. A difference plot to TROPOMI could help determine the location of missing emissions. Also, a comparison to an inventory like GFED 4s, which includes an algorithm to capture small fires, could be used to analyze the spatial uncertainties in the emissions. As a result, fire emissions required in the underrepresented area could be estimated and assessed for plausibility.
c) Is there reason to believe MACCity is too low by 20%?

2. Selection of the two date periods
a) More motivation is needed for selecting the date periods. The manuscript mentions "based on patterns seen in TROPOMI" (page 7, line 1), and suggests this is presented

in Section 4.3, which is not the case. The ground-based timeseries (Fig. 7) implies the cut-off could be Nov. 11-14, but also Nov. 11-16.
b) Clarify why TROPOMI data before November 11 was not used (was it not available?).
c) Section 3.4 discusses that ground stations show the polluted period begins on November 3rd. Why are the ground-based timeseries only shown from November 11th onwards? It would be valuable to plot all of November in order to see the increase from the 3rd of November onwards. Alternatively, the investigation of ground station data could be informed by TROPOMI and only the same time periods investigated.

3. The paper would benefit from a little more direct discussion on why investigating CO is important - e.g. in addition to a CO health threshold, co-emitted species are also relevant for human health; CO can be measured from space so using this to analyze transport of CO can determine the impact of transported pollution. This would be best built into the Introduction, Discussion and Conclusion. Also, discussing the potential impact on many people in this region would help motivate why a study of this specific region is important. Finally, while meteorology and stagnant conditions seem to drive this pollution event, there would not be as high pollution in the region without the high anthropogenic emissions. The authors could be a little more direct in highlighting this (e.g. page 15, line 12). Quantification of the anthropogenic contribution could be added to the Abstract, Discussion and Conclusion.

4. Expand the discussion of India versus China
a) Using the CAMS reanalysis timeseries of 2012-2017 as November average CO over the IGP and comparing with NE China could add more support to the argument on page 7 that India is now more polluted than Chna. Expanding this discussion would help make the manuscript less focused on a specific area.
b) Page 8, line 1: Please explain that if NCAP does not have emission reduction targets, what does it aim to do?

5. Emission resolution

How are the 0.5 × 0.5 degree MACCity emissions interpolated to the 10 km by 10 km WRF grid? Are they downscaled? Is the result mass conserving?

6. Introduction:
a) Page 1, line 21: What is the temporal resolution of the "high concentrations", i.e. is 10mg/m^3 per 8hr, 24 hr? Also describe temporal resolution for 400 mg/m^3 on page 2, line 8 so we can compare to the health standard more accurately.
b) In general, the motivation for using both mg/m^3 and ppb in the manuscript is unclear. I suggest to make it clearer on page 2, line 8 why the conversion to ppb is described, and to only use ppb from there onwards (e.g. Page 11 swaps back to using mg/m^3 even though it talks about plots that are displayed in ppb).
c) Page 2, paragraph starting line 10: Why is GFAS uncertainty brought up in this paragraph, i.e. why is it relevant for this study? I suggest to include an introduction of the fire emission sensitivity tests (× 5, × 10) in the paragraph on page 3.
d) Page 2, lines 27-29: Clarify "this period". Did TROPOMI (and CAMS) observe high mixing ratios for November? All days or certain days?

7. Section 2:
a) More information is needed about the TROPOMI a priori. For example, does it come from a model climatology? Is it spatially and temporally varying?
b) Are land-only retrievals used or ocean as well? If ocean retrievals are used, explain retrievals over ocean (e.g. must have clouds).
c) Page 4, line 14: Biases in the KNMI, 2018 report are relative to MOPITT and IASI which are assimilated into CAMS, so are not independent measurements. Please remove "satellite" from this sentence, leaving the bias relative to TCCON.
d) Define vertical resolution of CAMS rather than "various pressure levels" (page 4, line 16).
e) Define time period of CAMS data used. I interpret that 2012-2017 is used to look at October-December interannual variability, and an earlier version of CAMS was used for November 2017 boundary conditions in WRF.

f) Does WRF nudge completely towards the meteorology, or does it nudge as a percentage?
g) The WRF set-up is unclear. What is the temporal period of WRF modeling? Is there a spin-up period? Are there some WRF with tracers simulations and some WRF-chem simulations? I did not realize until page 13, line 34 that there was a MACCity-only simulation. Is OH oxidation of CO performed in the sensitvity run (a major sink) subtracted from the total CO in all simulations? It would be helpful to define the base-run that everything else is compared with. Perhaps a table recording all the simulations performed would also be helpful.
h) Figure 2: Names which are combinations of region and station are a little confusing. The acronyms are not used further in the manuscript, so I suggest writing the whole name on the map and coloring/shading the regions underneath, or adding boxes around the three regions investigated. Add latitudes and longitudes to the map.
i) Why is the OH climatology scaled by 0.92?
j) Page 7, line 2: Expand how the amount of data per day was used to select periods - e.g. how many data points are in each period, were they chosen to be similar?

