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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Neutral atmosphere temperature change at 90 km, 70 N, 19 E, 2003–2014" *by* S. E. Holmen et al.

Anonymous Referee #3

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General comments:

This work addresses a very important topic of the middle and upper atmosphere, the trends in temperature, in this particular case at high latitudes in the NH. This then adds a new analysis based on a new dataset to the current studies in this field, which I consider very important.

I have found the paper very well written, with a clear methodology, with adequate references for previous work, a good error analysis, a clear discussion, and important conclusions. I find the paper suitable for publication in ACP. I only have a few minor comments and some suggestions for the authors who may want to take or not (they are not compulsory for the acceptance of the paper, but will probably improve it or

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consolidate their results).

Comments:

Probably my major comment is why to use MLS temperatures for validating the NTMR temperatures when they have such a large bias and, another major drawback from my point of view, that they have a broad vertical resolution in the 90 km region, which is or the order (or larger) than 15 km. I would suggest to use temperatures with better vertical resolution, e.g. SABER. They could also use the pressure from SABER, since, as discussed, this might introduce a significant error. Of course, SABER is not perfect, since it cover the high NH latitudes only 6 months per year, but even though I think it would be very useful to "calibrate" NTMR temperatures with this instrument. Another advantage of SABER is that it covers more local times than MLS, not just 2 local times.

I like very much the discussion of the temperature trends in different seasons, mainly winter and summer. This is an important result, in line with previous studies.

Page 2., l. 18. I suggest to add reference(s) to this assertion.

Page 3, l. 19. 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.

Top of page 4 and further in the discussion: 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.

Page 4, lines 14 and ff. Is there a particular reason of why to change the "calibration"

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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.

Page 5. 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.

Page 6, lines 8-11. 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).

Page 6. Lines20-21. "For calibration of the remaining NTMR temperatures ..." Could the authors clarify what are the "remaining NTMR temperatures"?

Page 6. Lines 23-24. 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: Da-rejected, local time correction and the "MLS-calibrated", I suggest to add an adjective about the correction when refering to the different corrected temperatures.

Line 25. 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?

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Line 29. 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.

Page 7, line 17. 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.

Page 7, lines 22-23. Could the authors mention the altitude range at which Ogawa et al. look at?

Page 7, line 26 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?

Page 7 bottom and ff. lines. 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.

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

Page 10. lines 20 and ff. See point before. Why not use pressure from MLS? Or use p-T from SABER?

Sec. 5.2 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?

Lines 16-17. 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

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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.

Page 11, line 30. 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?

Fig.1 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 Da?

Fig. 2. Are the NTMR T corrected for high Da 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?

Fig. 4. Are the NTMR "raw" temperatures corrected for Da high values? If yes (as I guess), why then call them "raw"? Do you mean "uncalibrated"?

Fig. 5. and ... LOCAL-TIME-corrected Aura MLS temperatures?

Fig. 6 Daily? Monthly?

Fig. 7. I think this figure is not necessary. The seasonal cycle is quite well know.

References

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