

Journal: ACP

Title: Neutral atmosphere temperature change at 90 km, 70°N, 19°E, 2003-2014

Author(s): S. E. Holmen et al.

MS No.: acp-2015-326

MS Type: Research Article

General response:

All feedback we got on this manuscript is highly appreciated, and we think that it has helped improve the manuscript a lot. Some of the comments from the reviewers we consider out of scope for this paper, and we hope that they acknowledge that, but in general we have tried to address most of the comments. In the updated version of the manuscript we have redone nearly all analyses and most of the figures, using Aura MLS temperature data at 90 km geometric altitude instead of at 0.001 hPa.

Response to comments from Referee #1:

Page 15291, lines 1-11:

Referee's comment: Another important aspect in this context is the shrinking of the atmosphere associated with a general cooling of the middle atmosphere. For this reason it makes a big differences, whether temperature trends are studied at constant pressure or at constant geometric altitude. Perhaps this can be mentioned briefly. This point was also discussed in detail in recent papers by Luebken, Berger et al.

Author's response/changes in manuscript: We have, in the updated version of the manuscript, done all analyses again using Aura MLS temperatures at 90 km geometric altitude instead of at 0.001 hPa. The Lübken et al. (2013) paper discusses the aspect of the shrinking atmosphere, and we have added a paragraph about this in page 15294, line 2 (updated version: page 5, line 1): "Because of a general cooling of most of the stratosphere and mesosphere the last decades due to e.g. increasing concentrations of CO₂ and O₃, the atmosphere has been shrinking, leading to a lowering of pressure surfaces at various altitudes. It is important to distinguish between trends on fixed pressure altitudes and fixed geometric altitudes, since trends on geometric altitudes include the effect of a shrinking atmosphere (Lübken et al., 2013). In this study, we have obtained Aura MLS temperature data (version 3.3) for latitude 69.7°N ± 5.0° and longitude 19.0°E ± 10.0° at 90 km geometric altitude."

Page 15293, lines 5-8:

Referee's comment: It is mentioned later in the paper, but I suggest mentioning here, that these density climatologies do not take potential long-term of solar cycle variations into account.

Author's response/changes in manuscript: Added text: "These densities do not take into account long-term solar cycle variations."

Page 15294, line 2:

Referee's comment: 'Version 03' This is not an official version number. It's probably vs. 3.4 or 3.4? The current version is 4.2, I think.

Author's response/changes in manuscript: This was a slip-up. The data used is from version 3.3. Changed 'version 03' → 'version 3.3'.

Page 15294, line 3:

Referee's comment: 'at pressure 0.001 hPa, corresponding to ~ 90 km.' I think you should use the temperatures at 90 km geometric altitude, and not at a fixed pressure level. The Aura files also contain geopotential height for each profile, which can easily be converted to geometric height.

Author's response/changes in manuscript: All analyses have now been conducted again using Aura MLS temperatures at 90 km geometric altitude instead of at pressure 0.001 hPa. Figures and trends have been updated. Changes in text: "...and longitude 19.0° E ± 10.0° at 90 km geometric altitude."

Page 15294, lines 12 – 15:

Referee's comment: 'This was done by super . . . during auroral particle precipitation' The first sentence deals with the issue of diurnal variations, the second sentence with an entirely different issue. The diurnal variations are taken up again a few sentences below, i.e. its discussion is interrupted by the issue of modified electron mobility. I suggested arranging the paragraphs in a different way to avoid this 'interruption'.

Author's response/changes in manuscript: We agree that the way we have written this may lead to confusion, and have therefore made this change to the text: "There is an ongoing investigation into the possibility that D_a derived by NTMR can be affected by modified electron mobility during auroral particle precipitation. According to Rees et al. (1972), neutral temperatures in the auroral zone show a positive correlation with geomagnetic activity. It is therefore a possibility that apparent D_a enhancements during strong auroral events do not necessarily depict neutral temperature increase."

