Response to G. Manney (Referee)

Dear Gloria Manney,

Thanks a lot for this extremely thoughtful review that was made in a very constructive spirit; it has encouraged us "to do things better". In the following, we address all the points raised by you (denoted by italic letters). The second reviewer, Eric Ray, also gave us many very constructive hints which, to some extent, slightly overlap with your comments. Following your (and Eric Ray's) recommendation, section 5 and related Figures were almost completely revised. To render this response easy to read, you will find your original comments in italic letters, our answers in roman letters and the references to the manuscript (and in the manuscript) are highlighted in red.

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 donot 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.*

  Yes, we completely agree. In the response to this criticism, section 5 was almost completely rewritten. This section was also subdivided in several sub-section in order to achieve a clearer structure for our arguments. The new Fig. 8 should make it easier to switch between the physical- and tracer-space. Furthermore, we combined original Fig. 7 and 9 as the new Fig. 9 and included some reference correlation in order to see better the changes in the correlations during the winter.

- *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.

  This review comment again criticizes the discussion of tracer correlations and suggests a more extensive discussion of the literature – in response to this citicism we have considerably expanded the discussion of this point, in particular with a focus on the relation between physical space and tracer space as suggested (see also response above). A new Figure has been added (Fig. 8 in the revised version) and section 5 was rewritten accordingly.
  Further, we have added some background material on the impact of different processes on tracer relations in the introduction and added a schematic (new Fig. 1). This serves to give the background to the further discussion later in the paper and will make this material easier to follow. In these discussions, we also refer now to many of the papers suggested (e.g. *Michelsen et al.*, 1998; *Ray et al.*, 2002; *Sankey and Shepherd*, 2003; *Hegglin and Shepherd*, 2007; *Plumb*, 2007).

- *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 meteorological 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 sub-millimeter radiometer, JGR.
Lahoz, et al, 2011, The 2009 stratospheric major warming described from synergistic use of BASCOE water vapour analyses and MLS observations, ACP.

The third paragraph of introduction, where the case studies of SSWs are discussed, is rewritten. We extended the general introduction of previous works to a more detailed discussion. *Orsolini et al.* (2010) is cited and placed in the third paragraph in introduction together with related studies of *Manney et al.* (2008, 2009).

Lahoz et al. (2011) is cited in Section 2 (L. 199-201) on the bottom of Page 6, which is highly related to eddy heat flux in Figure 2. *Manney et al.* (2009); *Lahoz et al.* (2011) used tracer isopleths to estimate the descent rate during 2009 SSW. In the CLaMS simulation, $\dot{\theta}$ estimated by diabatic heating is employed as vertical velocity. Using $\dot{\theta}$ in polar region (see Figure 2 d), the descent rate was found to be consistent with the previous studies (*Manney et al.*, 2009; *Lahoz et al.*, 2011) in the sense that 1) the descent rate in the upper stratosphere was much higher than that in the lower stratosphere; 2) the descent rates were the highest in the time period during and shortly after the MW.

- *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, 2015, BAMS) rather than "MW", which is rather jarring to the reader who is familiar with past SSW studies.*

  We replaced the "stratospheric sudden warming" with "sudden stratospheric warming" following the recommendation of *Butler et al.* (2015). We have considered to replace MW with SSW because, as the reviewer said, some reader may be more familiar with "SSW" than with "MW". However, because the major sudden stratospheric warming is more standard and precise term here than SSW. Therefore, we now use "major SSW" instead of the abbreviation "MW" throughout the manuscript. We explained it at the beginning of the paper and use this abbreviation consistently.

- *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.*

We have submitted the revised manuscript to the English proofread department of our research center. However, the proofread costs about 2-3 weeks which will be beyond the deadline of submitting the responses. Thus, we decided to submit the response and manuscript first. The language check will be finished in next few weeks and we will included all the changes in the final publication.

Specific comments

- *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.*

  We agree that such difficulties are not specific to SSWs. The second paragraph of introduction has been rewritten.
  See the second paragraph on Page 2.

- *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.*
  The third paragraph of introduction has been rewritten.
  See the third paragraph on Page 2.

- *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?*

  We are aware that there are different definitions of major SSW as discussed in *Butler et al.* (2015). Since different criteria introduce no difference in identifying this 08/09 event, the popular criteria (60°Nzonal-mean zonal wind on 10 hPa) is used to identify the MW in our study (*Charlton and Polvani*, 2007). Only some small shifts of the onset date can be found by using different criteria and different dataset. Here we cited 2 studies (*Taguchi*, 2011; *Gómez-Escolar et al.*, 2014) which pointed out using the highest polar cap temperature date as the onset date. According to these studies, the response of the BDC to the MW event is better characterized.
  See L.145- 151 on Page 5.

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

Figure 2 (original Figure 1) is based on ERA-interim reanalysis and this information is added in text and figure caption.
See L. 152- 153 on Page 5 and Figure 2 caption.

- *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.*

  We agree. To keep connection with previous studies, we added some text in section of dynamical background.
  1) *Ayarzagüena et al.* (2011); *Harada et al.* (2010) studied the planetary wave propagation and its troposphere forcing, which agrees with our E-P flux analysis. We added the statement at L.183-184 on Page 6; 2) The estimation of descent rate studied by *Manney et al.* (2009); *Lahoz et al.* (2011) was cited at L.200-201 on Page 6 in the last paragraph of this section when we discussed the enhanced polar descent based on $\dot{\theta}$.

