

Interactive comment on “Middle atmospheric water vapor and ozone anomalies during the 2010 major sudden stratospheric warming” by D. Scheiben et al.

Anonymous Referee #2

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This paper uses ground-based microwave measurements at three stations, augmented by data from the Aura Microwave Limb Sounder (MLS) at the station locations, to describe the evolution of ozone and water vapour at these locations during the 2010 major stratospheric sudden warming (SSW). Little has been written about this event to my knowledge, so a detailed description of the event would be a valuable contribution to the literature. Likewise, a careful assessment of the information that can be obtained by a "mini-network", as the authors call it, of ground-based measurements and the degree to which that alone can characterize the atmospheric processes taking place would be valuable. Unfortunately, the authors fail to accomplish either of these, and thus it is not clear to me what the goal of this paper is. Given that the authors are

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using MLS data, which have nearly global coverage, if the aim of the paper is to provide a detailed description of the 2010 SSW, that would be best accomplished by using the MLS data to provide a complete description of the event, including synoptic evolution from maps and vertical cross-sections and/or time-series to show the time and space evolution in detail. If, on the other hand, the goal is to show how much of the essential processes occurring during the SSW can be captured by data at the three stations, the authors should again use the MLS data more fully to put the evolution at the stations in the context of the evolution of the full fields. As it presently stands, the authors guess at or assume changes result from particular processes, when very often simply showing the full fields from the MLS data could demonstrate whether those are indeed the relevant processes. In addition, the discussion is in general vastly over-simplified, in that it assumes effects are based on location with respect to the vortex (but doesn't give the reader enough information to be able to tell whether the stations are under the vortex or not at a given time) and oversimplified assertions about whether the trace gases are expected to be higher or lower inside the vortex, without properly taking into account the changes in these distributions with altitude, changes in vortex structure with altitude, or that the values "inside" and "outside" are not uniform but still depend strongly on the distance from the vortex edge.

These, along with other problems with the paper, are detailed below; I cannot recommend this paper for publication in ACP in anything close to its current form.

Specific Comments (in order of appearance, not importance):

page 32393, line 10, Suggest saying "results in" rather than "forms", since the "lack of solar radiation" in itself does not "form" the low pressure region.

page 32393, lines 13-14, It is not necessarily true that the "air inside the vortex is considerably colder ... than {that} outside". In fact, the low temperature region in the Arctic stratosphere is typically not well correlated with the vortex and can extend outside it – especially during SSW periods (e.g., Manney et al, 1994, MWR; Manney

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et al, 1998, GRL), such as that being studied here.

page 32393, lines 15-18, "The polar vortex exists from the troposphere up to the mesosphere, but is strongest in the stratosphere". At what level in the troposphere are you saying it exists down to? The studies I have seen indicate that it does not exist below the "lowermost stratosphere" (eg, see Santee, et al, 2011 and references therein), even in the Antarctic, and often not far into that region in the Arctic. How are you measuring strongest? How are you defining the vortex in order to conclude this? Little is known about the structure of the vortex in the mesosphere or its role as a transport barrier.

page 32393, lines 18-20, Neither Rosenfield et al (1994) nor Manney et al (1994, JAS, which should also be cited here) extended into the mesosphere. It would be best to cite Fisher et al (GRL, 1993) for modeling descent from the mesosphere.

page 32394, line 12, "...the stratopause often reforms at approximately 75km" is not accurate – this only happens after very strong, prolonged major SSWs, such as those in 2004, 2006, 2009 and 2010 – not during most major warmings. And the altitude of reformation depends strongly on latitude.

page 32394, lines 13-17, At the least, the references given here should include "and references therein", as there are *many* earlier papers on both subjects.

