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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Seasonal variability of surface and column carbon monoxide over megacity Paris, high altitude Jungfraujoch and Southern Hemispheric Wollongong stations" *by* Y. Té et al.

Anonymous Referee #1

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The manuscript 'Seasonal variability of surface and column carbon monoxide over mega-city Paris, high altitude Jungfraujoch and Southern Hemisphere Wollongong stations' by Te et al. examines the seasonal cycles of CO at the surface, in the mid-troposphere and the total column abundances. Measurements at three stations in different environments are compared to retrievals from two satellite-borne instruments and the differences discussed with help from the GEOS-Chem CTM v.9. The manuscript provides a very good compilation of the various data sources and model runs.

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Discussion paper

General Comments: The topic of this paper is suitable for ACD. The CO distributions above Paris and Wollongong are unique and should be published. Their comparison to multiple independent data is noteworthy. My major concern is whether the analysis convincingly shows a significant difference in the timing of the seasonal cycles in the surface and free troposphere. While I support publication, this issue and other comments below should be addressed beforehand.

Specific comments:

Introduction: This section could be re-written, excluding the first paragraph and the reference to medical studies, instead focusing on topic of the paper: the sources/sinks which determine the atmospheric variability of CO. Previous works should be referenced.

Sections 2 and 3: I found these overly long. It is not clear which information is new or specific for this study, or had been discussed in previous papers. Without removing pertinent information these sections could be written more concisely; and possibly combined. The important details of the instrument measurements with suitable references could be listed in a Table. Any significant differences should be discussed. The key point is to show the three surface FTS instruments are comparable.

P 7, Figure 1: The averaging kernels for all three instruments should be shown as an indication of the comparability of the retrievals.

Figure 2: The curve fits in panels 2 and 3 are hard to see and could be made darker or thicker.

Figures 2 and 7, P 12 L 320: The seasonal variability of the measurements was characterized by a sine function. It provides constant cycle over time. It does not account for variability in the observations and not particularly good at fitting the data. The authors should consider using a function which includes the sine curve while also incorporating the residuals from the curve.

P 9: A figure showing the averaging kernels of the two satellites should also be added.

P 13„ L 365: The GEOS-Chem results tend to be lower than the measurements. The text comments that this is consistent with results from two inverse models. It is likely the forward and inverse models have different reasons for underestimating CO in the NH. The authors should cite results from other forward models.

P 14, Figure 4: The results from the two satellite data sets should be plotted in different colors and fit separately.

P 15, Figure 5, bottom panel: The text states the surface measurements show a maximum in Jan-Feb, however the in situ data are highly variable with a broad maximum from late fall through spring. The FTS and the model PBL results appear to show the seasonal maximum shifted later in the year. Looking at the data and not the smooth curves I find it difficult to identify seasonal offsets among the data. This should be investigated.

Note: The symbol captions on the figure may hide the highest CO in 2008 – 2012.

P 15, L 393-395, and Figure 5 top panel: Why do the IASI partial columns show much larger variability than the Paris FTIR? Why aren't the MOPITT results shown? The discussion beginning line 419 - 'As the lifetime...' is long and confusing. Could it be rewritten simply as: 'In addition to local surface sources, column abundances are influenced by the transport of down wind emission sources.'?

P 16, L 409-411: Does GEOS-Chem account for lower vehicular emissions in Paris during July and Aug? The OH sink is very likely the main factor determining the seasonal minimum. The seasonal and vertical effects of CO oxidation by OH need additional discussion.

Figure 6: This needs revision. The figure caption is wrong. It is not clear if the data points are the mean of multiple years at different sites (the urban sites) or individual years (Jungfraujoch). But they should include an indication of the spread of values.

The red curve doesn't seem to follow the red circles. Does the curve fit account for measurement/aggregation uncertainties? The authors should try a singular value decomposition (accounting for the Y error) or orthogonal distance regression (both Y and X errors).

P 17-18, L. 429-445, Figure 7: This section adds little to the paper. Many previous works have shown the seasonal maximum in the SH reflects biomass burning. The model run without biomass burning emissions shows reduced VMR but a very similar seasonal cycle. One could conclude from Figure 7 that anthropogenic sources contribute equally to biomass burning. I suggest this discussion be removed.

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