

Reply to Reviewer comments for Manuscript ACPD-10-26361-2010

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We are grateful to the constructive and thorough comments of the reviewers. In our revision, we took into account all specific and technical comments by the reviewer as detailed below. In addition, we performed a number of minor technical/semantic corrections to improve the overall readability of the manuscript.

Anonymous Referee #1

The manuscript by Sodemann et al. uses two case studies to examine transport of a pollution plume across the Arctic in two different types of transport models. Aircraft and satellite data are used for model evaluation. Both models are found to be able to accurately simulate cross-polar transport, especially in the horizontal, although there is too much diffusion of fine structure in the Eulerian model. The authors conclude that the two types of models provide complementary information and are both necessary for analysis of pollution plume transport and processing.

This work is scientifically valuable, particularly in the context of the recent International Polar Year (and this special issue), and provides important results for interpreting model, aircraft, and satellite based studies of polar pollution transport. The paper is generally very well written and organized. I recommend this paper for publication in ACP once the following comments have been addressed.

Specific Comments:

1. In general, it would be useful to the reader to have a table or maps showing CO emission totals for the different simulations. The different values used across different simulations will impact the simulated concentrations, and remembering the relationships is non-trivial. Also, it would help for comparison to other (and future) studies.

In the revised manuscript, we include a table comparing the total emissions of anthropogenic and biomass burning emissions during the period of June and July 2010 from the FLEXPART and TOMCAT model. In the manuscript, we refer to the new Table 1 at several locations, including Sec. 2.1, 2.2, 3.1, 3.2, 3.4.

2. I don't see the point of including WRF-CHEM in the analysis. It is only used for one small part of the comparison, where the two main models are also used, it is never compared to the data, it doesn't show the ability to do anything the other models can't do, and it is discarded due to the low bias. If there is a good reason for including it, more justification and discussion of results are needed.

The main point of including WRF-Chem in the analysis was to investigate how increased horizontal resolution in an Eulerian model affects properties of the plume, in particular how numerical diffusion is different. Unfortunately, the strong low bias in the WRF-Chem simulation does not allow meaningful incorporation of the model results into the full suite of comparisons, including satellite and aircraft data, without compromising the readability of the paper and the visibility of the main point of the manuscript, and intercomparison of Eulerian and Lagrangian model results with observational data.

We thus decided to largely remove the WRF-Chem simulation results from the manuscript. This concerns Fig. 6 and the discussion in Sec. 3.1.3, as well as the methods section 2.3. We keep however a short paragraph in Sec. 3.1.3 pointing to the supporting evidence from the WRF-Chem simulation: "To investigate the effect of horizontal grid resolution on the shape of the pollution plume, an additional model simulation was conducted using the higher-resolution Eulerian model WRF-Chem (Weather Research and Forecasting model coupled with Chemistry) (Grell et al., 2005) for the period 25 June 2008 00 UTC 10 July 2008 18 UTC using 6-hourly 0.5°x0.5° ECMWF analysis as input data. The model domain covered the area north of 20°N at a horizontal grid resolution of 50 km, with 34 vertical levels up to 20 hPa. WRF-Chem simulated the shape of the pollution plume during passage over the North Pole as an intermediary between the FLEXPART and TOMCAT simulations (see Sodemann et al., 2010, Fig. 6). Numerical diffusion was clearly leading to lower gradients than the Lagrangian FLEXPART simulation, even though some more fine-scale structure could be retained than in the coarse-grid Eulerian TOMCAT simulation."

Pg 26368, line 14: In polar summer we would expect relatively high OH concentrations, leading to significant chemical destruction of CO. This should be mentioned and the implications discussed here or elsewhere in the paper.