page 32394, line 18, "...major SSW during the winter 2007/2008 led to...." This is a misconception that appears throughout this paper, accompanied by the authors' failure to account for the fact that SSWs begin earlier at higher altitudes and affect the lower stratosphere later. The lower stratosphere is often cold at the time a major SSW state is reached in the middle stratosphere, and thus may still be experiencing chemical processing/ozone loss; but it invariably (and the late Feb 2008 major SSW (the only major SSW in that season) was no exception) warms up shortly thereafter in association with the major SSW, so that it would be accurate to say that the major SSW resulted in a cessation of chemical ozone loss. So at the most, it would be accurate to say that chemical processing was still going on in the lower stratosphere at the time

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the warming reached "major" status in the middle stratosphere.

page 32394, lines 28-30, Orsolini et al, 2010, JGR, should also be cited among studies showing the descent of H₂O into the stratospheric vortex during Arctic winter, and in particular after SSWs.

page 32395, lines 7-10, This statement makes it sound like this has not been done already. In addition to several papers already mentioned (Lahoz, et al; Orsolini et al) and numerous recent papers using model simulations (e.g., search for results on studies of SSWs using WACCM), both Manney et al (2009, ACP) and Manney et al (2009, GRL) described the impact of prolonged SSWs on transport and dynamics from the UTLS to above the stratopause in more detail than this current manuscript does.

page 32395, line 18, "is" should be "are".

page 32395, lines 19-21, "This is the first study..." This is hardly an argument for the value of the study, unless the authors can demonstrate that we have learned something about how the behavior of the middle atmosphere is reflected in these station measurements by putting them in the context of global measurements (as was done, for example, in some detail, by Manney et al (2008a in the citations here) for a single station during the 2004 and 2006 major SSWs). See my main general comment above.

page 32395, line 27, Replace "concludes" with "summarizes".

page 32396, line 4, Need to specify that this is for the ground-based microwave measurements (and give the frequencies for MLS measurements when they are discussed).

page 32396, lines 23 and 27, "is" should be "are" in both cases – the word "data" is plural.

page 32396, line 11, to page 32397, line 4, Some information on data quality (precision, accuracy) needs to be given for the ground-based instruments.

page 32397, Since the vertical resolution (along with recommended vertical range and

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other characteristics) of the MLS data in general changed between v2 (described by Lambert et al) and v3 retrievals, the v3 data quality document (which should be cited and in the reference list as a JPL Technical Memo by Livesey et al, 2011) should be checked and cited for the MLS vertical resolution. Also, as for the ground-based measurements, some quality information should be given. MLS temperatures are used later in the paper, so all of this information should also be given for those measurements.

page 32397, lines 12-14, Just giving the "coincidence criteria" (and what is the time limitation?) is not sufficient. Are all MLS profiles in that range averaged or just the closest selected? If averaged, how? What procedure is used to smoothly join MLS to ground-based data in a profile (I am concerned that this may have been neglected, since several figures show very large discontinuities in the combined profiles, especially Figure 8)? In addition, the spatial coincidence criteria used are much too broad to accurately match profiles either near the vortex edge where PV and trace gas gradients are extremely strong, or near the region of the warm pool during the SSW where temperature gradients are very strong.

page 32397, lines 22-23 and Table 1, Why is MLS rather than ECMWF temperature used throughout the altitude range? Yes, MLS extends higher – but MLS temperatures have known biases and artifacts (including vertical oscillations) that make them generally of inferior quality to ECMWF at levels where the latter is reliable, that is, up into the upper stratosphere.

page 32398, lines 3-4, I don't think the first phrase of this sentence adds anything.

page 32398, lines 2-9, "qualitatively" is not sufficient here. The reader needs to know specifically what method was used to determine the vortex edge – if/where a specific PV contour was used, the reader needs to know what that contour is, and in the mesosphere, exactly how the jet position is used to define the vortex edge.

page 32398, line 2 through page 32399, line 6, All of this discussion depends on the profiles (individual MLS-measured profiles?) really being "typical" of inside and out-

