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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "CO emission and export from Asia: an analysis combining complementary satellite measurements (MOPITT, SCIAMACHY and ACE-FTS) with global modeling" *by* S. Turquety et al.

S. Turquety et al.

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We thank the two anonymous reviewers for their helpful comments which have allowed us to improve this paper. In particular, we have addressed the main comments and revised our manuscript accordingly:

1) We have modified the abstract, introduction and conclusion in order to define more clearly the objectives of this study and the main findings, namely: what complementary information is provided by the different observation techniques of CO from space in the framework of the analysis of pollution outflow from Asia.

2) We have included a more specific description of the general meteorological conditions and export pathways.

3) We have reduced and summarized the section 4.3 and the analysis of the long range transport so that specific results should appear more clearly.

A new model simulation has been undertaken for this revision, correcting for a minor problem in the previous one. This has minor impact on the results and the conclusions do not change.

A specific answer to each reviewer is provided in italics following the corresponding comments.

Answers to anonymous reviewer 1:

General comments

The main objective of the paper is to document the CO sources over and pollution export from Asia through the simultaneous use of CO data set from different satellites. In the mean time, the capability of a global chemical transport model to represent those sources and export is also evaluated. I find the paper to be very interesting because there is a clear attempt to intercompare CO observations from 3 satellites and also to combine their different level of information to explore the Asian source and continental export of pollution. The resulting outcomes however, especially those in terms of the characterization and/of quantification of the Asian sources and export are not entirely new and original, and I would like to encourage the Authors to focus more on findings that are really different in comparison to other existing studies.

We agree with the reviewer that the main contribution of our paper is in the approach and the combination of complementary satellite measurements. There is increasing interest in the scientific community in the coupled use of different satellite products,

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thermal and near IR CO retrievals for example which could allow the decoupling of lower tropospheric CO, directly dependent of surface emissions. However, in order to clearly understand the respective merits of each instrument, we find it necessary not to limit our analysis to theoretical considerations but really combine them in a scientific context. In this study, we have chosen to analyze their complementary pictures of the Asian outflow, which is a critical issue since Asia is one of the most important - and increasing - pollution source regions.

Unfortunately, we also show the current limitations of such coupled analysis, mainly due to inconsistencies between the retrievals. But we think this paper is an important step towards a better integration of the available observations. Following the reviewers' recommendation, we have made our objectives and conclusions clearer in the introduction, conclusion and abstract, focusing more on the technical issues of such integrated analysis.

We have refocused the objectives and main conclusions in the abstract, introduction and conclusion.

Specific comments Section 1.

Please provide additional details about the work of Jiang et al., 2007. How does that differ (or is similar to) the present work?

Jiang et al. analyzed the CO retrievals from MLS in the upper troposphere over Asia and the Pacific ocean in order to analyze the driving factors explaining the distributions. Therefore, they use emission climatology and NCEP wind fields. In our study, we have attempted to analyze our knowledge of the Asian outflow based on complementary satellite observations during one specific season. Therefore, we combined our data analysis with model simulations. We think both papers are complementary in this respect.

We have added a more complete discussion of the Jiang et al results and of our ob-

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jectives in this analysis: Section 1, end of 4th paragraph: "The MLS/Aura CO measurements have recently been used for the study of transport from Asia (Jiang et al., 2007). They analyze the variability in the measured upper tropospheric CO using emission climatology and meteorological conditions and show that upper tropospheric CO does not only depend on the surface emissions but also on the importance of deep-convection and intensity of horizontal winds. While surface emissions peak during the boreal spring, they find that the upper tropospheric CO is largest during the summer due to stronger convection. While previous studies focused on either nadir or limb observations, we examine the complementary views of the Asian outflow provided by these different observation techniques using global chemistry transport model simulations. Therefore, the solar occultation measurements from the ACE-FTS and the nadir observations from MOPITT and SCIAMACHY are analyzed, focusing on spring 2005."

Section 2.

I have several comments about this section. In fact, I am not sure I clearly understand the main purposes of this section. If the overall idea is to justify the scaling of the EDGAR inventory, then the Authors should say so.

The purpose of this section is to describe the model used for the analysis and provide a first evaluation compared to in situ observations. We think this is useful in order to have a basis for the comparisons with satellite observations. Therefore, we have decided to keep this section but added a sentence to motivate this short discussion.

Section 2, beginning of 3rd paragraph: "Before using the model simulations as an intermediate to better understand the information provided by the different satellite observations on the Asian outflow, we have first evaluated the CO simulations against in situ measurements."

Some more comments:

What do the Authors mean by "Different convection schemes" (page 1714, line 6-7). I

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assume this has not been done in the frame of this paper so please provide references.

For this study, we evaluated the performance of the model using two convection schemes implemented in LMDz-INCA: Tiedtke and Emanuel. After fixing a minor problem in our simulation for the submitted version of the paper, we have found out that the simulation using the Emanuel convection scheme seems actually closer to the observations, with in particular, larger variability of the CO simulated in the free troposphere. The differences between the two simulations (with different convection schemes) need to be more closely evaluated. Since this is beyond the scope of this paper, we have chosen here to remove this sentence in order to avoid any confusion.