Page 15297, line 23:

Referee's comment: 'The long-term linear temperature trend using monthly means is – 3.6 K +/- 1.1 K / decade' Please mention briefly what the uncertainty estimate is based on. Is it based on the propagated uncertainties of the monthly means or on the quality of the linear fit without considering the uncertainties of the monthly means. These values may be different. And if the uncertainties of the monthly means were propagated, what was used as an uncertainty estimate: The standard deviation or the standard error of the mean?

Author's response/changes in manuscript: The uncertainty is based on the quality of the linear fit (standard error). This has now been specified in the text.

Page 15301, line 23:

Referee's comment: 'the peak altitude of the OH airglow layer can range from 75 to > 90 km (Winick et al., 2009)' It's of course correct that the OH emission altitude is quite variable, particularly at high latitudes and during/after sudden stratospheric warmings. However, von Savigny (JASTP, 127, 120 –

128, 2015) has shown recently that the OH emission layer height is remarkably constant from 2003 to 2011, also at high latitudes (Table 3 in that paper).

Author's response/changes in manuscript: Changed to: "It must be noted that the peak altitude of the OH* airglow layer vary and thus affect the comparability of OH* airglow temperature trends and meteor radar temperature trends. Winick et al. 2009 report that the OH* airglow layer can range from 75 to > 90 km, while the newer study by von Savigny, 2015, indicates that the layer height at high latitudes is remarkably constant from 2003 to 2011." von Savigny's paper is added to the reference list.

Page 15302, lines 1 – 5:

Referee's comment: the pressure change issue: I think one could use the MLS data to investigate possible changes in pressure surface altitudes. The MLS temperature profiles come on a pressure grid, but the data also include geopotential height, which can be easily converted to geometric height. It should be fairly straightforward to determine changes in pressure surface altitudes from 2005 to 2014.

Author's response/changes in manuscript: All analyses have now been conducted again using Aura MLS temperatures at 90 km geometric altitude instead of at pressure 0.001 hPa.

Page 15302, line 8:

Referee's comment: 'and increased GW drag leads to cooling.' This is only true for the polar summer mesopause region, isn't it? Above the winter pole I would expect increased adiabatic heating.

Author's response/changes in manuscript: Thank you for pointing this out. This section (page 15302, lines 6-15) is referring to gravity waves and their effect on temperature trends in the mesosphere and thermosphere and not the mesopause alone. We have made this change to the manuscript: "It has been proposed that GWs may be a major cause of negative temperature trends in the mesosphere and thermosphere (Beig, 2011; Oliver et al., 2013). GWs effectively transport chemical species and heat in the region, and increased GW drag leads to a net effect of cooling above the turbopause (Yigit and Medvedev, 2009). GWs are shown to heat the atmosphere below about 110 km altitude, while they cool the atmosphere at higher altitudes by inducing a downward heat flux (Walterscheid, 1981). However, there are large regional differences regarding trends in GW activity. Hoffmann et al. (2011) ...". Two references are added to the reference list.

Response to comments from Referee #2:

1. *Referee's comment:* The altitude of your analysis (90 km) is near the upper limit of MLS temperature data, and the uncertainties are large there. There are several other satellite instruments that provide intermittent but extensive measurements of temperature at 90 km over the period of the observations (AIM/SOFIE, ACE-FTS, TIMED/SABER, Envisat/MIPAS). Given all the uncertainties in your temperature retrieval, it would be appropriate to take advantage of these data to improve the confidence in the results.

Author's response/changes in manuscript: We acknowledge that we could have used another dataset than Aura MLS to calibrate the NTMR temperatures. One argument for using MLS compared with e.g. SABER or SOFIE data is the temporal coverage. SABER does not yield daily temperature values over the relevant time period. We did consider incorporating SOFIE data into our analysis, but in order to get sufficient temporal coverage we would have to expand the area of interest (latitude, longitude) to such a high extent that we considered the temperatures not so representative for Tromsø anymore. Incorporating temperatures from other satellite instruments at this point would result in a major re-analysis. We are receptive towards incorporating temperatures from other instruments, but have refrained from this in the present submission.