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side the vortex – but although the strongest gradients in trace gases (not necessarily in temperature, see previous comment) are along the vortex edge, the values of temperature and trace gases still vary strongly depending on the position in the vortex and even more strongly outside the vortex. So individual profiles cannot be said to be representative of either region as a whole. If you really want to characterize the vortex and extravortex region, I see no reason why, given that you are already using the MLS data, you should not show some sort of "vortex average" and mid-high latitude extravortex average (perhaps north of 45N excluding the vortex, to include the latitude of your lowest-latitude ground-based station).

page 32398, lines 13-15, Hitchman et al, 1989, JAS should be cited for the mechanisms resulting in the high polar stratopause.

page 32399, lines 3-6, Just because a trace gas has a strong gradient across the vortex edge does not mean it is appropriate to use it as a tracer of transport (to take an absurd example, ClO when activated has an *extremely* strong gradient across the vortex edge!). For the latter to be the case, the chemical lifetimes must be much longer than the transport timescales. While H₂O typically is a good tracer of transport in this sense from the lower stratosphere through the lower mesosphere, this is the case for ozone *only* in the lower stratosphere (below ~30hPa) and there *only* when there is no heterogeneous polar processing. In particular, anytime there is a persistent high-latitude anticyclone in the middle stratosphere (such as in disturbed conditions in the times surrounding SSWs), "low-ozone pockets" form in that region that can have comparably low ozone to some parts of the polar vortex (Harvey et al, 2008, JGR, and references therein), thus confounding signatures of transport.

page 32399, section 3.2, "The global view" would be much more complete, and more useful in understanding the conditions at the three stations, if the authors utilized the MLS data that they have to provide a global view of the trace gas evolution. Also, as per previous comment, why use MLS temperature rather than ECMWF at middle and lower stratospheric levels? Showing only 10hPa also does not give an adequate description

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of the vortex behavior during the major SSW – for example, the vortex *did* reform strongly in the upper stratosphere to lower mesosphere after the warming, although it did not at 10hPa or in the lower stratosphere.

page 32399, line 26, The wording "moving westward" and "moving eastward" is potentially confusing, as it might be read as implying westward or eastward winds, when in fact all I believe you doing is pointing out the locations of the two vortex fragments. This should be re-worded.

page 32399, lines 26-27, Why does it matter whether it is a vortex split or vortex displacement event?

page 32399, line 29, "...to the midlatitudes...": What is meant by "midlatitudes" here? It is not at all uncommon for the Arctic vortex to shift so that part of it extends into midlatitudes even in the absence of a strong warming. Was it extending farther south than is common? If so, please quantify.

page 32400 to 32401, Sections 3.3 and all of section 4, not enough information is given to interpret the local observations. At the very least, the reader needs to know the motion of the vortex and when the vortex was and was not over each station. Although PV contours are overlaid upon some of the trace gas time series, because what is shown is not presented in relation to the specific definition of what is inside and outside the vortex, it is of very little help in determining the location of the stations with respect to the vortex. To do this adequately, the overlays need to be of a quantity of which a specific value divides vortex from extra-vortex. First, the authors need to describe how they determine what is inside and outside the vortex. If that definition is based on a specific PV contour (necessarily scaled in some way so as to get a similar range at each level) then overlays of that quantity could serve to indicate when the vortex is and isn't over the station (as done in, e.g., Manney et al, 2008a). Another approach would be to, once the position of the vortex edge has been determined (in latitude or equivalent latitude), to overlay contours of the distance from the vortex edge

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(typically with inside being positive and outside negative). I am sure there are other possibilities – but SOME method needs to be used to show the reader what is inside and outside the vortex in a quantitative way in conjunction with not only the trace gas time series, but also the temperature time series. The local observations need to then be interpreted in light of this.

page 32400, line 15 through page 32401, line 2, The presentation of the trajectories is very poor. It is extremely difficult to discern from Figure 5 the relative amounts of descent/ascent and sometimes even the overall direction of the motion. The discussion is also inadequate. It isn't clear how the fact that, in pressure coordinates, the air rises and sinks depending on where it is in relation to warm and cold regions (which are typically not concentric with the vortex, and thus the air is moving in and out of them (eg, Manney et al, 1994, JAS)) relates to the patterns of motion shown. And it is not always clear when you are speaking in pressure coordinates or in isentropic coordinates – in the latter, the average daily motion will always be descending unless there is actually diabatic heating.