Section 2, end of 1st paragraph: "The simulations were performed with the standard horizontal resolution of 3.75° in longitude x 2.5° in latitude on 19 vertical levels extending from the surface to 3 hPa, using Emanuel's convection scheme.";

Page 1713, line 23: Please define Asia.

The emissions have been scaled in Asia using the values and regions described in Petron et al. (2004). We have added a reference to this to avoid any confusion. "In order to minimize the effect of this underestimate, the total Asian emissions of CO have been scaled using the inverse modeling results of Pétron et al. (2004) for the corresponding regions, which was based on the MOPITT observations for the year 2001."

Page 1716, line 3-5. Please quantify what you mean by ";The agreement between the model and the surface data is very good". What does "very good" mean?

The reviewer is right to point that this does not provide useful information. We have rewritten this paragraph giving specific numbers.

Page 1716, line 9. Same comment than previously. The Authors say "the transport events are well captured". What does that mean? How could they say that from a comparison to the ESRL data?

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CO peaks in the data are well captured by the model, resulting in a good correlation in the timeseries. These peaks are mainly attributed to Asian outflow in the model, giving us confidence that transport events are well captured. We have stated this more clearly.

Page 1716, line 23-27. The argument that the MOZAIC data are representative of highly polluted regions that are not well resolved by the global models because of their low model resolution does not hold for the middle troposphere, in my opinion. In fact during their ascend/descend, the planes get further and further away from the highly polluted conditions that they can find in the boundary layer. Also, what are the implications of this underestimate for the comparison with satellites (see for example sensitivity of MOPITT to that region of the troposphere)?

The referee is right to highlight this point. In the free troposphere, MOZAIC can sample rather background conditions or transport events in the mid troposphere. The global model will underestimate transported plumes due to a too low resolution, but should be able to reproduce the averaged mid-tropospheric values.

This underestimated CO in the free troposphere is reflected in the underestimate compared to both satellite observations. This underestimate is most critical for MOPITT, which has its maximum sensitivity in the mid- troposphere.

We refer to this in the discussion of the satellite distributions in section 4.1.

Section 4

Page 1722, lines 25-27. Please include references for these processes.

We think that the reviewer is referring to Page 1721, lines 25-27. We have added references for previous studies of the main transport pathways for Asian outflow.

Page 1722, line 2. What do you mean by "vertical winds"? Are those associated to convection, orographic forcing? How would vertical winds over the western Pacific induce a transport of the pollution from the boundary layer to the free troposphere? Does

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that mean that the pollution has been already transported over the oceanic region? These questions are also connected (to some extent) to my remark mentioned above about the real contribution of this study to the understanding/quantification of the Asian pollution export.

The purpose here was to look at the information in the CO observations, and not a full meteorological analysis. However, we agree that the description of meteorological conditions is not precise enough; we have added a few sentences (Section 4, 1st paragraph).

Page 1723, line 4. Was the LMDz-INCA model included in the study of Shindell et al.?

Yes, LMDz-INCA was included in the Shindell et al. intercomparison. Its results were close to the multi-model average. We have mentioned this more specifically. We have mentioned this in the model description (Section 2, end of 2nd paragraph).

Page 1724, line 15-25. I wonder whether the Authors try to tell too much from these various satellite observations. Given the inherent uncertainties of satellite observations as well as the apparent systematic bias between SCIAMACHY and MOPITT, I wonder whether the BLR quantity that they defined can be interpreted in any quantitative manner. In addition, they have to apply a correcting value that is taken over "west-central Asia". What is west-central Asia? Why is that region chosen? In general, why should that value be correct if the emissions over Asia are incorrect?

We are discussing this part with much caution and mention that the analysis of the BLR can not be quantitative due to all current uncertainty. This study is a first attempt at combining two existing and published data products. A more in depth comparison of the SCIAMACHY and MOPITT retrievals is currently undertaken to better understand their consistency (or inconsistency). We have chosen to correct the bias using observations close to our area of interest but which does not have local emissions. We agree that this method is an approximation. But this study highlights the issue of consistency between the datasets for this kind of analyses. C.f. P. 1726, l.11-20.

We have chosen to give an approximate correction of the bias using a region with low emissions around 40°N and 85°E. This region was chosen because it corresponds to a remote area with low emissions, but still close to our region of interest here. The problem of consistency between SCIAMACHY and MOPITT is global, but it needs clearly more in depth investigation. This study is currently undertaken at SRON as part of a validation of the SCIAMACHY CO retrieval.

We make a more cautious statement (section 4.2, 3rd paragraph) : "We have attempted to remove this bias by assuming that the observed BLR over low CO regions far from the sources (chosen to be above west-central Asia, around 40°N, 85°E, a region close to our region of interest but with no emissions in the model) is equal to the model BLR."

And mentioned explicitly the uncertainties (section 4.2, 4th paragraph): "Considering the strong uncertainty of this approach, only a qualitative analysis can be undertaken at this point."

