Atmos. Chem. Phys. Discuss., 8, S9233–S9240, 2008
www.atmos-chem-phys-discuss.net/8/S9233/2008/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Interannual variability of long-range transport as seen at the Mt. Bachelor Observatory" *by* D. R. Reidmiller et al.

D. R. Reidmiller et al.

Received and published: 18 November 2008

General Comments Springtime observations of CO in 2005 and 2006 at the Mt. Bachelor Observatory were analyzed in the context of global model simulations and observations from MOPITT and TES. The datasets analyzed are comprehensive and various possible reasons for the difference of CO between the two years are investigated. The inclusions of different datasets and viewpoints also bring a consistency problem in the paper. In a way, I like the fact that different views are expressed. It seems clear to me that the transport indices could not explain the observed CO variability. That is an improvement over the papers with too much emphasis on transport indices. However, after reading the paper, readers would have difficulty to identify quantitatively how each factor contributes to the observed changes. Even qualitatively, it is unclear which factor is most important. Based on the observations of MOPITT and TES, it seems to me that

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

emission change is more important. I think that this paper can be improved to be more scientifically substantial. Here are the inconsistencies I see in the paper.

REVIEWER 1. The LRT3 index shows a negative value of -0.3 in April 2005 and a positive value of 0.1 in April 2006. It would suggest that transport from East Asia is more in 2006 than 2005. Yet the observations in Figure 3 show that the largest CO increase in 2005 from 2006 is in April. And Section 7 specifically discusses rapid transport in April 2005. I don’t think that the LRT3 index discussion adds anything to the paper and suggest deletion of the LRT3 index.

RESPONSE 1. We agree and have deleted all mention of the LRT3 index.

REVIEWER 2. Table 1 shows the different ways of grouping data using water vapor. In the discussion, the variables used are monthly means and max or daily values of CO. If grouped data were not used in the analysis of CO changes, that section can be deleted.

RESPONSE 2. The main reason we include Table 1 and Sec. 3 is because this publication is part of the INTEX-B special issue and is the lone manuscript summarizing the multiple trace gas and aerosol measurements from MBO during that campaign. As such, we believe Table 1 and part of Sec. 3 should be retained. We have, however, eliminated the first paragraph of Sec. 3 discussing the various ways of segregating the data by water vapor / time-of-day as this was deemed to be extraneous.

REVIEWER 3. Figures 3 and 9 clearly show that the change from 2005 to 2006 in May is different in the MBO data compared to satellite observations. Satellite CO, and dust, ozone, and Hg data at MBO all show large increases in May 2006 than 2005. On balance, these data seem to suggest more transport from Asia in 2006 than 2005 at least in late spring. It may be that the tropospheric burden of CO is higher but surface CO is lower in 2006 than 2005, but why are there signals in surface dust, ozone, and Hg? It is just as likely that the change between the two years is in surface CO background level rather than transport from Asia.

ACPD

8, S9233–S9240, 2008

Interactive Comment

RESPONSE 3. We have clarified this discussion to show that in March and April, the MBO observations, GEOS-Chem model results, the backtrajectory-based transport index and vertical velocity fields all support our finding that 2005 had more East Asian transport of CO to the lower FT than 2006. In May, the situation appears to be reversed.

REVIEWER 4. This is another question for Figure 3. Does the total CO change over the Northeast Pacific simulated in GEOS-CHEM agree with the satellite measurements? If they look like the simulated changes at MBO (i.e., the simulated total CO changes do not agree with the observations), it probably suggests that transport is not the reason. The comparison with simulated “Asian” CO is misleading because the absolute magnitude of “Asian” CO change is probably much smaller than “total” CO. All the measurements are obviously for total CO.

RESPONSE 4. We did not sample GEOS-Chem over the NE Pacific (as was the case for the satellite retrievals); it was sampled directly at MBO. Therefore, we cannot speak to the exact question the reviewer poses in the beginning of this comment. However, we do note that results from GEOS-Chem (run with climatological emissions) suggest that transport is not the main cause of the interannual variability we observe. As a result, we investigate CO emission anomalies (specifically, wildfire emission from SE Asia) in detail in Sec. 7.

