Response to RC2

Interactive comment on "Analysis of 24 years of mesopause region OH rotational temperature observations at Davis, Antarctica – Part 1: Long-term trends" by W. John R. French et al. Anonymous Referee #1 Received and published: 7 February 2020

Reviewer Report on the manuscript acp-2019-1001 Analysis of 24 years of mesopause region OH rotational temperature observations at Davis, Antarctica – Part 1: Long-term trends by W.John R. French et al.

General Remarks

1. . .The paper presents 24 years of observations of OH temperatures, which is an intersesting extension of an earlier data set worth publishing.

Thank you.

2 The data were taken in Antarctica where such measurements cannot be performed in summer. This is a drawback for several interpretation aspects and must be carefully considered.

Observations of the hydroxyl nightglow cannot be made over the summer at this latitude and we understand this is a limitation of the observational program for long-term trends using this technique. We contribute a solar and long-term trend assessment of the mean winter temperatures at this high southern latitude site and make comparisons with satellite observations to place these observations into global context.

3 The data are discussed in the context of increasing CO2 mixing ratios. They are extensively compared to MLS and SABER satellite results, and to computer models (WACCM-X).

Yes, comparison with other observations and models place these measurements into context.

4 The paper gives a long term analysis and discusses possible trend breaks. These results are questionable because of the lack of winter data.

Winter data is provided. It is the southern hemisphere *summer* data that is lacking. We make this clear from the outset of the manuscript.

5 The authors see a quasi-quadrennial oscillation (QQO) in their data. They announce a detailed discussion in a second part of the paper. This should take into account recent work in the literature on 3 – 5 year oscillations.

The second part of this work is available in discussions as acp-2019-1097

6 The paper is well written, and is recommende for publication after some modifications.

Thank you.

Major Comments

Line 221 pp: Figure 3 indicates five oscillation periods. The approximate period lengths are 2x 3 yr, 1x 4 yr, and 2x 5 yr. It is not obvious that a mean can be taken. Superposition of a 3 yr and a 5 yr oscillation should be checked (see for instance Offermann et al., JASTP 135,1, 2015).

The quasi-quadrennial periodicity revealed in the residual temperatures (seasonal, solar cycle and long-term trend fits removed) is an interesting feature and forms the basis of part 2 of this work; available in discussions as **acp-2019-1097**. We examine many possible sources for this feature using correlation and composite analyses with other data sets. We cannot ascertain whether the oscillation is a superposition of 3 and 5 year periodicities.

Line 383 pp, 540: The paper Offermann et al., 2006, should not be used to demonstrate a trend break. It was outdated by Offermann et al., JGR 115, D18127, 2010, who show a longer data series.

The reference to Offermann et al., 2006 applied to a time period when trend breaks first began to appear in the literature in relation to mesopause region temperature trends. In that context the reference is still valid despite it being later updated. We have added the updated reference (Offermann et al., 2010) to the paragraph noting the continued occurrence of trend breaks.

Offermann, D., P. Hoffmann, P. Knieling, R. Koppmann, J. Oberheide, and W. Steinbrecht (2010), Long-term trends and solar cycle variations of mesospheric temperature and dynamics, J. Geophys. Res., 115, D18127, doi:10.1029/2009JD013363

Line 405 pp, Section 5.1, 5.4: In the discussion of the trend data it should be elaborated that the summer data at Davis are missing. Trend data are different in summer and winter as shown by the MLS data in your Fig. 6. They can also vary from month to month as shown in your Fig. 5, but variations could be much larger (see for instance Offermann et al., 2010, their Fig.9). Possibly the summer trends are larger than your 1.2 K/decade (by number), and so might be the trend of annual data. Hence, if you want to include Davis data to Tab.1 please use annual MLS data!

The fact that we derive a trend in the mean *winter* temperatures is explicitly stated in the first sentence of the discussion in section 5.1 (lines 407-408) and in the first sentence of section 5.4 (line 619) "southern hemisphere (SH) *winter* months (AMJJAS)". We think it should be clear to the reader that hydroxyl temperatures at Davis cannot be obtained over the summer months as the sun does not go down.