2. *Referee's comment:* The technique for comparisons with MLS is not optimal. Rather than using monthly means to derive correction factors, I suggest that you start with observations that you can compare directly. This would mean first comparing the radar data at times of the MLS overpasses for comparison directly with the MLS temperatures. Finding individual coincident profiles would be the best but even using monthly means of similar variables (e.g. from a similar local time) is better than the comparison shown in Figure 1. The process described in the paper wraps several "corrections" (local time, bias) together is a way that comes across as arbitrary. It would be useful to the reader to see a separation of the local time correction and the bias correction.

Author's response/changes in manuscript: We have not used monthly means to derive correction factors, but daily values. (The old) Figure 1 shows the two datasets before any corrections are applied, which we thought would be useful to the reader. It is correct that Figure 1 contains daily means of first estimate NTMR temperatures, while the daily means of Aura MLS temperatures are from 01-03 UTC and 10-12 UTC. We could, as you propose, have used NTMR temperatures only from 01-03 UTC and 10-12 UTC, but since this paper is principally about the temperature change in NTMR temperatures (and not Aura MLS) we thought it would be most fruitful to use NTMR temperatures from 00-24 UTC as daily means. We agree that it would be useful to show how the local time correction and the cold bias correction affect the results separately, but we have refrained from doing this in the present submission.

3. *Referee's comment:* As a related comment, I was uneasy that you applied all the corrections to MLS, which is an extensively investigated and widely used dataset, rather than to your own retrieved temperatures. Did you look at MLS temperature validation papers for further guidance?

Author's response/changes in manuscript: The focus of this paper is not to correct Aura MLS temperatures, but to correct the NTMR temperatures relative to Aura MLS. The "only" corrections we have done to the Aura MLS dataset are the cold bias correction and the local-time correction. According to other papers (French and Mulligan, 2010; Garcia-Comas et al., 2014) Aura MLS exhibit a colder mesopause compared with other instruments. Regarding the local-time correction, the purpose was not to "correct" the MLS temperatures in itself, but since the diurnal variation shows that temperatures are in general lower in the forenoon (Fig.

2), which corresponds to when Aura MLS makes measurements over Tromsø, we thought it would be a good idea to compensate for this.

4. *Referee's comment:* Another advantage of using coincidences to compare with MLS would be the option to compare your pressure with the pressure from the MLS dataset. This would provide additional much-needed validation.

Author's response/changes in manuscript: To compare pressure values with Aura MLS pressure would result in a validation of the falling sphere measurements done by Lübken and von Zahn (1991) and Lübken (1999). We feel that this falls outside the scope of this paper. However, it would be very interesting to do this and it would be worth a separate study.

5. *Referee's comment:* The period of your data coverage presents difficult challenges for diagnosing trends and solar cycle. The period covers substantially less than one full solar cycle. Moreover, the net trend in the F10.7 radio flux over these 11 years is not negligible (for example see <http://www.swpc.noaa.gov/products/solar-cycle-progression>). It is premature to apply trend and solar cycle analysis to these or any observations for this time period.

Author's response/changes in manuscript: The data covers one solar cycle, and we agree that diagnosing trends over this time period presents challenges. We have used the term "change" instead of "trend" in the title of the paper, but we agree that we could have expressed ourselves differently throughout the paper to avoid misrepresentation. Therefore, we have made several corrections to the text regarding this.

6. *Referee's comment:* The discussion indicates that the trends are quite different for different seasons of the year. With this being the case, deseasonalizing the data and then calculating the trend using all months of the year will give a result that is very difficult to interpret.

Author's response/changes in manuscript: This is true. We have added an extra paragraph in the discussion to emphasize this. Page 13, line 13 (in updated version): "Our results indicate a cooling at 90 km altitude over Tromsø between 2003 and 2014 when deseasonalizing our dataset and calculating trend using all months of the year. At the same time we get quite different trends for winter and summer. We emphasize that this may give a result that may be somewhat difficult to interpret."

7. *Referee's comment:* Section 5.2 mentions that gravity wave activity could affect the temperature trend. Since you have radar wind data, this would be an important addition to the study. You could, at a minimum, determine whether there is a relationship between the interannual variability of gravity waves, mean winds, and temperature.