page 32401, lines 3-9, As per above comments, this is somewhat misleading, since the effects of the SSW reach the middle stratosphere sooner than they do the lower stratosphere. The cooling described in the lower stratosphere is thus not "during" the SSW if "during" is defined as the time the SSW effects reach those levels. Also, how does this discussion relate to the changes in diabatic motion during the course of the SSW?

page 32401, line 16, It is very difficult to see the point being made here (and impossible to discern what is inside vs outside the vortex) because the variations of PV are so small compared to its vertical gradients. As suggested above, it would be much better to show a "scaled" PV of some sort that has a similar range of values at each level, or a measure of the position with respect to the vortex edge, so the reader could see the variations that indicate changes in vortex position.

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page 32401, lines 17-18, While Schoeberl et al is a good reference here, the spatial correlation between PV and water vapour has been discussed/ demonstrated much more comprehensively in numerous more recent papers – the Lahoz et al and Orsolini et al papers mentioned above, and references therein, would be a good place to start.

page 32401, lines 18-19, Saying PV "is a good tracer" is wrong; as discussed in detail by Haynes and McIntyre (JAS, 1990), because the definition of PV depends on vertical gradients in potential temperature, PV behaves quite differently from a chemical tracer under the influence of diabatic effects. PV is thus a good proxy for passive transport (on short timescales, such that radiative effects are small) in the horizontal, but NOT in the vertical.

page 32401, lines 19-20, Whether or not "Stratospheric water vapor increases with height and latitude" depends strongly on the height and latitude range, and on the time of year. After low water from the mesosphere has descended into the vortex, water in the upper stratosphere in mid/high latitudes *decreases* with height and latitude.

page 32401, lines 21-22, The consistency of water vapor during an SSW with the results of Lahoz et al and Orsolini et al (both previously mentioned) would be much more relevant, as they studied the 2009 and 2006 prolonged SSWs that were much more similar to the 2010 event than was the brief, late February major SSW that Flury et al studied.

page 32401, line 24 to 32402, line 4, We need to know what the vortex position did in order to attempt to interpret the ozone changes in terms of the dynamics. Also, saying that ozone has a much shorter lifetime than water vapour is implicitly making the point I made above: It is NOT a good tracer of air motions. The weaker correlation could arise in part from the presence during the SSW (confirmed by a cursory glance at publicly available maps on the MLS website) of a strong low-ozone pocket in the anticyclone (which was at high latitudes), wherein low PV is correlated with low ozone – this possibility should be discussed.

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page 32402, lines 8-13, Because of the low-latitude source in the middle stratosphere, the vertical gradient in chemical ozone lifetimes (very long in the lower stratosphere), and the diabatic descent in the polar vortex, both the strength and direction of ozone gradients depend strongly on altitude, with ozone typically increasing in the lower stratosphere as you move from outside to inside the vortex (see, eg, Manney et al, 1995, JGR, 100, pgs 2953–2972, or any number of other general references showing ozone morphology in polar winter).

page 32402, lines 10-11, This assumption could, and should, easily be checked by simply examining the MLS fields and their day to day evolution.

page 32402, line 13, "typical" should be "typically".

page 32402, lines 18-21, You simply cannot tell this from what you have shown. In order to attribute the change to descent, you must first demonstrate that it is not consistent with horizontal transport or vortex evolution/motion. Again, simply looking at the full hemispheric MLS and PV fields could provide this information.

page 32402, lines 25-27, Again, you should verify this by looking at the full fields of PV and MLS H₂O.

