

Interactive comment on “Variability of the nighttime OH layer and mesospheric ozone at high latitudes during northern winter: influence of meteorology” by A. Damiani et al.

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We thank Reviewer 2 for his/her useful advices allowing us to improve the paper. Below, first we show the comments of the reviewer and then we give our replies. In the revised manuscript we are applying your comments as shown below.

The paper presents the results of the MLS observations of OH and ozone in the mesosphere and the lower thermosphere in polar regions during five winters 2005-2009. Medium-term and short-term variability is discussed. The analysis is well-thought. The paper is interesting. It certainly deserves publication in ACP. My (mainly minor) comments are below.

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COMMENTS

1) p.14585 lines 10-11: Please explain in more detail the HOx dilemma (one sentence would be sufficient).

REPLY: In the revised manuscript we have reminded readers that the HOx dilemma arises from previous observations of either OH or HO₂ alone which have shown poor agreement with standard chemistry (JPL recommended rates). Therefore, in order to reach better agreement, some modifications to the rate constants of HOx partitioning reactions were suggested (e.g., Clancy et al., 1994; Summers et al., 1997). Nowadays the availability of MLS OH and HO₂ data are allowing to solve the problem (see Canty et al., 2006; Pickett et al., 2008) and no HOx dilemma is present between MLS observations and the standard chemistry.

2) p.14587 line 14: please explain the notation [O].

REPLY: We have indicated that the square brackets represent " concentration ".

3) p.14587, lines 18-20: I think it is worth not only mention the modeling work by Sonnenman et al., 2006a but also explain shortly their findings.

REPLY: We have inserted the main findings of Sonnenman et al., 2006a in the revised manuscript (p. col. 2, l. 34-40).

4) p.14588, line 23: The paper by Wang and Alexander (2009) discusses gravity wave activity during the 2008 SSW, not mesospheric ozone...

We have removed this reference.

5) p.14599, line 13: 2.5 km is the grid for the data product. Please indicate the actual MLS vertical resolution (this should be done using the information about the averaging kernel).

REPLY: The vertical width of the averaging kernel of MLS OH at pressures higher than 0.01 hPa is roughly coincident with the grid for the data (2.5 km). At the altitude of the

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OH layer it is slight larger (3 km). The resolution increases to about 5 km at 0.003 hPa. The changes in vertical resolution at pressures lower than 0.01 hPa are due mainly to use of a faster operational scan rate for tangent heights above 70 km (see Pickett et al., 2008).

6) Discussion of Figure 2 in pages 14590-14591: Significant difference between SH and NH polar regions is observed and it is worth to mention.

REPLY: In the revised manuscript we have discussed the elevated OH values recorded in the high latitudes of the SH. Obviously, in this case the descent of the OH layer is not connected to the raised OH abundance because it occurs at the summer hemisphere. There, nighttime OH may come from two sources: the nighttime production of OH by the reaction of ozone and atomic hydrogen and the daytime OH production via water vapor photolysis which lasts a few hours after sunset. In mesosphere (around 75-80 km) the HOx chemical lifetime of few hours (longer than in stratosphere where for example the HOx lifetime is of few minutes at around 60 km) makes possible preserving the daytime OH production some hours after the sunset. MLS nighttime OH data at southern high latitudes are recorded with SZA close to 95, therefore close to the local sunset, whereas at middle and low latitudes data are recorded with an higher SZA (e.g., SZA=160 around the equator) therefore only nighttime OH production has contribution and OH concentrations appear to be lower. We have inserted the above discussion in Section 4 (page 5, right column) of the revised manuscript.

7) Explanation of the mechanism of cooling in the mesosphere in pages 14593-14594: I think that together with the explanation of (Matsuno, 1971) based on the mass balance, another explanation based on gravity wave propagation (Holton, 1983) should be also mentioned. See (Wang and Alexander, 2009, introduction, and references therein).

REPLY: We have added Holton's explanation in the revised version of the manuscript.

8) The sentence: "The stratospheric wind reversal induced by planetary waves during SSWs also changes the filtering of gravity waves and allows more eastward gravity

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waves to enter the MLT: : " might be ambiguous. You probably mean "only a small portion of GW is able to enter the MLT", don't you?

REPLY: Shepherd, 2000 pointed out that the winter (summer) westerlies (easterlies) in the middle atmosphere filter out fluxes of gravity waves of the same sign, leading to net westward (eastward) angular momentum at mesopause altitudes. The effect drives the summer-to-winter-pole meridional circulation in the mesosphere that is responsible for reversing the meridional temperature gradient at these altitudes. Liu and Roble, 2002 showed that the gravity wave filtering effect should also operate on shorter time scales (i.e., SSWs). Therefore during SSW events the stratospheric wind reversal induced by planetary waves also changes the filtering of gravity waves, blocking gravity wave components with westward phase while allowing more eastward gravity waves to enter the MLT, with consequent reversal of the MLT jet (see also Ren et al., 2008).

9) Discussion on increased ozone rate production due to decreased temperature in page 14597: you are discussing polar night conditions when the ozone production is low. Therefore, the main reason for the expected ozone-temperature anticorrelation is the temperature dependence of the ozone loss reaction.

REPLY: We have pointed that out in the revised version.

10) P.14597: "Other events of sudden mesospheric ozone rise associated with temperature decrease are evident also in late February 2005, late January 2008 and February 2008." I think it is worth also to mention the SSWs when such behavior is not observed.

REPLY: We have inserted that in the revised manuscript. Only two SSW events are not associated with increased ozone at 0.01 hPa (i.e., the minor SSW of early February 2007 and the major SSW of late February 2008) but in both cases the temperatures were not so low as other SSWs.

11) Please discuss the ozone-temperature and OH-temperature correlations at 0.004 hPa.

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REPLY: The linear correlations shown in the Tab. 1 help us to identify many features that are not so obvious by simply looking at Figs. 3 and 4. In particular the reversal of the sign of the correlation at 0.004 hPa (about 85 km) is evident. First of all we point out that this altitude is normally above the altitude of the OH layer at ground vibrational state and close to the lower limit of the thermospheric ozone maximum. The correlations between OH and temperature and between O3 and temperature have the same sign in both “disturbed” years and “quiet” years but the mechanisms driving them are different. In “quiet” years we have SSW-induced cooling and uplift of the OH layer (see e.g., Sonneman et al., 2006) with consequent negative coefficient between T and OH and no correlation between T and O3. In “disturbed” years we have high temperature and low OH abundance (due to layer descent) therefore again negative coefficient between T and OH. Note also the positive correlation at 0.004 hPa between T and O3 in “disturbed” years which is mainly due to the high temperature and thermospheric ozone descent (see Smith et al., 2009). Additional features discussed in connection with Table 1 are inserted in the reply to comment 6 of Reviewer 1. We have inserted the above discussion in Section 4 (page 6, right column) of the revised manuscript.

12) I do not see gray regions in Fig.3.

REPLY: Note the maximum OH increase out of scale that occurred in January 2005 during SEP events.

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