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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Model study of the cross-tropopause transport of biomass burning pollution" *by* B. N. Duncan et al.

B. N. Duncan et al.

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Response to Anonymous Referee #2

We are grateful for the careful comments of the reviewer. To strengthen our argument and address several of the reviewer's concerns, we have conducted two additional simulations, in which we separate the component of the seasonal oscillation of CO in the UT/LS due to variations in transport (i.e., convection and slow ascent from the TTL to the LS) from the variations in the individual contributions of each CO source. The first is a tagged CO simulation, which gives the breakdown of the total CO from various sources (e.g., methane oxidation, fossil fuels in E. Asia, biomass burning in S. America, etc.) in the UT/LS. The second is a uniform tracer simulation, in which we emit CO homogeneously over the earth's surface (2400 Tg CO/yr) and assume a uniform 25 day lifetime. Consequently, the uniform tracer is decoupled from the seasonal variation in sources, leaving only the variation due to transport. The results of these two simulations clearly show that both variations in CO sources and dynamics are important players in the composition and seasonal variation of CO in the tropical UT/LS.

In addition, we now present a comparison of CO from our simulation with MLS data in the UT/LS. We had access to only a few days of released data of the MLS (version 2/level 2) data product. Since the submission of this manuscript, we have obtained 100+ days of recently released data from the MLS team. We added a comparison of the MLS observations and simulated CO for March at 68, 100, 146, and 215 mb, which shows that the model does a reasonable job reproducing the horizontal distribution of observations at all levels. We also show a histogram of simulated and observed CO from 20N-20S for four months, which illustrates that our simulation has a similar distribution of CO as the observations. Overall, the comparison is favorable, lending confidence to our simulation. We also added a discussion of the limitations of the observations for the purposes of our study, such as the coarse vertical resolution and patchiness of the observations.

The major modifications to this revised manuscript are in 1) Section 5.2-3, where we discuss sources of variation in the UT/LS composition, 2) Section 3.3, where we include a comparison of MLS observations with our simulated CO, and 3) Conclusions.

> (1) The paper is somewhat conceptual. There is no attempt to compare the predictions of the evolution of CO following the 1997 Indonesian event with observations. It was not clear why this is - presumably surface station data is available that would be of some relevance.

We do not present an evaluation of the model simulation of the Indonesian wildfires as this has already been done in Duncan et al. (2003b). We say this more clearly now in Section 4.

>Similarly, the simulations of dynamically induced interannual variability in Section 5

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are not compared with observations, I guess partly because they refer only to the variability due to changes in dynamics. If surface data end up being of little use here, I am not sure what can be done.

We hope that we have addressed this concern with our uniform tracer simulation (discussed above) in Section 5.2 and our comparison of the simulation with MLS CO in Section 3.3.

>In the first part of the paper, a large number of CO measurements from surface stations and aircraft were used to make comparisons with the model. These sophisticated comparisons did demonstrate some model biases. It was unfortunate that the origin of the biases was not identified.

The source of the bias, a common problem in many CTMs, is uncertain as we discuss in Section 3.2.

>(2) In my view, the role of biomass burning in driving the CO tape recorder is overstated, in the sense of not being fully supported by the simulations and not being consistent with some recent work.

Our new simulations support our conclusions about the importance of biomass burning. The reviewer is referred to Section 5.2.

>In the abstract, it states: "The seasonal oscillation in CO in the TTL/LS (i.e. the CO "tape recorder") is caused largely by seasonal changes in biomass burning". While this statement is correct at 14 km, where there is a clear semi-annual cycle, it is probably marginally accurate at 17 km, and almost certainly wrong in the LS. I think there is compelling evidence that the seasonal cycles of ozone and CO in the lower tropical stratosphere have a common dynamic origin in the seasonal variation in upwelling. I am mainly referring to a recent preprint: Randel, W.J., M. Park and F. Wu, 2006: A large annual cycle in ozone above the tropical tropopause linked to the Brewer-Dobson circulation. I would encourage the authors to obtain a copy of this preprint to put their

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work in context.

We believe that the reviewer's statement that our conclusion is "certainly wrong in the LS" is not true as we now show more clearly with our two additional simulations. We have added the following paragraph to the conclusions:

"Both variations in the timing of CO sources (i.e., emissions and chemical production) and transport (i.e., to the UT by convection and within the TTL/LS by slow ascent) conspire to create a semi-annual oscillation of CO below the TTL that evolves into an annual oscillation in transit to the tropical LS. Seasonal biomass burning in local spring in each hemisphere creates a semi-annual oscillation in tropical CO. In boreal winter, between the SH and NH burning seasons, ascent from the TTL to the LS is seasonally high and CO in the northern tropics (i.e., from fossil fuels and NH burning sources) is at an annual maximum. Consequently, CO in the TTL/LS remains high between the two burning seasons. In boreal summer, between the NH and SH burning seasons, ascent is seasonally low and CO in the NH tropics is at an annual minimum. Therefore, CO during this time is at an annual minimum in the TTL/LS. Randel et al. ["A large annual cycle in ozone above the tropical tropopause linked to the Brewer-Dobson circulation", accepted to the J. of Atmos. Sci., 2007] used MLS CO data to conclude that the observed annual cycle provides strong evidence that the oscillation in the LS arises mainly from the annual cycle in the ascent rate from the TTL to the LS; that is, the impact of variations in the sources of CO are less important. By separating the transport and source contributions to CO in the UT/LS, we were able to show that both transport and sources are equally important contributors to the observed seasonal cycle."

