

Review of "Contribution of Asian emissions to upper tropospheric CO over the remote Pacific" by Smoydzin and Hoor

Smoydzin and Hoor presented a well-written paper which I enjoyed reviewing. Overall, the quality of the content is high. The authors analyzed space-borne MOPITT CO data in an innovative way, which leads to multiple new and interesting results. The study period covers 20 years (2000-2019) (the backward trajectory simulation covers 2000-2018) so this is a large undertaking. With analysis based on daily CO values for this long period, the statistic results are robust. I recommend this paper to be accepted.

I have some minor comments for the authors to consider, mostly for clarification.

L56, "LRT events" is defined as "This AOD distribution is log-normal and Luan and Jaeglé (2013) choose the top 20% days in the frequency distribution as LRT events". It is unclear how the days are connected to the events.

This is explained in detail in Luan and Jaeglé (2013). They choose those 20% days of their study period having the highest AOD values and select them as LRT events of pollution from Asia over the Pacific.

L75, the authors selected 400 hPa, a level at the middle to upper troposphere where MOPITT CO data are less biased than at the other levels. Nevertheless, the signal at this level may still be contaminated by signals at the other levels. It is informative to show some information on the vertical sensitivity of MOPITT data over the NH-Pacific, in the main manuscript or Supplement, for example, in term of the vertical distribution of the average kernels over the NH-Pacific. Are both daytime data and nighttime data used in the analysis?

We added a supplement to the manuscript showing the averaging kernels for our study region and added a sentence in section 2.1 referring to the supplement

L80, "A global coverage" can be changed to "A near-complete global coverage".

We changed this line.

L139, "Though, the linearity (and therefore the trend) of the data set is rather weak."? The trend appears statistically significant. Please check.

We rephrased this line:

In particular, during winter time the decrease of CO-maxima days is statistically significant. We find CO-maxima on ~80% of all days in 2001 while less than 60% of all days in DJF are selected in 2019 (Fig. 4(C) DJF). However, the correlation of the data points is rather weak ($r < 0.5$) showing a high interannual variability.

L155, which ENSO index is used? When correlate the ENSO index with CO mixing ratios, is the lag set as 0?

(1) Yes!

(2) We use the Oceanic Niño Index.

We realised that the link to the web page was incomplete and we corrected it in the caption of Fig. 4 (https://origin.cpc.ncep.noaa.gov/products/analysis_monitoring/ensostuff/ONI_v5.php)

L171, "16 days" would be too long and the data from the last few days may not be reliable. Please provide an justification for this selection

This is indeed true. We performed the simulations for such a long time period simply to ensure that we capture all source regions. Trajectories from India need longer to reach the eastern NH-Pacific than those from China.

In the manuscript, we only mention the average residence time of the trajectories in the free troposphere (End of section 3) which is on average less than 7 days for all trajectories (apart from those from India). For all trajectories included in the statistics, the considered time period is on average less than 10 days (time between crossing a CO emission region and reaching the CO maxima) again with trajectories from India being an exception.

L176 and 254, please change “850hpa” to “850 hPa”. Please leave a space between a value and its unit throughout the text.

We changed this.

L302, “We identified”? To be consistent with the rest of the sentences, “We identify” is better.

We changed this.

Fig. 8a. There are two areas in white. Change one of them in a different color.

We slightly changed this plot.