

Review of "Analysis of 24 years of mesopause region OH rotational temperature 1 observations at Davis, Antarctica. Part 2: Evidence of a quasi-2 quadrennial oscillation (QQO) in the polar mesosphere" by French et al.

This manuscript reports on the detection of a quasi-quadrennial oscillation detected in the SH polar winter OH nightglow, MLS and SABER temperatures. The feature is characterized after the removal of the solar signal and a linear trend as described in Part 1 of the study. The signal has a counterpart in the NH according to MLS data. The authors suggest a relationship between the temperature QO and the changes in the mesospheric meridional winds and the corresponding changes in downwelling through adiabatic cooling and heating. The authors also show a significant correlation with the SST and provide an explanation of the connection with the mesospheric QO through a changing GW filtering by modulated PWs.

I found the paper interesting in general and well written, the methodology used is convincing, and the results are new. Therefore, I think it is in principle suitable for publication in ACP. However, I must admit that I sometimes got lost when reading the discussion in Sect. 4. There are several places where it is not clear to me what the authors want to state. There are cases where mechanisms that are clearly not connected to the feature (for example, due to their time-scale) are even described in detail. The discussion should be more concise in order to make the reading more fluent and the reasoning more understandable. It should provide briefer but still comprehensive information only on mechanisms that may potentially be connected to the mesospheric QO. Also, the authors should try to use a more cautious wording because they only suggest the origin of the feature through reasonable connections but do not provide a solid proof. In summary, in order to improve the paper, I urge the authors to re-write and significantly shorten section 4. Besides taking into account the suggestions listed below, I think the paper would that way be ready for publication in ACP.

Comments:

L19 "peak-to-peak" amplitude

L27 suggesting -> suggests

L27 That the authors have found a correlation between the QO and the CO vertical transport does not necessarily mean that a "substantial part of the QO is the result of adiabatic heating and cooling driven by the meridional flow" but just that the QO is plausibly linked, at least, to the change in meridional flow. Please, provide some quantification proving such substantial contribution or re-write the sentence.

L30 In case it is not confirmed by modelling, please, replace 'suggest a tidal or planetary wave influence in' by 'is consistent with tidal or planetary waves influencing'

L32: 'potentially' -> 'plausibly'

L40, insert comma after Antarctica

L78. Close parenthesis

L147 'Retrieved' might be ambiguous in this context. Do you mean 'selected'?

L148 Did you select both daytime and nighttime MLS profiles? If so, did you check mean biases with respect to nighttime only profiles?

L153 The selection of this altitude is somehow artificial. The OH-layer altitude changes with time. Explain using SABER data the year-to-year change of this altitude and its effect on MLS data. Also explain the relationship between the QO in temperature and the QO in altitude.

L184 What is the error in temperature due to a 1km-vertical shift?

L190 Your figure 2b shows that the seasonal variation of the QO is significant, further, its year-to-year change is quite important. Indeed, according to the colour lines in Fig.1b, the large 2011 residual temperature seems to be influenced by the lack of measurements during days 141-195, with lower temperatures. This suggests that, due to a potential

sampling bias, comparing SABER data with the ground-based OH dataset shown in Fig.1a, that includes doys 141-195, might be misleading. Also there should be a bias in the derived trend and solar term due to that fact. I suggest to compare just with results from ground-based OH using only the coincident days, or at least FMA+ASO data in Fig. 1b, even if this needs from another figure. This may lead to a better SABER and Davis OH agreement. Indeed, I was expecting a better agreement than with MLS, given that SABER takes into account potential variations of the layer altitude.

L190. I wouldn't say that not observing from 21 May to 14 July means that 'SABER samples the same days'. Please, correct.

L193 What trend and solar terms do OH data provide when not using doys 141-195?

L197. According to a sentence written four lines above this one, the SABER T\_VER trend is -0.77K/decade. This sounds contradictory with the -0.13 K/year mentioned in L197.

L198 This anti-correlation has a strong seasonal dependency, being significant in late autumn and very small in early spring for their case (e.g., Garcia-Comas et al., 2017) this. It is therefore not surprising that you get only slight anti-correlation when mixing temperatures for the two intervals doy 75-140 and 196-L208 End sentence with a dot.

L250 Is this MLS temperature also?

L262 What happens to the temperature-altitude anti-correlation if you separate these two intervals? This should also help to answer my previous comment on the effect of OH altitude variation on temperature.

L260 Why the detrended OH temperature is compared with the GPH anomaly instead of the detrended GPH?

L294 Perhaps, this is more like a WN1 + WN2 structure. The WN2 is more clearly seen at 50hPa.

L308 I do not think the feature is a wave-3 structure but more a wave-2. Note that the apparent wave-3 is due to the equirectangular projection.

L347 Do you mean 10m meridional and zonal winds?

L350 I do not see a clear correlation between sea ice and the OH QO, except for mesoscale areas. The QO is however more clearly correlated with the SST in Fig.6b or the winds in Figs 6c-6d. That the sea ice concentration should be related to the SST and the winds seems not really big news. Then, what is the additional information from OH-QO and the sea ice correlation here? If it is difficult to provide a satisfactory answer, I would suggest changing the title of section 4.1 to "SST and winds", suppressing Fig.6a and the long discussion below on the sea ice QO (L376-L386).