8. Section 3:
a) While CAMS has been compared to TROPOMI globally (page 3, line 22) bias and correlation between TROPOMI and CAMS for the IGP region is needed to ensure CAMS can be used to probe the relationship between 2017 and earlier years. A sentence or two on this subject would be a valuable addition.
b) Figure 5: Column and row headings in this figure would make it faster and easier for a reader to compare the plots.
c) I suggest that section 3.3. does not need to be separate to section 3.2, because it is still comparing WRF and TROPOMI.
d) Figure 7 Caption: Plots are in ppb not mg/m^3.

9. Section 4:
a) Page 11 lines 1 to 9: This text can be removed because the methodology section

(page 6) mentions that stations close to sources are not used in the analysis.
b) The authors might want to discuss the episodic nature of fire emissions versus the consistent anthropogenic emissions. Also, while a source might contribute a small amount to the total amount of CO, it might be a more substantial contributor for anomalous CO. Finally, in Table 2, why wasn't the period split into 11-14 November and 15-19 November? It is unclear what showing October 1 to November 19 in Table 2 adds to the manuscript, when the focus has been on the two periods.
c) Page 13, line 30: Show the plots of outflow in WRF upper layers to support the argument. A map of the mean meteorological conditions in each period overlaid on the WRF maps of CO may be helpful to visualize the differences between conditions. Alternatively, a Hovmöller diagram of the WRF results may help visualize this outflow.
d) Page 14, lines 7-10: Reword to something such as "In our study, 2017 meteorological conditions are more favorable towards pollution accumulation than 2015 conditions." Also, what data exactly supports this claim? Is it the CAMS concentrations? Or is it meteorological data that isn't shown?
e) Page 14, Section 4.3: Does the inclusion of OH oxidation fix the overestimation for 15-19 Nov in the non-IGP region? I suggest to add another bar to Fig. 6 for the simulation that includes OH oxidation. Section 4.2 would flow better if it was moved to the end of section 3.2.

10. Section 5:
a) Page 15, line 22: Mention the quantitative amount that previous studies have found GFAS to be underestimating fire emissions of CO.
b) Page 15, line 29: Are there any leads on geoengineering meteorology? Also, geoengineering meteorology could have unforeseen consequences for the local or downwind regions, while reducing emissions is likely good overall.

Technical corrections:

I suggest to use consistent date formats throughout the manuscript. For example on the first page there are already several formats being used:"13 October 2017" (page

1, line 1); “11 and 19 November 2017” (page 1, line 6); “11-14 November” (page 1, line 11); “15th of November” (page 1, line 11). The changing of formats can be a bit discontinuous for the reader.

Page 1, Line 1: 2017, measures ⟶ 2017, has measured
Page 1, line 11: The meteorological situation⟶ Meteorological conditions... were most likely
Page 1, lines 13-14: “...emphasizing the important role of atmospheric dynamics.” seems like an unfinished sentence. Expand to explain the role specifically.
Page 2, lines 4-5: Flip the sentence, “Nine out of ten most polluted cities are in the IGP according to the 2018 WHO...” etc.
Page 2, line 4: Define WHO acronym.
Page 2, line 12: Sentence beginning “In addition” is suggested to move to end of paragraph.
Page 2, line 19: practices, these other
Page 2, line 21: extend ⟶ extent
Page 2, line 25: measurement instrument ⟶ measurements
Page 2, line 26: the orbits of scientific ⟶ the scientific
Page 2, line 30: combining ⟶ comparing
Page 2, line 32 to page 3, line 2: Unusual to stop the list of four objectives in the middle with a full stop after objective (2). Suggest to re-write.
Page 3, line 6: section also the role of met... ⟶ section the role of meteorological conditions is also...
Page 3, line 15: Define SICOR acronym.
Page 4, line 6: global resolution ⟶ global horizontal resolution
Page 4, line 20: Remove “ARW” if not used again.
Page 4, line 24: Remove “MYJ” if not used again.
Page 4, line 24: Should “Eta” be capitalized?
Page 4, line 27: Add citation after RRTM and add “, respectively” to the end of the sentence.

Page 6, line 17: As outlined in section 2.1, the...
Page 7, line 6: ...regions (Fig. 2 red and black labels are inner and outer city, respectively).
Page 7, lines 12-13: ...China was the most polluted...
Page 8, lines 3-4: To determine how unique these high CO values were, we analysed the last four years of CAMS data.
Page 8, line 8: However, $\longrightarrow$ These
Page 8, Line 25: Reword the last sentence to say something such as "Atmospheric chemistry is not sufficient to explain differences in CO between the two time periods."
Page 9, line 4: then $\longrightarrow$ than
Page 11, Figure 8 caption: (b.) total column mixing ratio.
Page 13, line 21: According to Fig. 8, the wind speeds clearly...

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