We do explore the seasonal variation in trends where possible over the observing season at Davis (fig 5) and understand that the trends are variable.

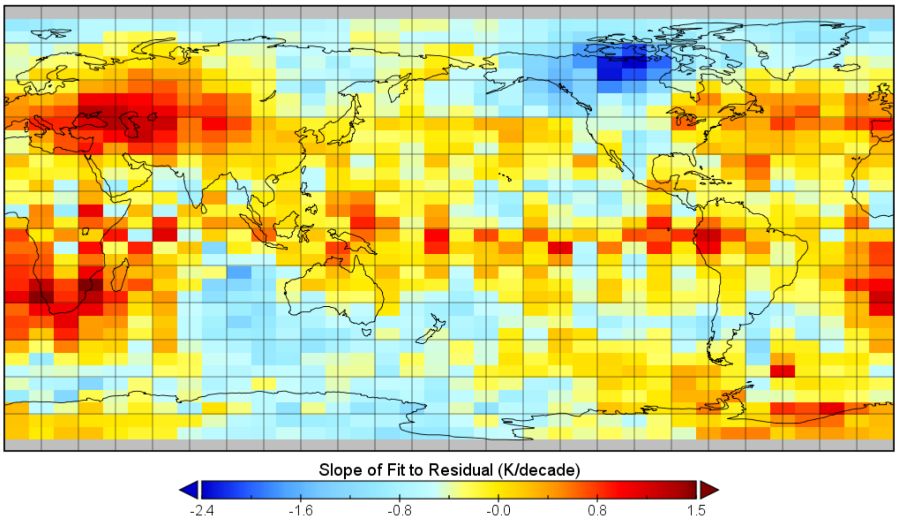
We also compare with MLS over winter and summer for each hemisphere (Figure 6) to show the seasonal difference in trends. MLS does not indicate that the summer trends [ONDJFM average trend plots in Fig 6] are larger than -1.2 K/decade for the grid box over Davis. The grid box over Davis yields a long-term trend of +0.02±0.08 K/decade for the summer months [ONDJFM] and an annual average value [JFMAMJJASOND] of -0.37±0.06 K/decade.

Table 1 is a list of ground based observations. It does not contain the MLS trends as they are globally and seasonally variable. Figure 6 shows the map of these coefficients for comparison with the ground based observations in Table 1.

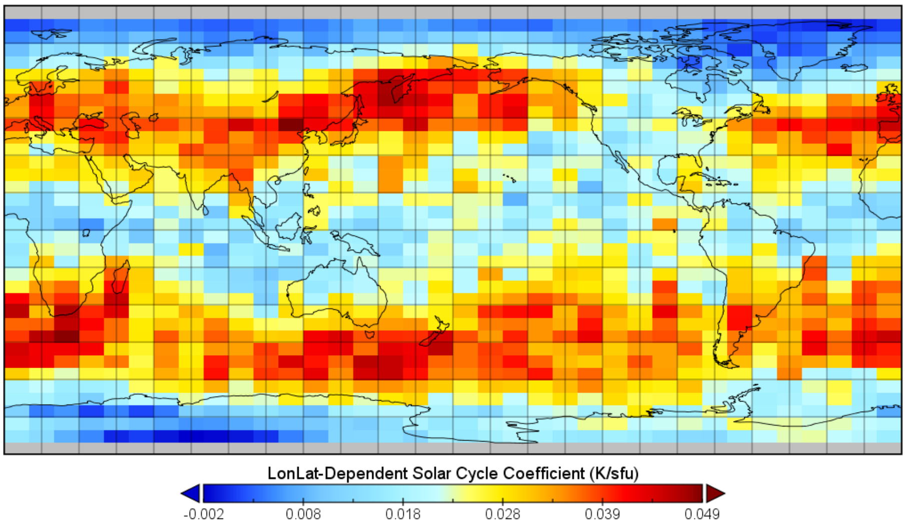
Line 558/559: . . ."no . . .sign of a discontinuity in the trend. . ." Kalicinsky et al., 2018, in their long-term analysis find that the summer data may be much more important than the winter data. Therefore please check your above statement by means of annual MLS data.