Author's response/changes in manuscript: Our main goal with this paper is to identify a temperature change over the 11 year time period from 2003 to 2014. We are suggesting hypothetical reasons for the change we see. It would be very interesting to do an analysis on the relationship between winds, temperatures and GW fluxes, but we consider this to be outside the scope of this study and something that would be worth a separate study. We have made an addition to the text in the discussion to emphasize this. Page 12, line 30: "It is

not the purpose of this study to analyse the contribution of dynamics to the temperature change we observe”.

Referee's comment: Editorial: There is an abrupt transition between the first and second paragraphs of Section 3 that is hard to follow.

Author's response/changes in manuscript: We agree that the way we have written this is confusing, and have therefore made this change to the text: “There is an ongoing investigation into the possibility that T_a derived by NTMR can be affected by modified electron mobility during auroral particle precipitation. According to Rees et al. (1972), neutral temperatures in the auroral zone show a positive correlation with geomagnetic activity. It is therefore a possibility that apparent T_a enhancements during strong auroral events do not necessarily depict neutral temperature increase.”

Response to comments from Referee #3:

Page 2., l. 18.

Referee's comment: I suggest to add reference(s) to this assertion.

Author's response/changes in manuscript: References added. The text is also slightly changed, since different model studies predict different results. Page 15291, lines 6-8 (page 2, line 16-20 in updated version): “Newer and more sophisticated models include important radiative and dynamical processes as well as interactive chemistries. Some model results indicate a cooling rate near the mesopause less than predicted by Akmaev and Fomichev (1998), while others maintain the negative signal (French and Klekociuk, 2011; Beig, 2011).”

Page 3, l. 19.

Referee's comment: It is not clear here if the radius of 50 km refers to horizontal space or to volume. I believe it refers to horizontal space. However, it is important to know at which altitudes the meteors (or better the echoes of them) take place. Also, I miss a figure showing the vertical distribution of them. It is mention that most of them occurs at 90 km with a height resolution and range resolution of 1 km. It would be useful to show the reader the actual vertical occurrence of the meteors. I point this out because the temperature trends tend to change significantly with altitude in this region, so it would be good to show it.

Author's response/changes in manuscript: 50 km refers to horizontal space. This has now been specified in the text. Also, we have added a figure showing the vertical distribution of meteor echoes from 2003 to 2014. (This is Figure 1 in the updated manuscript.)

Top of page 4 and further in the discussion:

Referee's comment: The effect of temperature. Both MLS and SABER measures the pressure. I think it would be very useful to repeat the study but with the "simultaneous" pressure measured by the satellite instruments.

Author's response/changes in manuscript: We feel that comparing pressure values falls outside the scope of this paper. However, it would be very interesting to do this and would be worth a separate study.

Page 4, lines 14 and ff.

Referee's comment: Is there a particular reason of why to change the "calibration" of radar temperatures from the OH rot. temperatures to MLS? MLS data have a very broad vertical resolution at this altitude, probably wider than the OH layer thickness.

Author's response/changes in manuscript: There are unfortunately no OH airglow temperatures available from Tromsø for the time period of interest. The nearest location for measuring OH temperatures is Andøya.

Page 5.

Referee's comment: Da rejection. Possible correlation of Da with geomagnetic activity. Have the authors looked at the correlation of Da with a geomagnetic index, Ap or Kp? That would be very useful. Not clear which Da values were rejected: "all half hourly Da values with a standard error larger than 7% of the estimated Da value were excluded from further analysis." Does this mean that Da outside +/-7% (plus and minus) were rejected? I would tend to reject only those with high values, but not the low values.

Author's response/changes in manuscript: We agree that looking at the correlation between Da and geomagnetic indices would be very useful and interesting. We have not calculated any correlation. However, we believe there is a clear relation between D enhancement and geomagnetic activity. Regarding the excluded values of Da: We rejected all individual D values (half hourly) whose estimation error was significantly large (standard error > 7 %). We calculated the standard errors, se, relative to the half hourly Da values, and the rejection procedure was as follows: if $se_t > Da_t \cdot 0.07$ → reject Da_t .

Page 6, lines 8-11.

