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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on “The governing processes and timescales of stratosphere-to-troposphere transport and its contribution to ozone in the Arctic troposphere” *by* Q. Liang et al.

Anonymous Referee #2

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This paper uses a GCM-CTM to simulate stratospheric and tropospheric chemistry over a 2-year period with the goal of quantifying the processes and timescales of STT and its contribution to ozone in the Arctic troposphere. The topic is highly relevant to ACP and is very timely given the recent interest in the Arctic sparked by IPY. Overall the authors have succeeded in providing a realistic model simulation of Arctic chemistry and transport, however I find much of the interpretation of the results to be inaccurate. I think the interpretation can be greatly improved as outlined in my comments below. Once the paper undergoes a major revision it should be acceptable for publication in ACP.

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Major concerns:

1) This paper appears to have a flaw in its fundamental description of STE processes in the extratropics. The authors seem to imply on page 19389, page 19392 and Figure 5 that in the polar regions there is a slow, downward, diabatic descent of stratospheric air across the tropopause and down through the troposphere. As discussed below, I can find no physical basis for this process being a significant STT transport pathway. I would like the authors to either point out the evidence for such a process (either through previous studies, or from tracer studies in the current work), or remove this process from the paper and explore other mechanisms that can produce the observed distribution of modeled stratospheric tracers.

More specifically:

Page 19388-19389: Here, citing Stohl et al. [2001] and Haynes et al. [1991], the authors imply that on the large scale, it is the slow transport processes that control the STE flux as determined by the downward control principle. It seems to me that the authors use this perception of the downward control principle to explain the 1-3 month transport times of the stratospheric tracer to the upper, mid- and lower troposphere.

However, the following paper: Stohl, A., et al., Stratosphere-troposphere exchange: A review, and what we have learned from STACCATO, J. Geophys. Res., 108(D12), 8516, doi:10.1029/2002JD002490, 2003.

provides a very good review of STE processes. Nowhere in the paper could I find mention of “slow transport processes that control the STE flux as determined by the downward control principle.” Stohl et al. [2003] point out the downward control principle (which was originally just a thought experiment in the original Haynes 1991 paper) applies to a control surface at the top of the lowermost stratosphere and is only partly relevant for the mass transport at the tropopause level where other processes contribute to control STE. Stohl et al. [2003] only describe tropopause folds, cutoff lows and gravity waves as being important for STT. This is consistent with my own review of the literature. While Holton et al. [1995] and Appenzeller et al. [1996] have shown that diabatic descent is an important process between the overworld and the lowermost stratosphere, I can think of no examples of slow, diffuse, diabatic downward transport of stratospheric ozone across the tropopause in the polar regions. STT transport is almost entirely associated with episodic transport along isentropes that extend from the stratosphere into the troposphere (see the classic Figure 3 of Holton et al. 1995, which only shows isentropic transport across the tropopause, not diabatic descent across the tropopause). The one month average time that it takes for the ozone to cross the tropopause in the current study is just the average of many individual intrusion events that occur quite quickly (see Stohl et al. 2003), and not the result of slow diffusion. The many intrusions allow for the accumulation of the tracer in the upper troposphere, having a cumulative peak after about one month. The authors need to clarify what they mean by the average transport times from the stratosphere to the upper, mid- and lower troposphere.

Furthermore when discussing the average circulation between the stratosphere and troposphere, Stohl et al. (2003) A NEW PERSPECTIVE OF STRATOSPHERE–TROPOSPHERE EXCHANGE. Bull. American Met. Society, state the following: “This two-dimensional picture may suggest that STE in the extratropics is a continuous downward flow. However, actual STE is highly episodic, associated with strong mesoscale perturbations of the tropopause”

Does the GMI CTM actually diagnose diabatic descent? I wonder if an apparent diabatic descent across the tropopause is just an artifact of relatively coarse model resolution near the tropopause (the authors need to provide information on the horizontal and vertical grid spacing of the model). If the model is relatively coarse near the Arctic tropopause then it may not resolve the strong temperature gradient at the tropopause. The model may than allow for artificial mixing of UT and LS air in the model layer that straddles the tropopause, which then erroneously transports LS air into the UT, giving the appearance of diabatic descent. Is there any in situ evidence for diabatic descent

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from TOPSE or the recent ARCTAS flights of the DC3?

Appenzeller, C., J. R. Holton, and K. H. Rosenlof, Seasonal variation of mass transport across the tropopause, J. Geophys. Res., 101, 15,071–15,078, 1996

2) Figure 5 seems to imply that 20-25% of the air that is transported downwards across the tropopause in the polar regions remains in the polar regions over 3 months as it descends into the lower tropopsphere, as if there exists some type of quasi-permanent Arctic tropospheric vortex. This seems very unlikely given the transport patterns across the Arctic. Stohl [2006] provides a very nice analysis of transport patterns and lifetimes in the Arctic, for tracers of stratospheric air and for anthropogenic pollutants. He concludes that the average age of an air parcel north of 80 N is 1-2 weeks at the surface and much less at higher altitudes, only about 3 days in the upper troposphere. This was clear to the many researchers who visited the Arctic this past spring for the various air pollution studies. Polluted air masses from the mid-latitudes frequently traversed the Arctic leaving little time for air masses to become isolated and well aged. Therefore the stratospheric tracers in the present study that enter the troposphere in the Arctic are quickly advected out of the Arctic and then travel far and wide across the northern hemisphere as they descend through the troposphere and become well mixed with stratospheric tracers that entered the troposphere at lower latitudes. A comparison between the present study and Stohl [2006] regarding transport times and STE needs to be included in the manuscript.

