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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Impact of the 2009 major stratospheric sudden warming on the composition of the stratosphere" *by* M. Tao et al.

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General Comments:

This paper presents a detailed analysis of transport during the 2009 major sudden stratospheric warming (SSW) using the CLaMS Lagrangian transport model and MLS trace gas data. The detailed analysis of transport is presented primarily by discussing correlations between O3 and N2O. This work, and the conclusions drawn from it, is very interesting, presenting a new view of transport during an SSW; the work it decribes is appropriate for and should be of substantial interest to the readership of ACP. However, substantial revisions are needed before it is suitable for final publication.

The most serious issue is the discussion of tracer correlations in Section 5, which I

found it extremely difficult to follow though I have significant experience with the use of tracer correlations. Correlations of ozone and a long-lived tracer such as N2O have been used in numerous studies, and can be an interesting and informative way to view trace gas data, especially in cases where those data don't offer hemispheric daily coverage and thus transport and chemical processes cannot be diagnosed by examining the day-to-day evolution in physical space. However, interpretation of such tracer correlations is complex, and chemical and transport processes can, in some situations, produce similar changes. In addition, not all of your readers will be familiar with the use and interpretation of tracer correlations. Therefore, a much more complete and systematic description of how various processes affect the tracer correlations, and what that implies for the particular cases shown here, is needed. In fact, I believe the authors are missing an important opportunity here: Because they are using MLS data and a model that both offer full daily hemispheric fields, it is possible to relate the changes in tracer correlations to specific changes in physical space and in time – if done systematically, this would be extremely valuable and would not only clarify the interpretation that is currently given of the tracer correlations, but would serve as a valuable guide to the interpretation of trace correlations in general. I strongly encourage the authors to include such an analysis. In addition to the very helpful Plumb et al review paper already cited here, several papers that I have found useful for their clear descriptions of the complexities of interpreting tracr correlations are:

Waugh et al., 1997: Mixing of polar vortex air into middle latitudes as revealed by tracer-tracer scatterplots, JGR.

Michelsen et al., 1998, Correlations of stratospheric abundances of CH4 and N2O derived from ATMOS measurements, GRL.

Michelsen, et al., 1998, Correlations of stratospheric abundances of NOy, O3, N2O, and CH4 derived from ATMOS measurements, JGR.

Plumb, et al., 2000, The effects of mixing on tracer relationships in the polar vortices, JGR.

Esler and Waugh, 2002, A method for estimating the extent of denitrification of Arctic polar vortex air from tracer-tracer scatterplots, JGR.

Ray, et al., 2002, Descent and mixing in the 1999-2000 northern polar vortex inferred from in situ tracer measurments, JGR.

Sankey and Shepherd, 2003, Correlations of long-lived chemical species in a middle atmosphere general circulation model, JGR.

Hegglin and Shepherd, 2007, O3-N2O correlations from the Atmospheric Chemistry Experiment: Revisiting a diagnostic of transport and chemistry in the stratosphere, JGR.

In addition to the Plumb (2007) review paper, the last of this list is already cited in the current manuscript. However, the tracer correlation method is sufficiently intricate and dependent on particular circumstances (e.g., patterns of mixing, types and rapidity of chemical processes, spatial/temporal variations in chemical lifetimes and in transport barriers) that a fuller description of the relationships to spatial variations is needed to guide the reader through the interpretation of the correlation plots. Some of these papers may be helpful in accomplishing that.

The other significant issue I have is that there are several papers that examine three-dimensional transport during the 2009 SSW that should be cited and discussed in relationship to the results presented here. There are also some papers discussing the meteorology of that winter that are either not cited or for which the current results are not placed in the context of this previous work. One of these papers, Manney et al, 2009, GRL, is cited, but only in the introduction in a general sense – their results discussing the time evolution of MLS trace gas data (including N2O and the implications of that evolution for mixing) should be related to the results shown here. They also discuss the meteorology during this event, and this should be related to the meteo-

rological discussion in this manuscript. Two other papers with results that should be discussed in the context of the work on transport presented here are:

Orsolini, et al, 2010, Descent from the polar mesosphere and anomalously high stratopause observed in 8 years of water vapor and temperature satellite observations by the Odin submillimeter radiometer, JGR.

Lahoz, et al, 2011, The 2009 stratospheric major warming described from synergistic use of BASCOE water vapour analyses and MLS observations, ACP.

A smaller point is that I would encourage the authors to use the much more standard acronym "SSW" for "sudden stratospheric warming" (which is in turn preferred to "stratospheric sudden warming" for historical as well as other reasons, e.g., Butler et al, 2014, BAMS) rather than "MW", which is rather jarring to the reader who is familiar with past SSW studies.

Finally, there are errors in English grammar and usage throughout the paper that render it even more difficult to understand. These are too numerous to note here, but two areas that are consistently problematic are the misuse of commas and the misuse of "which" versus "that" throughout the text. I appreciate the difficulty in writing in a language that is not one's native one, and strongly suggest that the authors take advantage of the available editing for English usage before final publication.

Specific Comments (in order of appearance in the paper):

Page 4385, lines 6 through 14, these processes (as well as the difficulties that models have in representing them) are not specific to major SSWs. See, e.g., Sutton, et al, 1994, JAS, Fairlie et al, 1997, JGR, Manney et al, 1998, JGR; and references therein.