MLS values for the summer and winter seasons for each hemisphere are shown in Figure 6. It does not appear that the summer values are greater than winter values for the MLS trend coefficients globally (the two linear trend plots in figure 6 use the same colorbar scale). Coefficients maps (linear trend and solar cycle) for the whole year (all month MLS averages) are provided below for comparison with those in Fig 6. It merges the winter and summer features (as one would expect) and particularly highlights the mid-latitude maxima in solar cycle response.

MLS Linear trend coefficient [JFMAMJJASOND]



MLS Solar cycle coefficient [JFMAMJJASOND]



Minor Comments

Line 219: The error of your solar cycle response (1.02 K/100sfu) appears relatively large (see Tab.1). Do you know a reason?

The solar cycle response error is not unreasonably large compared to others in Table 1. (less than, or of the same order as 7 out of 11). We would suggest that the main reason for the error is the goodness-of-fit of the F10.7 and linear trend model. In particular that the model does not contain a quasi-quadrennial oscillation term that is evident in the residuals.

Line 295: Please give the error of the MLS trend.

MLS trend is 1.4±1.1 K/decade . added to text

Line 325 pp, Fig.6: a) Please show Panel numbers. b) Please show latitude scales. c) Part of the captions are difficult to read. d) Line 334: There is no Fig.1B. Do you mean Fig.3b?

Added panel numbers and latitude scales and modified the figure to improve readability. Yes, thank you we mean figure 3b for the comparison, but the caption has changed with the addition of all sites to the map for comparison as requested by another reviewer.

Line 353 pp, Table 1: Plese give the selection criteria for the sires shown.

We have updated the table from Table 2 in French and Klekociuk (2011) where updates were available since 2011.

Line 456 pp: ". . .peak altitudes.." Here and in the following it is sometimes unclear whether you mean the maximum of the peak or the geometric altitude. Please clarify.

We mean the altitude of the peak of the VER profile. This section has been extensively modified in response to another reviewer and this paragraph is no longer included.

Line 505 pp, 508: It is unclear whether you mean your Fig.5 or Qqian et al.. Please clarify.

This refers to the seasonal variation in the solar cycle coefficient in our Fig 5(a) and can be compared with Fig 4 of Qian et al (2019); this is clarified in the text.

Line 686: Fig.1a does not show this! Do you mean that you derived it from this Figure?

The R^2 value is shown in Fig 3(a) (Fit of solar cycle and long-term trend model to OH temperatures). This is corrected in the text.

Line 693: Sentence difficult to understand.

Modified sentence to read "Although the altitude of the mesospheric cold point changes with season (e.g., Yuan et al., 2019) and tends to be higher than the centroid height of the OH* layer, the global solar response value obtained for T-CPM (4.89 ± 0.67 K/100 SFU) is in good agreement with the solar response coefficient derived from ground-based OH* observations."

Line 706 pp: Where can this be seen? The Supplementary Material was not available to me.

The original Fig 6. also contained a plot of the error in fitting the solar cycle and long term linear trend model (see below) and the point was made here that where the error was largest coincides with a strong QQO signal. However the QQO investigation is now discussed entirely in Part 2 of this work (acp-2019-1097).

The paragraph was modified to read "As a final comment on the global trends, it is noted that the largest errors in the linear trend fit for the SH winter understandably occur coincident with the regions positively or negatively correlated with the QQO (not shown here). The fit can be significantly improved if the QQO component can be understood and modelled. We investigate the QQO in detail in part 2 of this work. "

Error in model fit to MLS [AMJJAS]