REVIEWER 5. MBO data did not show large amounts of aerosol scattering on April 12-17 or afterwards in 2005. The max is before April 12. That seems to be inconsistent with the model results in section 7. If the transport does not affect the MBO site, does it show up in satellite CO or MODIS AOD measurements?

RESPONSE 5. It is quite possible that trace gas emissions (such as CO) from biomass burning in SE Asia were transported to the NE Pacific region, while the smoke/carbonaceous particles did not make it all the way to the coast of the U.S. due to scavenging and/or deposition. However, in response to the reviewer’s comment, we have added Figure 12 and text in Sect. 7 which shows NCEP reanalysis

ACPD

8, S9233–S9240, 2008

FT wind fields for the period from 29 March – 5 April. Superimposed on this figure is a red box outlining the “India and Thailand” region Yurganov et al. (2008) used in their analysis of MOPITT- and AIRS-derived CO burden anomalies which showed significantly large anomalies in early spring 2005 due to wildfires in the region. The vector winds show a strong shift from pure easterlies to strong southwesterlies between 150 - 170 deg E which allowed for rapid transport of these emission to the west coast of the U.S. Figure 12b shows that this feature is a climatological anomaly, so the anomalous wildfire emissions from SE Asia, coupled with the anomalous transport pattern led to the observed CO enhancement. The earlier time of this NCEP wind field vs. the NAAPS aerosol transport model implies that the wildfire emissions from this region began at least in late March and lasted through mid-April. This is reflected in the CO timeseries at MBO (Fig. 4a) which shows CO beginning to increase in late March, peaking in early April and only gradually declining from mid-April through early May. Corresponding text has been added as paragraph 4 of Sect. 7.

Here are the specific comments.

REVIEWER (a) P. 16344, line 10-11. The sentence needs to be rewritten.

RESPONSE (a)We have rewritten this sentence to read: “Validation efforts were not as successful near Hawaii and over Anchorage, Alaska, where long-range transport of pollution created well-defined plumes that the aircraft specifically flew into but TES may not have passed over.”

REVIEWER (b) P. 16346, suggest deletion of the 2nd paragraph.

RESPONSE (b) As the reviewer suggests, we have eliminated all mention of the LRT3 index, including the aforementioned text as well as Table 2.

REVIEWER (c) P. 16350, section 4.3. A general question for this section is how representative is the MBO data? I would think that the satellite data are more regionally representative. If this is true, how should one weigh the surface site data compared to

ACPD

8, S9233–S9240, 2008

the satellite data?

RESPONSE (c) The MBO data seems to show an extreme in terms of regional representative-ness. This could be because of its altitude (i.e., sampling lower FT air vs. satellites which are most sensitive to CO in the mid- to upper-troposphere) or because of its geographic location near the coast, and so it samples long-range transported air that is less diluted than more inland sites. We sought to determine how regionally representative the observations at MBO were through our comparison with high altitude ESRL site data in the western U.S. While the MBO data shows changes of greater magnitudes than Wendover, UT or Niwot Ridge, CO all three sites do show a similar interannual change in CO for the months of interest. We have added text at the end of Sect. 4.3 to address/explain the difference between MBO and satellite observations.

REVIEWER (d) P. 16350, last lines. I do not understand how these two reasons can lead to the difference between satellite and site data. CO lifetime is long enough that sampling at a frequency of once a day should be fine. The a priori profiles should not affect significantly the relative change between the two years. The issue here is not just why the magnitudes are different. The hemispheric means of satellite data also show similar decreases. The question is again how to relate MBO data to satellite data.

RESPONSE (d) We intend not to show or attempt to compare the satellite measurements with the MBO observations directly (hence, we express the CO variability in terms of %, rather than in mixing ratios/concentrations). Rather, one dataset is meant to complement the other. Satellites capture “total column” information with limited vertical resolution. MBO samples at one point in the tropospheric column. So, while MBO shows that there were substantial changes in the lower FT, MOPITT and TES can speak to the broader scale changes throughout the tropospheric column near MBO and throughout the NH, in general. Indeed, the fact that MOPITT and TES observe similar – albeit smaller in magnitude - changes in CO as the measurements from MBO, suggests that there is some large scale emissions change(s)

ACPD

8, S9233–S9240, 2008

playing into this interannual variability as discussed by Yurganov et al. (2008) and in our manuscript in Sec. 7.

