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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.

Anonymous Referee #2

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Overall: This is an interesting paper on the transport of CO between the troposphere and stratosphere, and the impact of biomass burning on seasonal and interannual variability. I have two main concerns, and some more minor comments.

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

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.

(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.

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 work in context.

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.

Minor Comments

2.3 Transport. I think this section is confusing. For example, it says, "Convective transport is taken from the MATCH model, which uses the following meteorological fields as input: cloud mass fluxes, entrainment and detrainment fluxes, and large-scale

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downwelling." These variables would be sufficient to define convective transport, so it's not clear to the reader what exactly the MATCH model is doing. Perhaps these fluxes are obtained from the GEOS-4-GCM originally, but at longer time intervals (6 hourly?), so that the MATCH model interpolates these fluxes to the offline model timesteps, or does the convective transport for the various species? If so, what is the convective scheme of GEOS-4-GCM? Is it the same as the Hack (shallow) and Z and M (deep) schemes of MATCH? If there is an inconsistency, what are the implications?

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.

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.

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?

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.

Section 4.1.1 "In general, the maximum extent of the upward convective mass flux in our model is ~ 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

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an attempt to make both statements more quantitative.

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.

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

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