Page 1725, lines 3-6. Do the Authors imply that emissions may be underestimated by 37

This study is a first attempt. We gave this value as an indication of the results. We have added a comparison to recent inverse analyses, but also state that these results are not mature enough to be quantitative. C.f. P. 1726, l.11-20.

Page 1725, lines 7-8. Do they imply that all sources are too low in their model, including biomass burning and anthropogenic sources? Or is there an artifact in their methods which results that all source regions become apparent when plotting the BLR? Do they find similar (or different) results for other regions of the world?

We focused here on Asia and we did not attempt at discriminating between biomass burning as they are located in similar area, except for Siberia, which has low anthropogenic emissions. Using this scaling, we find that CO is overestimated in the model over the Eastern US, and Western Russia, but underestimated in the Southern Hemi-

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sphere biomass burning regions and North-Western US.

Page 1725, line 9-10. I may be mistaken but could not find any discussion about that on Section 4.1.

This is actually discussed in Section 4.3, we have corrected this error.

Section 4.3.

This section is a bit wordy (also includes some repetitions from previous sections to some extent) and (in my opinion) does not provide many new insights in terms of the trans-pacific transport of CO. I recommend that the Authors re-write this section in a more concise way, only focusing on new insights.

Following the reviewer's advice, we have rewritten most of this section.

Conclusion

As mentioned before, the conclusion needs to better highlight the new findings of their work with respect to the pollution export/transport.

Page 1731, line 15-17. The Authors say that "a more thorough analysis of the possible trends in the MOPITT CO...". Why don't they recommend the use of the BLR (rather than the entire MOPITT column) to derive trend in emissions (availability of data)?

This kind of BLR would be ideal to analyze trends in emissions; there is currently a lot of interest in the scientific community for these combined products. But we show the current limitations of the L2 retrievals: more work is required to better understand consistency between datasets, and eventually improve their consistency. We think this applied study provided an important step towards a better integration of the available satellite data. We make a more direct statement about these issues in the conclusion (end of 2nd paragraph and last paragraph).

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Answers to anonymous reviewer 2:

Abstract:

First sentence: The term "available satellite observations" may be a bit misleading as it can be misunderstood. It may give the wrong impression that the authors have used "all available" satellite data. That the authors have used available data is trivial. I suggest to replace available by, for example, "several complementary satellite observations" or equivalent.

As suggested by the referee, we have modified to "complementary satellite observations".

Page 1719, line 10: It is written that CO over low clouds is included but that this does not result in a bias. I would assume that if part of the CO is missing (the CO below the cloud) that this should result in too low columns, i.e., a low bias. Please clarify.

It is true that when CO over low clouds is included the part below the clouds is missing and that this results in a low bias. But this bias is not significant as is written in line 10 since the bias is within the instrument-noise error of the averaged CO measurements. We have also clarified this issue in the text, P. 9, l. 8: "Although over low clouds the lowest part of the CO total column is missing, the resulting bias remains within the instrumental signal to noise error, and no significant bias is found when comparing the averaged CO total columns to those based on the de Laat et al. (2006) selection criteria."

Figure 1: Unit of the emissions: Is it per year and per 3.75 deg x 2.5 deg grid cell ? Please clarify.

It is the total emissions per grid cell for the period March-April-May 2005. We have clarified this point in the legend.

Figure 2: The annotation is quite small (essentially unreadable in the printed version).

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We have modified the figure.

Figure 4: Three of the four maps shown in the first two rows show quite high CO over India, except SCIAMACHY (top right). What is the reason for this?

The fact that MOPITT is lower than SCIAMACHY over some areas seems to highlight a bias between the two measurements. This is particularly strong over India and the western part of the region plotted.

If we assume that both retrievals are correct: SCIAMACHY is closer to the true total column, which would mean that MOPITT is larger than the true state due to the retrieval method. If we use the formulae:

$XMOPITT = Ax + (I-A)xa$

It means that xmopitt is larger than the true state x if the apriori is larger. Since the a priori is relatively low, this kind of bias only happens in remote areas. This is unlikely here. Also, the study of the BLR in section 4.2 shows that even if we remove the contribution from the a priori from the MOPITT retrieval, we still have MOPITT total CO larger than SCIAMACHY above some areas. These inconstancies between the two products clearly need further investigation.

We have mentioned this point in Section 4.1 (last paragraph).

Figure 7: The correlation between MOPITT and the model looks suprisingly good. It this primarily because of topography combined with sampling ? Please clarify.

This good correlation is also seen in the comparisons with the surface data. Moreover, the regions chosen for the average are large enough to avoid major sampling effects. We therefore believe that the good correlation reflects the ability of the model to reproduce the main transport events.

Figure 9 and discussion on page 1726: I am not convinced that "good consistency" is an appropriate summary of the FTS and model comparison shown in Fig. 9. Apart

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from biases also the latitudinal variations seem to be quite different. I recommend to clarify this by providing, for example, correlation coefficients.

We thank the reviewer for pointing at this sentence, which is indeed not correct. We have modified the discussion of the comparisons.

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