Referee's comment: The MLS bias of 10 K at 90 km does not seem to occur at all seasons. Garcia-Comas et al., 2014 have carried out a comprehensive validation/comparison of several satellite instruments and showed that although for polar summer/winter conditions the positive bias is large (close to 10 K) (Figs. 8 and 9, top left panels), for spring/fall conditions the bias is not so large, about 5 K (Figs. 6 and 7 top left panels).

Author's response/changes in manuscript: The calibration of the NTMR temperatures has been conducted again, using 5 K cold bias correction on the autumn and spring temperatures and 10 K correction on winter and summer temperatures. Changes in text: "Also, the Aura temperatures were corrected for "cold bias". French and Mulligan (2010) reported that Aura MLS temperatures exhibit a 10 K cold bias compared with OH*(6-2) nightly temperatures at Davis Station, Antarctica. A newer study by Garcia-Comas et al. (2014) shows that Aura MLS exhibits a bias compared with several satellite instruments which varies with season. According to their findings, a 10 K correction for cold bias was applied to the Aura summer and winter temperatures (Jun – Aug, Dec – Feb), while a 5 K correction was applied to autumn and spring temperatures (Sep – Nov, Mar - May)."

The updated Figure 4 (which is Figure 5 in the updated manuscript) shows now that a linear fit gives a just as good overall fit to the scatterplot of corrected Aura MLS temperatures and uncorrected NTMR temperatures compared to a quadratic, least-squares fit. Therefore, Equation 4 and the belonging text is changed: "Figure 5 shows a scatterplot of the corrected Aura temperatures against the "raw" NTMR temperatures. The red line represents the linear fit ($R^2 = 0.83$) and is described by: $T_{\text{NTMR}} = 0.84T_{\text{Aura}} + 32 \dots$ " The figure caption under Fig. 4 is changed correspondingly.

Page 6. Lines 20-21.

Referee's comment: "For calibration of the remaining NTMR temperatures ..." Could the authors clarify what are the "remaining NTMR temperatures"?

Author's response/changes in manuscript: The remaining NTMR temperatures are the NTMR temperatures for days of measurements not coinciding with Aura measurements. To clarify, we have made the following change to the sentence: "For calibration of NTMR temperatures from November 2003 to August 2004 (before the start of the Aura MLS dataset), the same equation (Eq. 4) was used, using NTMR Da-rejected temperatures from November 2003 to August 2004 as input instead of T_{Aura} ".

Page 6. Lines 23-24.

Referee's comment: Here and at other places it is not always clear when the meaning of the terms "corrected" and "raw". For example, in these lines, do the authors mean: "To estimate the calibration uncertainty, all LOCAL TIME-corrected Aura temperatures were subtracted from the MLS-CALIBRATED NTMR temperatures, and the differences were plotted in a histogram with 5 K bins."? Since there are several corrections: Darejected, local time correction and the "MLS-calibrated", I suggest to add an adjective about the correction when referring to the different corrected temperatures.

Author's response/changes in manuscript: We agree that the terminology can be confusing at times. To make it clearer, we have made several changes throughout the manuscript.

Page 5, line 9 (updated version): "Figure 2 shows daily NTMR temperatures.." Removed term "raw", since it should be obvious from the rest of the sentence what kind of temperatures we are talking about.

Page 6, line 1: Added sentence: "NTMR temperatures after application of the Da rejection procedure will henceforward be referred to as D_a -rejected NTMR temperatures".

Page 5, line 3: "Figure 3 shows monthly averages of the superposed values of D_a -rejected NTMR temperatures as a function.."

Page 6, line 22: Added sentence: "The corrected Aura temperatures will henceforward be referred to as local time and cold bias-corrected Aura MLS temperatures."

Page 6, line 25: "Figure 5 shows a scatterplot of the local time and cold bias-corrected Aura temperatures against the D_a -rejected NTMR temperatures."

Page 6, line 31: "NTMR temperatures were now corrected for the days of measurements coinciding with Aura measurements and are henceforward referred to as MLS-calibrated NTMR temperatures."

