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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Observationally derived transport diagnostics for the lowermost stratosphere and their application to the GMI chemistry and transport model" *by* S. E. Strahan et al.

S. E. Strahan et al.

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This author comment contains responses to all three reviewers of our manuscript.

Response to Referee 1

This was a very helpful review. The comments/suggestions regarding the figures and their labeling have resulted in substantial changes to the figures which I think improve them considerably. I can't do anything about how small the figures appeared. They are small because of the half-size pages that ACPD creates during the publication process. ACP uses full size pages so this should not be a problem.

Introduction. I do not agree that the first 3 paragraphs can be condensed into 1. The intro has 5 paragraphs and they each serve a purpose: 1. Why do we care about evaluating the UT/LS? Because tropospheric pollutants can affect chemistry and composition there. 2. How does tropospheric air get into the stratosphere? 3. What large scale processes affect LMS composition? 4. Why there is a need for diagnostics of this region and what previous work has addressed this? 5. What the reader will find in this paper.

I have streamlined the Introduction and it is now 1/6 shorter, but I believe all of the content is important for introducing the paper.

Clarity of figures 3, 4, and 6. I have made the improvements suggested by the reviewer. Black backgrounds are gone. White lines and dots changed to black. The ER-2 data spread arose because a range of latitudes were shown, which I had failed to mention in the text. Color bars for the pdfs have been added. Figure 6 is completely redone, with solid lines for the means and shaded areas for the range. It is now much easier to compare the model and MLS.

Figure 5. You're right. What I should have said, and now have said, is that the N2O isopleths are much closer to following the dynamical tropopause than they are to following isentropic surfaces. The point is that isentropic transport is not happening. I have changed the bottom row to show the difference between fall and spring rather than the ratio. Instead of adding a third column showing differences between SPURT and the model, I have overlaid the model differences on the SPURT differences (bottom row). I think this makes it easier to see that the model has a comparable composition change below 360K during summer. For now, this will have to remain a qualitative test. I have some ideas on making it a little more quantitative but nothing I'm ready to publish yet.

Figure 7 and discussions. I agree that as a diagnostic tool, this figure was less than ideal. I have chosen to use a slightly different diagnostic for the thickness of the tropopause layer based on CO distributions as a function of distance from the

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tropopause. I think both the figure and the method for evaluating the model are clearer. The basis for this is still the Hoor et al 2004 analysis that showed that the tropopause mixed layer extends to 25K above the dynamical tropopause at all extratropical latitudes in all seasons. The evaluation (Figure 7) is done using histograms of model CO in 3 different layers (below, at, and above the tropopause). It is now easy to judge whether the model CO pdfs appear tropospheric, stratospheric, or mixed. Also, I do not have access to the full SPURT CO data set and meteorological analyses that go with the aircraft flights (including tropopause heights) which I would need if I were to plot the SPURT CO data along with the model results. Therefore, this diagnostic is based on the results of Hoor 2004 rather than the data itself.

Figure 8 and discussions. Displaying SPURT or other CO2 data does not help in model evaluation. Aircraft data have been collected over decades, and with the CO2 growth rate and temporally intermittent sets of aircraft observations, plotting observations over model results is not helpful - they are usually not directly comparable. It makes more sense to use the results of various published CO2 analyses. Those studies determined cycle phases and amplitudes from years of sporadic measurements, identifying key features such as gradient reversal or phase and amplitude change across the tropopause. These features, which are signatures of transport processes, are the bases for model diagnostics. This section is expanded to make this clear. Text has been added to explain how the model curves were produced.

Thanks for the TTL references. Sherwood and Dessler define the topic of the TTL as ranging from the level of zero heating (˜355K or 14 km) to the maximum reach of convection, which they say is about 70 mb (420-450K). Gettelman and Forster see it as ranging from the minimum lapse rate (10-12 km) to the cold point tropopause (16-17 km). These are two very different TTLs, but the Gettelman and Forster [1998] and Folkins [2002] seem most relevant here and have been added.

The Eyring reference was an unintentional omission, glad you caught it.

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A reference for CO lifetimes has been added to section 3.3.1 (Duncan et al., 2007).

Response to Referee 2

Length of introduction: Referee 1 is right that two of the purposes of the intro are to establish the concept of the LMS and the relevant transport pathways. There is a 3rd purpose, and that is to provide motivation for this study. The motivation is in paragraph 1: tropospheric pollutants can affect composition of the LMS, and a few examples are cited. I believe it is essential in every publication to show the motivation for the work, and in this case it is 'why do we care about the composition of the LMS and how well we can model it?'. There are 5 paragraphs and they each serve a purpose; I have shortened the section by 1/6th, but I cannot eliminate any paragraph.