page 32403, lines 9-14, This is not well supported. That "ozone concentrations remain very low" does not suggest chemical loss (which would result in decreasing ozone concentrations in absence of transport). The ozone concentrations in the lower stratosphere in Figure 7 are, in fact, decreasing after about 15 February, but the PV also appears to be decreasing – and since we don't have any reference to tell us what value of PV corresponds to the vortex edge, this could be interpreted as the vortex moving away from the station and the region where ozone decreases outside the vortex edge over it. Or the authors interpretation could be correct – we simply cannot tell based on the information given. Not only has it not been demonstrated where the measurement locations are with respect to the vortex as a function of time, but also the morphology of ozone (which is typically quite complicated in the lower stratosphere,

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and at levels up to perhaps 30hPa generally has a maximum inside the vortex (though often decreases from there to the vortex core). Unless it is shown what the morphology of ozone is in relation to the vortex (eg, using MLS and PV maps), and where the vortex is with respect to the station as a function of time, the changes in ozone cannot be interpreted. Furthermore, no support is given for the statement that PSCs were observed – if this was the case, either (a) citation(s) or more specifics need to be provided.

page 32403, lines 18-20, Again, the assertion of mixing with mid-latitude air is not supported, when it could be by showing the MLS fields.

page 32403, lines 21-23, The magenta line in Figure 8 is meaningless for deducing descent, since the vortex is not only moving with respect the station at this time, but also its shape and size are changing. Descent is not closely spatially correlated with the vortex – diabatic descent is closely spatially correlated with temperature (which is not strongly correlated with the vortex, especially during the SSWs), and vertical motion in pressure coordinates is more localized, tending to show a dipole pattern across portions of the vortex edge (see, eg, Fairlie et al, 1990, QJRMS; Manney et al, 1994, MWR; Manney et al, 2005, JAS). In isentropic coordinates, one expects to see, on average, monotonic descent within the vortex; it is not clear what one should expect in pressure coordinates. But even if there is a monotonic signature of descent, it would be confounded by motion of and changes in the vortex. The simplest way to sort out some of this would be to use the MLS data to provide information on the horizontal motions/changes, rather than basing your interpretation on guessing at those effects.

page 32404, line 1, How do you know this? No direct evidence has been presented indicating such mixing is going on.

page 32404, lines 4-6, See above comment on Onsala discussion.

page 32404, line 8, "To support our interpretation..." Not to say that trajectory calculations are not useful, but the "interpretation" given could have been supported through-

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out simply by fully utilizing the MLS data.

page 32404, lines 22-23 contradict lines 19-21 – if the winds are easterly (that is, from the west), they do not "enclose the polar vortex" do they?

page 32405, line 5, It doesn't seem likely to me that 47N (Bern's latitude) could be accurately described as bordering on the "subtropics"?

page 32405, line 17, The parenthetical comment isn't clear. What is the mesosphere over Bern warmer than?

page 32405, lines 1-4, Again, whether the ozone is lower or higher outside the vortex than inside depends on altitude, and on the position within or outside the vortex; eg, in the lower stratosphere, ozone is typically, and was in 2010, highest along the vortex edge and inside the vortex, a bit lower in the vortex core, and lowest outside the vortex, decreasing with distance from the vortex edge. So high mixing ratio in the lower stratosphere could very well come from air inside the vortex. In addition, the vortex changes size and shape and tilts with height (especially during an SSW), so much more information is required to understand the evolution of ozone at a station, and this understanding must be based on the three dimensional structure of the ozone fields and the vortex.

page 32407, lines 1-3, And they were observed over the whole hemisphere by MLS.

page 32407, line 8, "...a few days before the start of the SSW...." The start date of the SSW processes/signatures depends on altitude, as noted in a vast body of literature. At the least (and throughout the paper), it must be clarified what level you are talking about when you say "start of SSW", and the altitude dependence of the effects (earlier in the mesosphere/upper stratosphere, later in the lower stratosphere) should be discussed.

page 32407, line 17, 10hPa is much higher than ozone depletion via heterogeneous reactions on PSCs occurs. Even in the Antarctic, you wouldn't see it above about 25hPa, in the Arctic, it wouldn't be expected above about 30hPa.

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