Page 7, line 6: "To estimate the calibration uncertainty, all local time and cold bias-corrected Aura temperatures were subtracted from MLS-calibrated NTMR temperatures, and the.."

Page 7, line 11: "Finally, Fig. 7 shows the MLS-calibrated NTMR temperatures with uncertainties plotted together with Aura MLS temperatures, corrected for cold and time-of-day measurement bias."

Page 7, line 15, line 18: "calibrated" → "MLS-calibrated"

Line 25.

Referee's comment: The uncertainty of the calibration is 8.9 K. This is large! How has this been taken into account when estimating the error in the trend? Assumed negligible because it is assumed to be constant in time?

Author's response/changes in manuscript: We have not taken the uncertainty of the calibration into account when estimating the error in the trend. As the referee mentions, the uncertainty is assumed to be constant in time. We are aware of that this uncertainty is high, but we are presenting it here as a caveat to the reader.

Line 29.

Referee's comment: I believe what you applied here is a "local time" correction (not tidal). It should be in general dominated by tidal (btw which is the tidal amplitude at 70N?) the correction was done in local time.

Author's response/changes in manuscript: We have replaced "tidal" with "local time", both in the text and in Fig. 6 (Fig. 7 in updated manuscript) (legend).

Page 7, line 17.

Referee's comment: The solar response coefficient obtained here is somehow smaller than the values obtained by Forbes et al. from SABER data of 5.9 K/sfu. at 80-90 km, 50-70N, and close to 10 K/sfu for 90-100 km at 50N-80N.

Author's response/changes in manuscript: In the updated version of the manuscript the solar response coefficient is $4.5 \text{ K} \pm 0.3 \text{ K/100 SFU}$. It is true that our estimated solar response coefficient is smaller than what found by Forbes et al. (2014), but it is in line with results from other studies (Beig, 2011). Also, Forbes et al. write in the "Results in context of previous works" section that their values in general are larger compared with values obtained by others. We have included a short paragraph in the discussion regarding the solar response value. Page 12, line 6: "The estimated solar response coefficient of $4.5 \text{ K} \pm 0.3 \text{ K/100 SFU}$ is somewhat smaller than values obtained by Forbes et al. (2014) using SABER data from 2002 through 2013. For latitudes 50°N-80°N and altitudes 90-100 km solar response coefficients obtained by Forbes et al. are close to 10 K/100 SFU. However, Beig (2011) summarizes results from numerous studies on temperature variability due to solar activity and

reports that in general, temperature response is 4-5 K/100 SFU in the upper part of the mesopause. Our result is in line with this.”

Page 7, lines 22-23.

Referee’s comment: Could the authors mention the altitude range at which Ogawa et al. look at?

Author’s response/changes in manuscript: The altitude range they look at is 200-450 km. We have now pointed this out in the text.

Page 7, line 26

Referee’s comment: philosophy -> rationale? Also here, I am not sure about using the quadratic form. Figure 8 shows that at SFU larger than 180 units the quadratic fit is not really good. If applied the linear fit, would that change the trend?

Author’s response/changes in manuscript: If we apply the linear fit, the trend using monthly values would be $-4.7 \text{ K} \pm 1.4 \text{ K/decade}$. We have included also these numbers on page 8, line 26.

Page 7 bottom and ff. lines.

Referee’s comment: Since the AURA correction was done on the basis of monthly mean, I would then do the solar fit also using monthly mean values. Probably the results would be very similar but it seems to me more consistent.

Author’s response/changes in manuscript: The Aura MLS correction was not done on the basis of monthly means, but daily means.

Sec. 5.1 Page 9, bottom and first lines on page 10.

Referee’s comment: After this discussion, where those effects "... may lead to errors of several kilometres ..." do the authors still think that the errors in the altitude where they are looking at temperature trends is still 1 km, as asserted in page 3? It would be good to include a "realistic" error in this altitude and be included in the conclusion (remember the broad vertical resolution of MLS temperature around 90 km).