The referee comments that the 2nd paragraph doesn't refer to a number of small scale, cross-tropopause transport processes. These processes are intermittent in time and space, and while they cause some STE, they do not fundamentally control the composition of the lowermost stratosphere. The Fischer et al [2003] reference noted by the referee says that convective injection into the midlatitude lowermost stratosphere has only rarely been reported, and suggests himself it is not a major contributor to STE. The first sentence of my 2nd paragraph states that there are two major transport pathways. The most significant forces in the LMS are the Brewer-Dobson circulation and the seasonally varying horizontal transport driven by monsoons. This paper is about evaluating the large scale phenomena because we need to first understand how well models get the big picture right before trying to evaluate finer details (e.g., midlatitude convective influence on the lowermost stratosphere).

Comments about the use of UT/LMS and suggested use of UT/LS: I have decided that 'middleworld' is an even better choice and has replace uses of UT/LMS.

Section 3.1. The text states that the model surface boundary condition is derived from global surface observations. This is also stated in the caption for panel A. The results shown are not from the same year as B96. I am using results of the B96 analysis

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to evaluate the model, not the actual ER2 data. The B96 analysis showed certain features in the phase and amplitude of the cycles; the actual mixing ratios are not relevant. Also, B96 used data from several years of observations in different months (but far from all 12 months) and they do curve-fitting to determine phase and amplitude. Thus, plotting their observations does not help evaluate a model. The annual trend in the model output was removed in order to make it easy to see the actual seasonal cycle amplitude - the y-axis on the plots says 'Detrended CO2', but ACPD shrank this figure so much I'm not surprised you didn't notice it. Panel C does not have the trend removed because it does not interfere with the comparison of the two cycles. These details are stated in the caption.

The referee suggests moving the Hoor et al 2004 reference to the end of the discussion, but this is the end of the discussion, although there is a different CO2 discussion in section 3.3. The Hoor reference is relevant here because both they and B96 used a similar vertical coordinate for their analyses, which is quite relevant to their results. I've changed a few words to make this connection clearer.

Section 3.2. You're right that the CO description does not belong here; this is an artifact of a previous version of the paper. It's moved to Section 3.3.1.

References to Krebsbach and Hegglin have been added.

MLS has daily coverage from 82oS-82oN (added). It is now noted in the first paragraph of this section that equivalent latitude coordinates are used for all aircraft data because it eliminates bias due to limited longitudinal sampling of the aircraft. Equivalent latitude expands the sampling range of the aircraft data nearly to the pole, which you well know from your own SPURT analyses.

Section 3.2.1 is now titled 'N2O' in parallel with Section 3.2.2 'Ozone'.

Figure 3. Colorbar added. All pdfs are area-weighted and this is now stated in the text and caption. The aircraft profiles can be examined at high latitudes because of the use

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of equivalent latitude. This is now explained in Section 3.2. A high latitude comparison was chosen because descent is greater at high rather than mid latitudes, so the high latitude LS has a greater influence on LMS composition. This makes it a more stringent test of model transport.

Figure 4 has been changed at the suggestions of Referee 1. You may find the aircraft data comparisons easier to see now. Thanks for the suggestion about using vertical profiles with respect to the tropopause. It could be useful for CCMVal applications, but I do not wish to add another diagnostic to this paper.

'Wobble' has been changed to 'wave-driven vortex displacements' to clarify. Missing from this paragraph was a mention of the latitude range for the figure, which is 66-82N. As these latitudes span the vortex edge, clearly there is going to be a lot of variability in the pdfs from the motions of the vortex. The suggested rewording regarding vortex breakdown and homogenization is barely different from what is already written. I have added 'homogenization of long-lived trace gases' to make it clear I am talking about trace gases rather than some other kind of meteorological homogenization.

p. 1459, l5. I do not see this as a contradiction of Chen. Meridional transport is occurring, it's just not following isentropes. It is quasi-horizontal transport that roughly follows PV contours.

The Logan reference is relevant to Section 3.2.2 and has been added.

