Review of "Analysis of 24 years of mesopause region OH rotational temperature observations at Davis, Antarctica. Part 1: Long-term trends" by French et al.

This manuscript presents the analysis of a very long dataset of OH temperatures over Antarctica. This is an extension of 8 years of the dataset presented by French and Klekociuk (2011). Indeed, results for the trends derived here coincide with their previous results and those for the solar response are only slightly different. In this work, the authors further identify and isolate a close-to-4-year period signal, which is to be studied in the second part of this work. Even if the results in this paper may initially look as a mere update of previous results using an extended database, they are interesting and certainly worth publishing because they show the persistence of the trend and the consistency of the solar signal. Therefore, I suggest the publication of this paper in ACP, once the following concerns and comments are taken into account.

Line numbering corresponds to the latest version uploaded by the authors (acp-2019-1001-manuscript-version3.pdf).

Main comments

There is no discussion on the effect of MLS broad vertical resolution in the mesosphere and the potential impact on the comparisons shown in the manuscript. Indeed, it would be interesting to see comparisons with SABER, even with a smaller winter temporal-coverage. Additionally, SABER provides information on the altitude of the OH layer, potentially providing a more accurate approach. In the same context, the choice of a fixed pressure level in MLS data (seemingly done based just on a better agreement) is not very well justified, as the layer altitude varies.

There is also a lack of discussion on previously reported seasonal or latitudinal effects on trends that the authors mention but do not connect with their results. It would be useful to overplot these results from other authors on the corresponding figures in the manuscript.

The discussion section is too long. It is a good review but it is not easy to follow and, more importantly, to see how the results presented here fit on the discussion. I suggest revising the section, shortening it and putting the results of this paper into the context.

Other comments and suggestions are:

L42-45 Include in the abstract the result of the global MLS trend analysis.
L148-L151. Is there any error associated to that interpolation?
L151. Is that 2% contribution independent of temperature? Or in other words, do potential uncertainties in the Q-line contribution incur into significant errors in the derived temperatures?
L157. Please, write a short sentence explaining why your choice is Langhoff et al. (1986). Do you reach better agreement with satellites when using specific values?
L160. Did you explicitly test the effect on trends of the probabilities used? How much is "not significantly"? This is important in order to understand differences in trends between the different datasets.
L161-163. This sentence is somehow redundant. If biases due to the choice of probabilities can reach 12K, it is obvious that comparisons between different instruments depend on the choice. Please, delete.
L169. Specify selection criteria quantitatively.
L183 and Fig. 1. There are more recent versions for MSISE. Use latest version or at least show that it makes no difference.
L190. Introduce Fig. 2 at the beginning of this paragraph.

L207-210. Remove this text from the caption. It is already in the text
L228. Are these MLS nightly means? Is there any time difference criterion used? If not, consider discussing possible sampling effects.
L246-252 and Fig. 4. This is a very nice and colorful figure but is it really essential or a short sentence would be enough?
L284. The choice of this altitude range sounds somehow arbitrary. The OH layer is not centered at 85km. Please, justify. What happens if you use 85-90km?
L290. This is the main problem I see using MLS data for this study. Why don't you use SABER data, even with a smaller time-coverage during the winter months? It could do a good job in your 60-day running means solar and linear trends. Garcia-Comas et al. (2014) showed that MLS biases at these altitudes could be large on the South Pole.
L293. The choice of this pressure level is not justified. You could be getting a good agreement due to a bias that could be masking a wrong selection of altitudes. Indeed, it is well known that the altitude of the OH-layer is variable (as you even mention in L582-584). Please, discuss this point.
L299. Yes, you show this very nicely in Fig. 5, my favorite figure of the paper.
L302. Indicate the figure where this is shown.
L304. Aura/MLS trends "at 0.00464 hPa"
L307. How did you derive that this is the OH equivalent pressure level and that it does not change with latitude? Does SABER, measuring OH emission and temperature vs z and pressure, show a significant change of equivalent pressure with latitude?
L308. Monthly anomalies? 60-night running means? What are these?
L316. What is the origin of these enhance bands?
Section 5.1. I enjoyed reading the review but it could probably be shortened and better organized and your results should be put into the context you describe. Are they reasonable? Do they agree? Does the seasonality of your data agree with other results? Does it agree with the expected variations? I suggest extending the title of this section in order to include ozone.
L440. Include reference.
L449. Include reference.
L465. This is already said, also mentioning the same reference.
L478-486. It is not clear to me what this has to do with this work. Hervig et al. (2019) paper mainly deals with the paradox on the solar response of H2O. Perhaps mentioning only their result related to temperature makes more sense. By the way, do you actually see a change of solar response of temperature from 1995 to 2018?
L488. Also Solomon et al. 2018
L501-502 I do not understand "global averaged temperature (..) as a function of latitude". How does their month-to-month variability compare with the seasonal variability you derive? Please, overplot on Fig. 5 and discuss.
L507. Please, write altitude
L559. Write "from 1995 to 2018". If you remove your QQO, don't you see such breaks? From your measurements, it seems that your trend is not monotonic. Quantify "no obvious sign".
L584. Please, also mention Liu et al. (2006).
L592. Garcia-Comas et al. (2017) also estimated the trend and solar response of OH* altitude and temperature from SABER.
P610. What is the expected change in MLS temperatures due to a change of 0.02-0.04 km? This may lead to a bias in the comparison between DAVIS and MLS. A way to test this could be done with SABER data by comparing temperature trends at a fixed altitude to temperature trends at a OH VER weighted temperature.
L657-661 Why are these results so different? Could the difference be due to sampling?
L720. According to a possible contribution of 30% by ozone, these values have (at least) a 30% error.
L723-726. Could you provide the reason for this minimum in 2009?

L747. Mention here also the results from other OH observations (those listed in Table 1).

Fig1b. Do all years contain the same days of measurements from doy 106 to 259? If not, there could be a sample bias.
Figure 2. Time coverage changes with doy. What is the effect of DW1?
Fig 5.a. the minimum in solar trend is during the month when downwelling is maximum. This migh be an indirect compensation of the cooling due to the direct dependence of downwelling (warming) and solar flux (COMPROBAR!!!)

Fig 5b. Perhaps you might be sounding different altitudes? What is the seasonal change of the altitude of the OH layer? Did you look at SABER data? Also, this might be connected to O3 trend seasonality or CO2 trend seasonality.

Figure 5. Define grey boxes and blue dots.
Figure 6. What is time sampling for MLS? Are you removing tides? Trends strongly depend on sampling (Rezac et al. 2018)
Fig 6. Please, overplot trend at Davis on the 1d plot. Also indicate CAP and LEO position on the maps.
Fig 6. b. The blue/red bands at 70N in the NH winter months look like the trend and solar response related to stationary PWs.
Fig. 7. What is the realtionship between this plot and the temperature anomaly? Can it help to explain differences between DAVIS and MLS?

Table 1. Discuss these results in the text, particularly mention them in section 5.
Table 1. Include MLS results in this list