Author’s response/changes in manuscript: In Section 5.1. we discuss the suitability of a meteor radar for estimation of temperatures at 90 km height. Using our method, adopted from Holdsworth et al. (2006), we assume that ambipolar diffusion alone determines the decay of underdense echoes. According to Hall et al. (2005) this is only the case between ~85 km and ~95 km. Above ~95 km anomalous fading times of the echoes can be attributed to Farley-Buneman instability, which can lead to errors in the temperature estimation (e.g. Dyrud et al., 2001). We only use echoes around 90 km height, and therefore do not have to be concerned about the “errors of several kilometres” due to Farley-Buneman instability. Since we are looking at 90 km height, we believe that the height resolution and the range resolution are both 1 km. To make this clearer, we have made a small change to the instrument section.

Page 3, line 27: “The height resolution and the range resolution are both 1 km, when looking at altitudes around the peak occurrence height.”

Page 10. lines 20 and ff.

Referee's comment: See point before. Why not use pressure from MLS? Or use p-T from SABER?

Author's response/changes in manuscript: We feel that comparing pressure values falls outside the scope of this paper. We could have used pressure from Aura MLS, but we chose to use pressure from Lübken and von Zahn (1991) and Lübken (1999) when calculating the NTMR temperatures, for consistency with Dyrland et al. (2010).

Sec. 5.2

Referee's comment: Do the authors have any explanation of why the trend derived from OH at near 87 km differs so much (it is nearly zero vs. -3.6K/dec) from the results of this work?

Author's response/changes in manuscript: The two trends differ, that is true. The OH* airglow temperature trend is from Longyearbyen, Svalbard, ~8° further north. Also, the OH* temperature series is much longer, dating back to 1983. Another difference is that the meteor radar temperatures are recorded all year round, while the OH* temperatures are only recorded during winter.

Lines 16-17.

Referee's comment: I think the result of Winick et al. is overstated here. The changes in the OH layer from 75 to > 90 km actually only occurs when there are extreme stratospheric warmings, such as that of 2009, occurring, as most, during only a few weeks. In general it is very stable (except for the tidal effects). There are several studies showing this. I think this is not a convincent argument for explaining the discrepancy.

Author's response/changes in manuscript: We have added another reference in addition to Winick et al. (2009) and changed the text: "It must be noted that the peak altitude of the OH* airglow layer can vary and thus affect the comparability of OH* airglow temperature trends and meteor radar temperature trends. Winick et al., 2009 report that the OH* airglow layer can range from 75 to >90 km, while the newer study by von Savigny, 2015, indicates that the layer height at high latitudes is remarkably constant from 2003 to 2011. "

Page 11, line 30.

Referee's comment: I understand that GW could lead to cooling but also to heating (breaking of GWs should deposit their kinetic energy). Could the authors give a reference to the assertion about the cooling?

Author's response/changes in manuscript: We have made this change to the manuscript: "It has been proposed that GWs may be a major cause of negative temperature trends in the mesosphere and thermosphere (Beig, 2011; Oliver et al., 2013). GWs effectively transport chemical species and heat in the region, and increased GW drag leads to a net effect of cooling above the turbopause (Yigit and Medvedev, 2009). GWs are shown to heat the atmosphere below about 110 km altitude, while they cool the atmosphere at higher altitudes by inducing a downward heat flux (Walterscheid, 1981). However, there are large regional differences regarding trends in GW activity. Hoffmann et al. (2011) ...". Two references are added to the reference list.

