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

Anonymous Referee #2

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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 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.

(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.

(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.

(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.

(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.

(5) MBO data did not show large amounts of aerosol scattering on April 12-17 or af-

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terwards 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?

Here are the specific comments.

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

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

(3) 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 the satellite data?

(4) 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.

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

(6) 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.

(7) 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.

(8) 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.

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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.

(9) 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.

(10) 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.

(11) 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.

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