>The importance of dynamics in contributing to the CO seasonal cycle is acknowledged on page 17 of Section 5.2, where it says "the tape recorder would exist without seasonal changes in CO sources". This seems at odds with the abstract, but perhaps could be quantified by looking at the seasonal variation of the flux of CO into the TTL, and showing whether or not it is in phase with the seasonal variation of CO, e.g. at 14

km.

We have removed the statement. As stated above, we hope that we better support our conclusions with the two additional simulations.

>2.3 Transport. I think this section is confusing.

Sorry for the confusion. Hopefully, we clarified the issue with a more accurate description: “A description of convective transport is given in the appendix of Rasch et al. (1997). The scheme uses the following meteorological fields as input: cloud mass fluxes, entrainment and detrainment fluxes, and large-scale downwelling. Both shallow and deep convection are considered, following the algorithms of Hack (1994) and Zhang and McFarlane (1995).”

>Section 3.1 page 8. "The model is typically higher from 30-40 latitude of both hemispheres,..". A model can’t be high or low. Also, "higher poleward of 30-40"? Ambiguous.

We changed “the model” to “the simulated ozone”. Our sentence concerning the high bias in simulated ozone is correct. We mean between 30 and 40 degrees latitude of both hemispheres.

>page 9. Just after 3.2. "... meteorology represents no particular time ..." Appears inconsistent with previous statements that winds and SST’s refer to 1994 - 1999.

Our statement is consistent with the GCM using observed SSTs, not winds. The complete sentence is: “Our evaluation is necessarily qualitative as we use climatological emissions and the meteorology represents no particular time, though it should capture IAV due to SSTs associated with the phases of the El Niño/Southern Oscillation (ENSO).”

>Section 3.2, page 9, "... as the seasonal maximum in the tropics ..." In Figure 1 Samoa has a CO maximum in October, Mauna Loa in March, other tropical stations have two maxima. What maximum is being referred to?

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We don’t agree. In general, the tropical stations experience one seasonal maximum, which is the result of seasonal biomass burning. The Seychelles station does show substantial IAV in both boreal winter and austral spring, which results in two maxima. In boreal winter, the station is impacted by pollution (e.g., biofuels) from the northern hemisphere and by southern hemispheric biomass burning in austral spring.

>Section 3.2, (page 9) If the low CO model bias during winter/spring is due to OH, it would be due to a high bias in model OH during winter/spring. Estimates of OH based on CH3CCl3 lifetime would give annual means, weighted toward the tropics and mid-lat summer when OH would be highest. I am not sure CH3CCl3 comparisons would be a good test of extratropical OH during winter where the bias starts.

We clarified this sentence in our revised manuscript: “..the lifetime provides less information on the quality of simulated OH outside of the lower tropospheric tropics; the rate constant for reaction of OH with CH3CCl3 is strongly temperature dependent. On the other hand, the rate constant for the reaction of OH with CO is only weakly pressure dependent. That is, CO loss is efficient throughout the tropical troposphere.”

>Section 4.1.1 "In general, the maximum extent of the upward convective mass flux in our model is Ÿ 200 mb". It would be interesting to see a profile of the tropical mean convective mass flux. Also, since the B-D circulation is on the order of 100 times smaller than the Hadley, only a tiny percentage of the Hadley mass flux need go above 200 mb to have a strong impact on the BD circulation. I would recommend showing a plot and/or quantifying this statement. This comment is related to the earlier comment in the paper that convective systems die out by 350 K. It would be useful if there was an attempt to make both statements more quantitative.

We added the following paragraph to the conclusions in Section 6: “Liu and Zipser (2005) and Rossow and Pearl (2007) found that very few tropical convective systems penetrate the tropopause, so this pathway is not likely important for CO and other trace gases with lifetimes longer than the time-scale of transport to the tropopause via slow

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ascent. Convection in our simulation did not penetrate the tropical tropopause, yet our simulation reproduces the distributions of MLS observations reasonably well; that is, this pathway is not needed to explain the large-scale features of the observations. However, it may become important locally near convective systems that do occasionally penetrate the tropical tropopause, such as those over central Africa, Indonesia, and South America (Liu and Zipser, 2005)."

>Section 4.2 (end). "In a situation of enhanced ozone, the LZH will descend in altitude..". This statement is made presumably on the basis of the additional ozone heating. However, it is not clear that the real atmosphere will respond in this way. One generally thinks of the upward mass flux in the lower tropical stratosphere as being externally constrained by momentum driving. If this is true also in the TTL, temperatures may increase in response to an O3 increase to keep the LZH near the same altitude.

We modified the sentence as we discuss the complexities of the impact of biomass burning trace gases and aerosols on the LZH in the 2nd paragraph of the Introduction: "A change in ozone in the TTL may impact the LZH, though this effect is not expected to be large for ozone (Gettelman et al., 2004)."

>Caption to Figure 10: are these climatological or for specific years?

Climatological biomass burning. We added this to the caption.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 2197, 2007.