Stohl, A. (2006), Characteristics of atmospheric transport into the Arctic troposphere, J. Geophys. Res., 111, D11306, doi:10.1029/2005JD006888.

3) Throughout the paper the authors discuss the monthly time scale that it takes for STT tracers to descend from one layer of the troposphere to the next, or from the Arctic to the mid-latitudes. The way these results are presented it makes it seem like the atmosphere is a slow sluggish environment with long transport times. In reality the atmosphere has quite rapid transport times, it’s just that it takes about a month

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for the STT maximum to register at the lower levels for several reasons: 1) the STT maximum flux is spread over many weeks in the winter and spring; 2) deep intrusions rapidly transport tracers to the lower troposphere; 3) shallow intrusions only reach the upper troposphere, 4) intrusions that descend and reach low latitudes become diluted and are eventually transported poleward in ascending airstreams; 5) different species have different chemical lifetimes. These 5 points are all touched on in the paper and it’s clear that the authors are aware of these processes, but it would make the paper much easier to follow if all of these processes could be reviewed in one location, along with a clear explanation that the one-month transport times are the result of these many varying and sometimes competing processes.

4) The authors need to carefully consider and reference the findings in two papers that are highly relevant to the present study.

James et al., [2003] used the FLEXPART Lagrangian particle dispersion model to study global STE processes. They discuss 1) the tropospheric distribution of stratospheric tracers with ages varying from 1 to greater than 90 days, 2) the regions most strongly impacted by stratospheric intrusions and 3) the seasonal variation of stratospheric ozone in the mid-latitudes (but with lifetimes less than 30 days).

James, P., A. Stohl, C. Forster, S. Eckhardt, P. Seibert, and A. Frank, A 15-year climatology of stratosphere–troposphere exchange with a Lagrangian particle dispersion model, 2, Mean climate and seasonal variability, J. Geophys. Res., 108(D12), 8522, doi:10.1029/2002JD002639, 2003.

Another paper (now classic) that is of interest for Arctic STT mechanisms is Shapiro et al. [1987]. SHAPIRO MA, HAMPEL T, KRUEGER AJ, THE ARCTIC TROPOPAUSE FOLD, MONTHLY WEATHER REVIEW Volume: 115 Issue: 2 Pages: 444-454, 1987

Specific comments:

Introduction: When discussing the cause of the springtime ozone maximum please

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also reference: Stohl et al. (2003) A NEW PERSPECTIVE OF STRATOSPHERE–TROPOSPHERE EXCHANGE. Bull. American Met. Society, which concludes that the springtime ozone maximum cannot be sue to STT alone because STT peaks earlier than the ozone maximum. This same Stohl paper is also the first (to the best of my knowledge) to recommended that STE be divided into STT and TST, as was mentioned in the present study on page 19381.

Section 2, Model description: Please provide the model horizontal and vertical resolution as well as the model time step for both calculations and output.

page 19383 What is the definition of the tropopause in this paper?

page 19384 Does the amplitude of the dynamic tracers range from 50 to 150 ppbv?

page 19386 What is the sampling time of the MkIV measurements?

page 19387 Please indicate the sampling frequency of ACE and the spatial coverage

page 19389 use STT instead of STE.

page 19389, lines 22-24 Here the text gives the impression that the only transport pathway back to high latitudes is via warm and cold conveyor belts, whereas poleward quasi-isentropic transport commonly occurs outside of these airstreams. Actually the CCB probably isn’t that important for transporting air from low latitudes to high latitudes as it originates poleward of the warm front which is already in the mid-latitudes, whereas the WCB originates in the sub-tropics.

page 19390 lines10-15 I am not convinced by the argument that STT of DTfix is delayed by one month in the poles as compared to the mid-latitudes. To me it seems that STT in the polar regions is much less than in the mid-latitudes, and the one month delay in the maximum occurring at high latitudes could just be the integrated effect of the time required to transport intrusions from the mid-latitudes to the high latitudes. Testing this hypothesis requires separate DTfix tracers for high latitudes and mid-latitudes

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page 19390 lines 21-22 If the seasonal variation of the 4-month and 6-month tracers are similar, doesn’t this imply that 4 months is the maximum amount of time that is required for a STT tracer to become well-mixed throughout the troposphere?

page 19391 lines 26-28 The transport process described here seems like the main process the paper should focus on.

page 19392 line 1-5 The text makes it sound like the CFC-12S tracer takes an additional cyclonic pathway that is not followed by the DT6-month tracer. But both take the same pathway, it’s just that the lag in the seasonal minimum of the CFC-12S tracer is due to the lag in the lower stratosphere.

page 19394 lines 6-10 and Figure 8 Are the mixing ratios shown just due to transport from the stratosphere or do they also reflect tropospheric sources?

page 19395, first paragraph: Does the model account for ozone destruction in the lower troposphere due to halogen chemistry?

page 19395 line 21 change concentrations to mixing ratios

Figure 10b I don’t understand this figure. It seems to show that there is a net flux of NOy from the troposphere to the stratosphere in Feb-March. But isn’t this the time when DTfix shows maximum transport to the troposphere?

Figure 3 The mass flux peaks in the upper trop. in winter. But Stohl’s [2003] review of Appenzeller et al [1996] puts it at late spring. Why the discrepancy?

Figure 4 I don’t follow the convention of changing the sign of the CFC-12 anomalies.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 19377, 2008.