We now briefly mention the role of OH in Sec. 3.1.1:

"Oxidation with the hydroxyl radical (OH) is an important, seasonally varying sink of CO in the atmosphere. During northern hemisphere summer, OH concentrations are about 10 times higher than during polar night ($10\text{-}15 \times 10^5$ molecules/cm³ vs $<1 \times 10^5$ molecules/cm³, Bey et al, 2001). OH concentrations in CTM simulations are however uncertain (Shindell et al., 2008). Typical global atmospheric lifetimes of CO against oxidation by OH on the order of 2 months (Fisher et al., 2010). Unlike the TOMCAT model, the removal of CO by the reaction with OH is not represented in the FLEXPART model simulation. However, adding the background CO profile partly takes this missing process into account implicitly."

Pg 26369, line 16: You should spell out and define CFL here; not all readers will know what it is.

We extended the description of the CFL criterion:

"For E-W advection the decreasing size of the boxes near the pole means that transport in this direction could violate the Courant-Friedrichs-Lewy (CFL) criterion, which specifies that the wind speed for a given timestep and grid size cannot exceed a certain value in order to maintain numerical stability. Typically, the grid box size has to be increased or the timestep decreased to overcome this problem."

Pg 26370, lines 23-26: WRF-Chem & FLEXPART emissions are compared to one another, but not to TOMCAT. This information would be useful in interpreting concentration results.

The details of the WRF-Chem model simulation have been removed from the revised manuscript. Instead, we now make a statement comparing the FLEXPART and TOMCAT emission inventories in Sec. 2.2:

"Note that emissions in July are generally lower than during other months of a year. Therefore, emissions used for TOMCAT are lower than the seasonally constant emissions from EDGAR used in the FLEXPART model (Table 1)."

Pg 26371: Time of IASI overpass is never mentioned, but the CALIPSO overpass time is given.

We amended the paragraph to provide information on the Metop orbit parameters:

"It provides near-global coverage twice per day on a 98.7°-inclination sun-synchronous polar orbit at about 817 km altitude. The local solar time at equator crossing is about 9:30 (ascending node) with a 29-day repeat cycle."

Pg 26371, lines 20-21: Can you be more explicit that you use only ONE (mean) AK for comparison of all model/satellite points? It took me a second read-through to realize this important point. Even though you show the AKs looking fairly similar during the study period, there will be some small error associated with the use of a mean. I don't think there's anything wrong with this methodology, but the reader needs to be aware.

The reviewer's comment prompted us to re-evaluate our method of averaging all averaging kernels (AKs) into a common AK. It turned out that the impact of using a mean AK is very significant for the model-satellite comparison. We now apply the daily mean AKs at each 1 degree latitude-longitude grid point to compare the models with the satellite observations. The result is that the agreement between both data sets has been considerably improved. We revised the description of Fig. 7 and the methodology section describing the use of the AKs accordingly.

Pg 26376, line 19: This would be a good place to discuss the implications of having no CO loss by OH in FLEXPART.

We added a brief discussion of the relevance of OH oxidation in this section, see answer above.

Pg 26379, section 3.2: Either here or in Section 2.4, it would be worth stating that IASI CO has been evaluated and performed successfully in the Arctic (Pommier et al., 2010).

We added a statement to this effect in Section 2.4:

"CO observations from the IASI instrument have been evaluated and were found to provide meaningful results in the Arctic (Pommier et al., 2010)"

Pg 26379, lines 16-17: If the models are sampled at the same space and time as the satellite observations, why are there no missing data patches in the model maps?

While some satellite retrievals are made and produce missing data due to impenetrable clouds, in the model world no such features exist and even in places where the satellite cannot make retrievals we can extract a total column CO value from the model data. We chose to show these in the model plots instead of gaps to provide a more continuous view of the total column CO field.

Pg 26380, line 3: You state that the middle (IASI) column in Fig. 7 uses a different color scale, but the color scale in the figure appears to be the same for all columns.

The middle color bar in Fig. 7 has been corrected.

Pg 26384, line 17: In the figures altitudes are shown in kilometers, not meters. Please change the altitudes in the text to km for consistency and ease of comparing description to illustration (here and elsewhere).

done

Pg 26385, line 27: "south of Greenland" – but the flight track shows the plane not ever going south of Greenland on this flight. Do you mean the southern part of the flight track, over Greenland? Please clarify.

