Atmos. Chem. Phys. Discuss., 7, S733–S736, 2007
www.atmos-chem-phys-discuss.net/7/S733/2007/

Atmospheric Chemistry and Physics Discussions

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7, S733–S736, 2007

Interactive Comment

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

Anonymous Referee #1

Received and published: 24 March 2007

Review by S. Fueglistaler.

General

Duncan et al. present a model study of the transport pathways of biomass burning pollution - essentially carbon monoxide - to the stratosphere. They use a chemical transport model coupled to a GCM which was forced with sea-surface temperatures of 1994-1998. The paper presents a wealth of information and interesting results. However, I believe the structure of the paper, and the layout of the results, could be improved. I recommend publication in ACP subject to the following concerns.

From the beginning on, the reader is asserted that deep convective transport may not be overly important for transport across the tropical tropopause. The papers quoted to support that assertion, notably the Fueglistaler et al. [2004] study, however, have *not* demonstrated that deep convective transport is not of importance under any circumstances. Rather, they - and subsequent work by Fueglistaler et al. [2005] - showed that no special mechanisms of dehydration tied to deep convection need to be invoked to understand stratospheric water vapour. For a tracer like carbon monoxide, however, there may be a different story. In fact, carbon monoxide is used frequently to study deep convective outflow. Thus, the present paper should demonstrate, rather than assume a-priori, that the transport as represented by the model parameterization of convection is sufficient to explain observations.

It appears also somewhat surprising that model results are not directly compared to (MLS) observations in the TTL. I do not think that referring to the Schoeberl et al. [2005] paper is sufficient. It is understood that the model period (1994-98) does not overlap with the MLS/Aura measurement period. However, I would think that climatological features, such as a 5-year average 100 hPa CO field for, say DJF and JJA, would be similar to an average over the MLS/Aura period (provided the model is correct). Having such a comparison would help to value the significance of the discussed patterns, and corresponding tranport routes.

Finally, I consider the structure of the paper somewhat less than optimal. Instead of starting with a (ENSO) perturbation study, I'd suggest first a discussion of the climatological mean state along with a comparison to observations as said above. That comparison should also give indications whether convective transport as implemented in the model is sufficient, or whether there are systematic biases that could point to problems with convective transport. Then, one could proceed to discuss how the 'CO tape recorder' signal arises in the model calculations, and finally present the perturbations associated with the strong El-Nino.

Minor comments

It is certainly not the authors' fault that currently there is no consensus definition of the

ACPD

7, S733–S736, 2007

‘TTL’. However, I’d suggest to replace the awkward ‘TTL/LS’ with the more widely used ’UT/LS’.

In the abstract, and elsewhere, you postulate that ‘convection was stronger’ during the El-Nino phase. I assume this does not mean ’more vigorous’, i.e. higher, but presumably refers to the strength of the Hadley-cell and can be seen in the streamfunctions? Is this really so? You may also want to check for publications searching for such a signal in analysed data.

p2200/l16: You may want to be a bit more specific rather than saying ’etc.’. Also, Haynes and Shuckburgh [2000] may be a useful reference with regards to transport across the subtropical jets.

p2201/l5: As said above, a statement like ’We do not believe ...’ is not very helpful here, rather, this should be a point the paper can demonstrate.

p2203/l18: Certainly, specifying a seasonal cycle in anthropogenic emissions, even if north of 35 degrees, is a delicate thing to do in the context of explaining the CO tape recorder, and a few words about its impact may be appropriate here.

p2207/l14: I am not sure I understand what you say here - why is the lifetime ’weighted toward the lower tropical troposphere’? I assume that for each level there is a different lifetime?

p2208/l1: Looking at Figure 2, I see only enhanced variability, but not interannual variations. To support your claim that that variability is due to interannual variations you would need to plot the standard deviation of monthly means - or is this what is shown in Fig2? In any case, a better description of what ’range of observations’ means may be appropriate.

p2216/l22: A statement like ’more CO crosses the tropopause ... as the ascent rate is higher ... a tape-recorder would exist without changes in tropospheric CO sources’ is prone to lead to confusion; and is probably even wrong: A stronger upwelling indeed implies a larger CO mass flux, but it does only indirectly affect CO *mixing ratios*, which is what the term 'tape recorder' is referring to. Upwelling and CO *mixing ratios* are only coupled through the modification of time to reach a certain altitude. Please clarify this in the text.

p2217/l8: I do not understand what you want to say here. I assume the main upwelling occurs in the tropics, and what you observe in the LMS is outflow from there? Why should then the LMS contribute to the 'tape recorder'?

p2218/l14: Again, did the El-Nino enhance *air mass transport* or *CO* transport to the TTL? In the former case, I'd like to see the streamfunctions.

l2221/p4: Note that the Rosenlof (1995) paper refers mainly to the stratosphere, whereas you are probably thinking more of the 150-100hPa layer.

References

Fueglistaler, S., M. Bonazzola, P.H. Haynes, T. Peter, Stratospheric water vapor predicted from the Lagrangian temperature history of air entering the stratosphere in the tropics J. Geophys. Res., 110 (D8), D08107, doi:10.1029/2004JD005516, 2005.

Haynes, P.H., E. Shuckburgh, Effective diffusivity as a diagnostic of atmospheric transport. 2. Troposphere and lower stratosphere, J. Geophys. Res., 105 (D18), 22795–22810,

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