Page 4385, line 19, Manney et al (2005) and the Manney et al (2008) paper cited here do not discuss composition, except in the context of describing meteorological conditions that affect composition. A second Manney et al (2009) paper – in ACP – does discuss the evolution of MLS-observed trace gases from the UTLS through the lower mesosphere during the 2006 SSW. Manney et al (2009, GRL) discuss composition from the UTLS through the lower mesosphere during the 2009 SSW, not just in the lower stratosphere.

Page 4388, lines 4–10, It would be good to cite Butler et al (2014, BAMS) regarding the "standard" definition of a major SSW. Also, the discussion here implies that maximum polar cap temperature is more relevant than the standard diagnostic of circulation reversal (zonal mean 10hPa winds changing sign poleward of 60N) for determining the central day of an SSW – if this is the case, why?

Page 4388, line 11, and Figure 1 caption, please say what dataset the fields shown in Figure 1 are from.

Section 2 overall: The dynamical evolution discussed here should be related to previous work on the dynamics during the 2009 SSW, including (but not limited to) Manney et al (2009, GRL), Labizke and Kunze, 2009, JGR, Ayaraguena et al, 2011, JGR. These papers are cited herein, but the consistency of their results with those shown here is not discussed.

Page 4389, lines 22-23, (a) reference(s) should be given for the wave-driving of the Brewer-Dobson Circulation.

Page 4390, lines 1–9, Hitchcock and Shepherd (2013, JAS) should be included and discussed in relation to radiative timescales during and following SSWs. (In fact, Hitchcock et al, 2013, J Clim, would also be a very good reference to include regarding the vertical structure of dynamical fields during/after SSWs; these papers include discussion of the 2009 event.)

Page 4390, line 25, 2500K is not "near the stratopause" in the polar regions immediately following strong, prolonged SSWs, including the 2009 event (e.g., Siskind et al, 2007, GRL, Manney et al, 2008, JGR, France et al, 2012, JGR; and references

therein).

Page 4391, lines 6-8, what coordinate is used in the troposphere and how are the vertical velocities determined there?

Page 4391, lines 21-23, presumably the intended meaning is that the simulations with full chemistry and mixing are the reference for the best representation of the atmosphere?

Page 4391, lines 26-27, the vertical coverage of MLS data depends on the species. N2O is useful only from 100hPa through 0.46hPa, and at pressures of about 5hPa and lower has precision greater than 100%, implying that extensive averaging is needed. The description of "from the troposphere to the mesosphere" is thus not accurate. (In fact, even ozone is only available from the *upper* troposphere.)

Page 4391, lines 18-19, I see a small, persistent bias between CLaMS and MLS, with CLaMS O3 being higher at a given N2O in Figure 2. Can you say something about the reasons for this bias?

Page 4391, lines 23-24, please elucidate what you mean by "very stable", and to what altitude range this description applies. The vortex on 9 January was, indeed, rather symmetric in the middle and lower stratosphere, but quite elongated and shifted off the pole in the upper stratosphere, and examination of the preceding days shows large variability in shape/position throughout the stratosphere.

Page 4393, line 18, please define "overworld".

Page 4393, lines 19-21, the description of Nash's method of defining the vortex edge does not seem quite accurate. Nash defines the vortex edge location as the maximum PV gradient with respect to equivalent latitude, provided that that gradient occurs near enough the windspeed maximum.

Page 4394, line 4, please define what you mean by "eddy mixing" here; what other type of mixing are you distinguishing it from?

Page 4398, line 4 through Page 4399, line 2: This is one place where clarification of the effects of different processes on the tracer correlations is critical. The intention of the schematic to elucidate that is good, but the discussion is extremely difficult to follow, and did not convey to me how one could use different patterns to diagnose different processes. Further, the effects of chemistry and how they may differ from or mimic transport processes are not represented in this schematic.

Page 4399, lines 23-27, this is another place where the statements made are not clear to me from the figure.

Page 4400, lines 14-15, is there some particular significance (e.g., processes of particular interest) to the focus region chosen in the boxes?

Page 4400, overall: One of the pervasive difficulties in interpreting tracer correlations is that their morphology depends on non-local effects, and thus by taking very limited latitude or altitude regions, one may be biasing the picture – this discussion seems to me like it may point to such a difficulty?

Page 4401, line 22, it is not clear to me what feature in the figure you are describing as a "weak polar correlation"?

Section 5.3: The chemical processes discussed here are primarily the gas-phase processes in the middle to upper stratosphere. While tracer correlations have been used extensively (albeit often inaccurately) to examine lower stratospheric polar ozone loss, the impact of the gas-phase chemistry at higher altitudes on them has not been much discussed – therefore, a fuller description of the expected change in tracer correlations in relation to these processes would be helpful. Also, some current and previous studies (e.g., Kuttippurath et al, 2010, ACP, Manney et al, 2015, ACPD) have shown calculations suggesting a small but significant amount of lower stratospheric (chlorine-catalyzed) ozone loss in Dec/Jan 2009 – is this consistent with the suggestion given here that such lower stratospheric loss was negligible?

Page 4406, lines 17-23, is this discussion based on CLaMS, the MLS data, or both?

Figure 3 caption: state how the vortex edge is defined in the caption.

Figure 4, typo "voetex".

Figures 5 and 6, state what the letters represent in the caption. Also, from the discussion in the text, it was unclear whether the letters/numbers used in Figure 5 were or were not related to the same letters/numbers used in Figure 6.

Figure 7, describe in the caption what the black lines represent.

Figure 9, say in the caption what the boxes represent.

Figure 11, say in the caption what the letters A and B in the middle panel represent.

Figure 13, it would be more intuitive to show the results without the averaging kernel smoothing on the left, and the smoothed fields on the right.

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