Fig.1

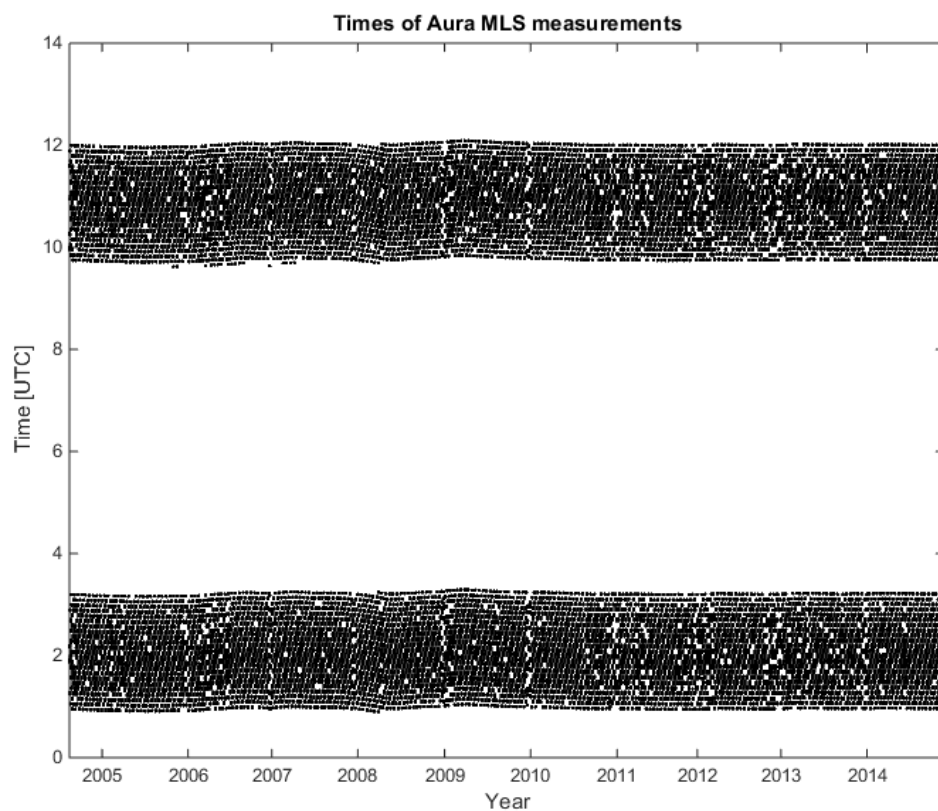
Referee's comment: Are daily, monthly values? Does Aura (uncorrected) mean "not local-time corrected", e.g, as measured by AURA? Are the "raw" NTMR already corrected for high D_a ?

Author's response/changes in manuscript: We realize that the figure caption lacks some vital information and have made these changes: "Daily values of NTMR temperatures derived from Eqs. (1) and (2), before correction for high D_a , plotted together with Aura MLS temperatures, before applying any corrections."

Fig. 2.

Referee's comment: Are the NTMR T corrected for high D_a here? One question about the variability of the local time of MLS measurements with years: are the local times fixed in the 10 years period? In other words, are the different local times in Fig. 2 (highlighted) for each of the two mean times spread more or less evenly over each year or do they have any trend with year?

Author's response/changes in manuscript: Yes, the NTMR temperatures are corrected for high D_a in this figure. We have made changes to the text and the figure caption. Regarding the local times of Aura MLS measurements, we have plotted the times (all individual data points, approximately 17 data points per day) in a figure:



The figure is not a part of the manuscript. It is only to show the reviewer that we consider the local times fairly fixed during the 10-year period.

Fig. 4.

Referee's comment: Are the NTMR "raw" temperatures corrected for Da high values? If yes (as I guess), why then call them "raw"? Do you mean "uncalibrated"?

Author's response/changes in manuscript: Figure caption changed to: "Scatterplot of Aura temperatures corrected for cold and time-of-day bias against NTMR Da-rejected temperatures. The red line.." Label on y axis changed to "NTMR derived 90 km temperature, uncalibrated".

Fig. 5.

Referee's comment: and ... LOCAL-TIME-corrected Aura MLS temperatures?

Author's response/changes in manuscript: Figure caption changed to: "Histogram of the differences between MLS-calibrated NTMR temperatures and local time and cold bias-corrected Aura MLS temperatures. The red curve..."

Fig. 6

Referee's comment: Daily? Monthly?

Author's response/changes in manuscript: Daily values. Figure caption changed to: "Daily values of MLS-calibrated NTMR temperatures plotted together with Aura MLS temperatures corrected for cold and time-of-day bias. The overall..."

Fig. 7.

Referee's comment: I think this figure is not necessary. The seasonal cycle is quite well know.

Author's response/changes in manuscript: We have now skipped this figure and made a small change to the text (page 7, line 3): "The seasonal variation is not shown here but reveals a summer minimum of around 150-160 K and ..."