REVIEWER (e) P. 16354, line 5. This statement contradicts the LRT3 index in Table 1.

RESPONSE (e) We have eliminated all mention of the LRT3 index, so there is no longer a contradiction.

REVIEWER (f) P. 16354, line 19. Section 6.1 should be section 6. I do not think that this section shows that the April anomaly in 2005 is due to fire emissions. Why is the fire influence only in April not in March or May? Figure 10 seems to show that March has more burning than April.

RESPONSE (f) We have corrected the change to the section number. The fires may have been burning in late March, but the transport time of 5-10 days, would lead to CO enhancements in the western U.S. in early- to mid-April. By mid-April the fires appear to have decreased (Yurganov et al. 2008) and the lifetime of CO would have decreased as more OH sink would be present (given the increased daylight), so weakening fires, coupled with an increased sink and the changing synoptic pattern (Fig. 5 vs. Fig 9) would likely work in concert to show the temporal pattern demonstrated here (i.e., gradually falling CO concentrations from the peak in early April). We have also added text to the end of Sec. 7 and Fig. 14, which show transport pathways much earlier in the spring (29 March – 5 April) that allowed for wildfire emission from SE Asia to reach the west cost of the U.S. by early April.

REVIEWER (g) P. 16355, line 23-28. Is the grouping by water vapor used in the comparison of 2005 and 2006 data? The difference at the MBO site shown in Figure 3 is the monthly mean.

RESPONSE (g) No, the grouping of data by water vapor has not been used in the comparison of 2005 and 2006 data in this section (Sec. 5.2), though it is employed in Sec. 3 and Table 1. The second paragraph of Sec. 5.2 was intended to serve as a

ACPD

8, S9233–S9240, 2008

comparison to the analysis of MBO results during INTEX-B performed by Wolfe et al. (2007).

REVIEWER (h) P. 16356. Section 6. If both TES and MOPITT show a hemispheric decrease of CO from 2005 to 2006, that seems to suggest that emissions in the two years are different. Transport does not affect hemispheric means as much as emissions. So the change over the Northeast Pacific is mainly caused by emission change not transport.

RESPONSE (h) We do not disagree with this statement. In fact, we explicitly state that the NH changes observed via satellites speaks to some broad-scale changes (likely wildfire emissions). The findings of Tansey et al. (2008) - with respect to global biomass burning emissions - corroborate this. However, the changes on a hemispheric scale are much more nuanced than those seen locally over the NE Pacific and via in situ observations. These latter two facts coupled with the work of Yurganov et al. (2008) show that indeed there were emissions differences, but the transport index and NCEP reanalysis plots of vertical velocity fields over western North America (Figs. 5 and 9) prove there were transport differences as well.

REVIEWER (i) P. 16357, section 7. I would also look at the change of CO over Siberia. That region can have more biomass burning than the other three regions.

RESPONSE (i) Stohl et al. (2007) discussed the strong Russian / Siberian fires in spring 2006, but emissions from those wildfires were largely transported into the Arctic. We could not find any reports of anomalously strong Siberian fires in 2005.

REVIEWER (j) P. 16358, second paragraph. Can the model simulated transport events be related to the observations at MBO or by satellites? The event looks interesting, but the discussion does not seem relevant to the observations presented at MBO or over the Northeast Pacific.

RESPONSE (j) We have added to the discussion (paragraph 4 of Sect. 7) to put these

ACPD

8, S9233–S9240, 2008

Interactive Comment

model results more in the context of MBO observations; the modeled event is largely responsible for the enhanced CO observed at MBO in mid- to late-April (Fig. 4a). We have also added Fig.12 (500 hPa winds [and climatological anomaly] over the Pacific from NCEP reanalysis) which shows that this rapid Southeast Asia-to-Northeast Pacific transport pathway exists earlier in the spring (29 March – 5 April).

REVIEWER (k) P. 16359, line 21-25. The discussion of PAN does not belong to the conclusion section. The high temperature dependence of PAN decomposition can make it more difficult to understand the changes of PAN than CO, ozone, and dust.

RESPONSE (k) Valid point. We have excluded any mention of PAN from the conclusions.

** We appreciate the referee’s careful and thoughtful review of our manuscript. His/her insights and suggestions have largely been implemented and we feel make this manuscript much more scientifically robust.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 16335, 2008.