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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 #1

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General comments:

This is an interesting and well written manuscript dealing with decadal changes in mesopause temperature retrieved from meteor radar observations near Tromso, Norway. The measurements are available from 2003 to 2014, i.e. cover about a solar cycle. Although there is the general issue, in terms of how well one can separate the 11-year solar cycle from potential long-term trends with time series covering less than 2 or 3 solar cycles, I think this manuscript if worth publishing. The methodology can be improved somewhat by using MLS temperature data at a constant geometric altitude, not at a constant pressure level (see specific comment below). Also, the MLS data set could be used to study temporal variations of the pressure surfaces, which would allow a more robust interpretation of the results obtained.

Specific comments:

Page 15291: lines 1 – 11: 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.

Page 15293, lines 5 – 8: 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.

Page 15294, line 2: '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.

Page 15294, line 3: 'at pressure 0.001 hPa, corresponding to $\sim$ 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.

Page 15294, lines 12 – 15: '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'.

Page 15297, line 23: '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?

Page 15301, line 23: '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).

Page 15302, lines 1 – 5: 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.

Page 15302, line 8: '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.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 15289, 2015.