L359 Do you mean cyclonic circulation anomalies? Note that you show correlations in Figure 6 and not winds.

L366 I think the reasoning is in general correct but what the authors show here is a strong connection between the QO and the SST and also links to the surface circulation. The phenomenon where the origin resides cannot be determined from this type of analysis.

L369 Again, unless I missed something, I do not understand the interest of mentioning the sea ice in this work unless the authors see it as a potential source of their OH QO. Please, provide a more convincing argument or suppress.

L385 QO temperature signal at what level?

L405 colder 'mesospheric' temperatures

L414-426 I do not really understand the need for this discussion. Even if the tides are affected by the QBO, there is no impact of QBO-ENSO on OH QO. Could you please extend on the point here?

L415 Not particularly but only in the cold years.

L423 'longitudinal' wave patterns

Figure 7a. Is that really detrended OH T, as the x-axis title states? Please, write also in the caption "zonal mean meridional wind anomaly". Also, this figure is somehow redundant with Fig. 1. Winds could be overplotted there.

Figure 7b. I think Fig. 7b is not needed. The correlation is clearly seen in Fig. 7a. Remove Fig 7b or, at least, move it to the supplement.

L439 Do you mean the background 'meridional' wind?

L438-442 I think it is well known that the mesospheric poleward circulation is connected to downwelling and adiabatic heating below 100km. I do not think the references credited by the authors are the first ones showing that.

L444 'the adiabatic action of the residual meridional circulation' sounds confusing and may be misinterpreted

L452-L454 Please, remove this sentence. It is not necessary to provide the sources and the sinks of CO. Just knowing that it is a dynamical tracer due to its long lifetime, particularly, during polar winter, is enough.

L464 I find the anti-correlation between temperature and the CO trends interesting. This might be out of the scope of this paper and not worth to mention in the paper but: What are the errors of the CO measurements? Is the instrumental drift characterized? Can the authors provide a link for the trend anti-correlation? Perhaps CO<sub>2</sub> increase?

L473 Would the correlation be better if the CO detrended data were used? Given the nature of the link between CO and T QQOs that the authors are providing, does it make sense to provide 8 maps with projections for selected altitudes instead of just one plot with a lat-z cross section? On the other hand, if the QOO is clearly exhibited in the SH polar cap average (see Fig. 2 and Fig 3), why the correlation with T and CO QOO is not ubiquitous south of 65S?

L495 Temperatures at what pressure level?

L498-500 I would not call that "somewhat smaller". There is a very weak QOO in the NH winter.

Section 4.5 Why is this section called "inter-hemispheric coupling"? Certainly, Figure 10 shows NH and SH residual temperatures but their connection is not discussed at all.

L530 If that is the case, why the QOO is largest during the SH summer?

L544 Please, state this is true for SH.

L550-554 I do not think that results from Sato et al. (2012) (showing a close to annular structure around the Arctic edge, with maximum values over the Antarctic Peninsula) are well reproduced by Fig. 2a and 2b (showing more or less homogeneous correlations over the Arctic and a lack of correlation over the Peninsula).

P554-558 Could the authors provide some conclusion after these sentences and make clear the relationship with their work?

L562-570 The vertical and latitudinal structure of Zhang et al.'s TO is similar to the QBO. However, the authors showed no correlation of their QOO with the

QBO. Then, why this discussion of that TO if the four-year oscillation is not even discussed in Zhang et al.? Please, clarify the interest of this discussion or remove. Please, also note that, even if Zhang et al. showed no evidence of the four-year oscillation at 45 km, they did at 85km.

L570-572 From the discussion in the previous section, I thought that the authors were suggesting that mesospheric QOO was related to orographic GWs.

L573-575 Did Liu et al. find any QOO in their GW potential energy at the equator. Figure 2b shows a QOO there at e.g. 81km

L579-585 That a changing eddy diffusion is not needed anymore to explain SABER CO<sub>2</sub> trends has nothing to do with its potential link with a QOO.

L594 Replace the fist two dots by a comma

L596 Offermann et al. note in their introduction periods ranging from around 2 yrs to 11 yrs. What periods do the authors refer to in this sentence? All of them?

L623-624 If the polar cap also shows a QOO of similar amplitude and in phase with Davis (as you write in the next sentence, it is obvious that the Davis QOO is positively correlated with the polar cap QOO. Please, re-write.

L631 extends vertically "at least" from the mesopause

L646 Would a significant variability in atmospheric tides be actually expected at this high polar latitudes?

L653-655 I do not think the authors prove this connection to be "most likely".

L669-672 Is the Davis QQO anti-correlated with the NLC boundary latitude reported by Russell et al.?

Fig 2 caption. I think the caption for b) is not correct. If "as for a)", shouldn't it be just "correlation of the SH polar cap average", without "0.0046hPa"?

L921 What green circle?

Figure 6. Please, indicate the maximum and minimum values in the legend of the color scale for the lower panels. Are the two color scales the same? If the answer is yes, just keep one of them. If the answer is no, use the same color scale for the four panels.

Figure 10. Please, use the same scale for both the NH and the SH residuals. Otherwise, they are not easily compared.