Section 3.3 (Strat-trop coupling). Partly in response to Referee 1's comments about Figure 7 (CO-O3 scatterplots), I have chosen to use a slightly different diagnostic for the thickness of the tropopause layer. I think both the figure and the method for evaluating the model are now clearer. The basis for this section is still the Hoor et al 2004 analysis that showed that the tropopause mixed layer extends to 25K above the dynamical tropopause at all extratropical latitudes in all seasons. The evaluation (Figure 7) is done using histograms of model CO in 3 different layers (below, at, and above the tropopause). The model CO histograms are easy to evaluate (that is, to deter-

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mine whether they look tropospheric, stratospheric, or mixed). The Pan 2007 paper is referenced here because they also examine the thickness of the mixed layer.

Also, I do not have access to the full SPURT CO data set and meteorological analyses that go with the aircraft flights, which I would need if I were to plot the SPURT CO data along with the model results. Therefore, this diagnostic is based on the results of Hoor 2004.

Section 3.3.2 CO2 (Figure 8). Displaying SPURT or other CO2 data does not help in model evaluation. Aircraft data have been collected over decades, and with CO2 growth rates and temporally intermittent sets of aircraft observations, plotting observations over model results is not helpful - they are usually not directly comparable. It makes more sense to use the results of various published CO2 analyses. Those studies determined phases and cycles from years of sporadic measurements, identifying key features such as gradient reversal or phase and amplitude change across the tropopause. These features, which are signatures of transport processes, are the bases for model diagnostics. This section is expanded to make this clear.

Section 4, Diagnostics Summary. I cannot reference either of the Pan papers for the consistent thickness of the mixed layer because she did not look at seasonal variations in its thickness. Pan 2007 is referenced in Section 3.3.1 when diagnostics to gauge the thickness are discussed. Hegglin 2006 is added to the table.

p. 1463, l20. references added

p. 1463, l4. The O3 seasonal cycle is a little low only in winter, while the CO2 and N2O are consistent with too much exchange in summer. If too much exchange is also going on in winter, we might expect to see this in the polar N2O, but we don't.

p. 1464, l27. In the section discussing the O3 comparison, I have added more information on MLS uncertainties at 350K (215 hPa). They are large, so the agreement seen is definitely within the instrument uncertainty. And Logan's and Folkins' ozonesonde analyses (also mentioned here) support tropical O3 at 350K that looks more like the model than MLS.

All the line by line technical corrections: done.

Response to Referee 3

p.3, TTL and zero radiative heating level. There seems to be some disagreement in the literature about where the TTL is. While Sherwood and Dessler [2000] put the base of the TTL at the level of zero heating, Folkins, Gettelman, and others put the base of the TTL at the level where convective outflow becomes small, which seems to be in the 12-14 km range. The TTL I'm talking about is the latter because this is where cross-isentropic middleworld transport is taking place. I have reworded this paragraph and added new references to make clear my usage of the TTL.

p. 4. Although we don't talk about water vapor, I agree that the Dessler et al. reference is relevant to pathways of tracer transport into the LMS.

Figure 2 and related text. I got rid of the dashed black line which really wasn't necessary. I now reiterate in the text that the Mauna Loa/Samoa average is a boundary condition forced in the model that is derived from observations. As stated in Section 2, model CO2 is forced by a 17-year time series derived from observed mixing ratios - thus, there is no way to compare model and obs at the surface. The caption is now consistent with the text.

You're right there could be some variation in the transit time from the surface (e.g., Folkins et al, GRL 2006), with a shorter time (40 days) in NH winter and longer time (70 days) in NH summer. Panel A (time series at the surface ant 380K) show a consistent 2-month time lag, give or take a few weeks. To look for seasonal variations in this lag we would need to do some kind of tagged tracer experiment. Perhaps that will be a future diagnostic.

Thanks for pointing out the Folkins ozonesonde analysis. I have added a mention of

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this, as well as a mention of the Logan [1999] paper which also shows measurements of tropical ozone in the UT.

Last sentence: I did not properly caveat my claims about what the model can do, and I appreciate your pointing this out. The transport evaluated in this paper is necessary to do the sort of simulations mentioned, but it is not all that is necessary. I definitely agree that there are numerous important chemical and possibly microphysical issues that the model must get right. This paper is only evaluating one of several processes necessary for such simulations. We learn in this study that model tropospheric emissions can perturb stratospheric composition and chemistry. If the emissions have lifetimes of a few months (e.g., CO), convection need only transport the emissions to the TTL and the BD circulation will do the rest. This is discussed in detail in a related paper also submitted to ACPD (Duncan et al.).

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 1449, 2007.