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

  We added a citation here (*Holton et al.*, 1995).
  See L. 190-191 on Page 6.

- *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.)*

  Hitchcock and Shepherd (2013) is a nice citation here (thanks a lot). We discussed and cited this study.
  See L.205-208 on Page 7.

- *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).*

  We agree that the stratopause broke down and reformed at a higher altitude (75km) during and after the Major SSW. Our statement is not accurate in this sense. 2500 K is the typical stratopause in boreal winter. Therefore, we changed it to "climatological position of the stratopause".
  See L. 224 on Page 7.

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

The model transforms from the isentropic to the hybrid-pressure coordinate below sigma= 0.3 hPa according to the procedure described in *Mahowald et al.* (2002). Thus, the ECMWF ω-velocity is used in the troposphere, which takes into account the effect of large-scale convective transport as implemented in the vertical wind of the meteorological analysis. We avoid this information because the θ coordinate is used in the altitudes we concerned. Nevertheless, we add additional sentence in the manuscript and related citations. See L. 230-234 on Page 7.

- *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?*

  Yes. For better understanding, we add this statement at L. 247-248 on Page 8.

- *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.)*

  We agree. The description "from the troposphere to the mesosphere" is removed.

- *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?*

  Looking into the daily validation of CLaMS with MLS on different altitude ranges, the worse corrlations (mainly over-estimated ozone of CLaMS) occur at 650 K- 1000 K in mid-latitudes from mid-February to March. We included this sentence into our manuscript. Here we can only speculate the the reason for the differences is a combination of 3 effects: not enough NO_xchemistry or too much ozone production or too fast poleward transport from the tropics (we did not include this statement in the manuscript).

- *Page 4392, 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.*

  We agree.
  See L. 277 on Page 9.

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

  "Overworld" is not apporpriate here, so it was removed.

- *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 wind-speed maximum.*

  We agree. The text about Nash's defination of vortex edge was modified.
  See L. 300-303 on Page 9.

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

  We replaced the term "barrier for eddy mixing" by "barrier for propagation of planetary waves". That is what we wanted to say at this place. Mixing is understood in this paper as a process changing the content of the air parcels, typically with a clear signature in the respective tracer-tracer space (if two air parcels having the same composition do mix, we also call this process mixing, although there is no practical way to detect it). We never use the term "eddy mixing" in this paper. Eddy mixing (i.e. mixing with a strong turbulence created by eddies) leads in most cases to a "true" physical mixing. However, especially within the Lagrangian school, pure trajectories describing chaotic advection are often denoted as eddy mixing.

- *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.*

  To achieve a better translation between physical space and tracer correlation space, Figure 1 has been added to the introduction in order to provide the basic concept of the tracer- tracer correlations and an overview of dynamics and chemistry impact on the tracer- tracer correlations. We hope this plot can prepare the readers for the upcoming intense discussion on the tracer- tracer correlations in section 5. Moreover, Figure 8 in section 5 has been extended. In particular, physical space interpretations are added to the corresponding tracer correlation patterns. The associated text was also modified. The effect of chemistry is still not discussed in this schematic cartoon.

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

  Hopefully the new version of section 5 will help.

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

We explained now in the text, that this choice should help to understand the difference between CLaMS results with and without mixing. We tried to address this point more clearly in section 5.3.2.

- *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?*

  Here, once again, we hope that the almost completelly rewritten version of section 5 will help.

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

  What we mean is "the weak polar correlation resolved with CLaMS (see Fig. 9(c2)) that is not resolved in the MLS observations", which pointing to the polar correlation shown on Fig. 9(c2) but not in Fig. 9(c1)
  See L. 524-525 on Page 16.

- *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 (chlorinecatalyzed) ozone loss in Dec/Jan 2009 - is this consistent with the suggestion given here that such lower stratospheric loss was negligible?*

  We didn't address the chlorine driven polar ozone loss during this winter because *Kuttippurath and Nikulin* (2012) (details see the Fig. 7 in the paper) estimated total ozone column loss in 2008/09 is less than other years without a MW and we also didn't find a persistent and significant polar ozone loss from January to March. However, after reading this paper (*Manney et al.*, 2015), we looked back to the period before the MW and did see a small but clear (chlorinecatalyzed) polar ozone loss in lower stratosphere before mid- January due to the unusual low temperature in 2008 early winter (see Fig. 11 b), which is consistent with the SSW case in 2012/13 according to *Manney et al.* (2015). Therefore, we agree that, similar as 2012/13 early winter with a very cold North pole, the lower stratospheric (chlorinecatalyzed) polar ozone loss in Dec/Jan 2009 is not negligible. We did some changes in the statement. See changes in L. 61-64 in the introduction and the 2nd paragraph in the section 5.4.

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

  The discussion is based on both, Or rather, based on the typical situation of N_2O and O_3 profiles during boreal winter and during the SSW period.

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

  Statement is added in the caption.

- *Figure 4, typo "voetex".*

  Changed.

- *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.*

  A sentence was added to the caption.

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

  The statement is added in the caption.

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

  The information is added in the caption of Figure 9.

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

  The information is added in the caption of Figure 11.

- *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.*

  The layout of Figure 13 has been changed.

References

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