This was corrected to "southern part of the flight track over Greenland".

Pg 26387, line 5: "Asia" forest fire – do you mean Siberian?

corrected to Siberian

Pg 26387, section 4.1: Much of this discussion doesn't seem relevant until the implications for pollution transport are given in the last sentence. I would suggest restructuring the section so that you first explain the relevance of these events to transport from mid- latitudes, then describe their occurrence.

We revised the section, starting with the sentence:

"The pollution transport across the north pole in association with low-pressure systems as shown here suggests that this could be an effective transport mechanism for polluted mid-latitude air to the Arctic atmosphere. "

Pg 26388, lines 19-21: A citation would be appropriate here (i.e., Zhang et al., 2009). Also, an emissions table as suggested above would be really helpful for this discussion, because the Streets inventory used in TOMCAT is updated relative to the EDGAR inventory. At the very least, you should remind the reader here that the two models do not use the same emissions, and that the TOMCAT emissions are more up-to-date.

A citation to Zhang et al., 2009 has been added. In the final manuscript, we revised this section, taking into account the information in the newly added Table 1, and pointing to the different emissions in the TOMCAT and FLEXPART model.

Pg 26390, lines 1-3: It would be worth reiterating that an additional drawback of the Lagrangian model is the lack of chemical production, and especially chemical loss.

We added a sentence:

"Chemical transformations, in particular loss of CO due to OH are typically not included in such simulations. "

Technical Corrections:

Pg 26363, line 13: Insert "The" before "main aspect"

corrected

Pg 26365, line 15: "counteracts" should be "counteract"

corrected

Pg 25365, line 19: "are" should be "is"

corrected

Pg 26370, line 1: Delete either "passes over" or "has passed over"

corrected

Pg 26370, line 16: "Modis" should be "MODIS"

corrected

Pg 26373, line 15: "20 percentile" should be "20th percentile"

corrected

Pg 26374, line 21: Text states tropopause is 2 pvu contour at 325 K, but figure caption states it is at 315 K.

corrected, it is 320K in both cases

Pg 26375, line 1: Text states 12:00 UTC but figure is labeled at 06:00 UTC

corrected to 6 UTC

Pg 26378, line 12: I think you either mean 7 July (not 8 July) or Fig. 6b, e, h (not 6a, d, g)

corrected to 7 July

Pg 26384, line 23: "dashed black line" – you mean gray shading Pg 26389, line 27: "a Eulerian" should be "an Eulerian"

corrected to gray shading

Fig. 1: Unclear why 1a comes before 1b when 1b is discussed first in the text. In 1b, are the gray lines just for retrievals during the analysis period in July 2008?

We exchanged Fig. 1 panels (a) and (b). The legend now states, as the main text, that the gray lines are local mean daytime AKs for 2 July 2008 north of 60 degrees N.

Fig. 2: Blue tropopause contour is very hard to see. Can you try another color (e.g. white, gray, etc.?) Also, both the text and the caption state that the two models use different color scales, but only one colorbar is shown!

We tried other colors for the TP, but blue with the underlaid white contour still appears as the best choice. Text and figure caption have been modified to mention only one color bar.

Fig. 3: I don't see white dashed 90% RH lines, only solid lines for both 80% and 90% RH (true of all other figures with RH lines)

Legend text has been corrected.

Fig. 7: The legend on the FLEXPART colorbar seems to be erroneous – values above 1100 are an order of magnitude too low. Also, is it possible to use gray to show missing data instead of white? Because white is included in your color scale for low values, it is hard to distinguish low CO from missing data.

This has been corrected. Missing data are now plotted in a light gray shading.

Figs. 9&10: (a) and (b) are swapped from what is described in the text and legends. Also, the CALIPSO profile plot shown to the right of (a) is not described in the legend, and AttCol and AttDep are never defined.

This has been corrected. The figure legend now reads (b) Mean profiles of attenuated color ratio (blue, AttCol) and attenuated depolarization (red, AttDep) in arbitrary units averaged over the segment marked by red lines in panel (a).