Comment on “Six years of mesospheric CO estimated from ground-based frequency-switched microwave radiometry at 57◦ N compared with satellite instruments” by P. Forkman et al.

The paper describes a unique data set of mesospheric CO obtained by a ground based microwave radiometer operated at Onsala over a period of more than six years. This actually is the longest data record of CO from the ground. The altitude range covered extends roughly from the stratopause to the mesopause with a vertical resolution of approx. 15km. The paper gives a clear description of the instrument, the error analysis and the retrieval process. Obtained CO-profiles are then compared with several satellite instruments using different observational methodologies.

The paper is clearly written and well structured, in most aspects straightforward to follow and most figures are of good quality.

However I have one problem with the paper regarding the altitude range of the measurements that I would like to have clarified before acceptance for AMT, plus several smaller issues as stated below.

Altitude range:

The altitude range that can be covered by a microwave radiometer depends on bandwidth and spectral resolution of the receiver plus the fact that pressure broadening must dominate over Doppler broadening. The nominal resolution of the spectrometer is 25 kHz (Table 1). Pressure broadening of the CO line is 23.33kHz/Pa (Table 2). The spectrometer resolution would thus allow roughly to get some information from pressure broadening up to a pressure level of 1Pa i.e. 80 km. Doppler broadening of the CO-line at 115 GHz is approx. 110 kHz (calculated according equation for Doppler broadening parameter as given in the literature, assuming a temperature of 200K). This value is 5 times higher than the one for pressure broadening. I am inclined to conclude that a retrieval of the profile only makes sense up to 5Pa or approx. 70 km.

The authors claim that pressure broadening dominates up to 85 km (line 7, p. 3912). This discrepancy needs some explanation.

Later on the authors say that they will only present profiles up to 70km and column density above (l. 24, p. 3920; and l. 21, p. 3921). On the other hand they say that the measurement response is above 80% for the altitude range from 55 to 85 km. They indicate that column density is obtained from profile measurements above 70 km, by converting VMR to number density using the temperature profile. I do not understand why they give column density at all if they claim to retrieve a profile up to 85 km. This is very confusing and definitely needs more clarification.

Specific comments:

Radiometer optics:

In Table 1, a beam width of 3° is given. I assume this is the beam width of the whole system including the Teflon lens. The lens will have some loss.

Question: where is this loss taken into account? This loss is not negligible (see for example Goldsmith, Quasioptical systems, eq. 5.29). Even if it only is a few percent, this needs to be corrected for otherwise an offset might show up when comparing with other instruments! The correction can be done exactly in the same way as given in equation (3). What is indicated there as $T_b(z_0)$ actually is the brightness temperature before entering the lens.

Figure 1: In the block diagram of the receiver it shows some power coupled out after the Gunn oscillator that goes back to the Gunn. Probably the bold arrow has to go to the harmonic mixer.

Figure 2: What is the integration time that leeds to these spectra?

Retrieval:

I would suggest to indicate on line 18, p. 3917 that y=f(x)=Kx

On line26, p. 3918 it is claimed that temperature is retrieved. This certainly is not the case. The temperature is used in the forward model, but it is not retrieved.

L. 15, p.3919. Give a short justification for the correlation value of 0.37. Or is there a reference to a previous paper?

In the forward model only CO, O2 and O3 is taken into account as stated on l. 5, p. 3919. What about H2O? This certainly is of importance. Give some details.

In the lower side band, exactly at the image position of the CO-line, there is a NaO-line according the JPL catalog. This line probably is very weak and I have to admit I have no idea what the NaO-profile in the mesosphere is. NaO plays a role in mesospheric airglow what could be of interest. This is just a side comment and needs not be addressed by the authors in a revision.

Table 2: please also indicate the temperature dependance of the broadening parameter.

Results:

Line 23. p. 3920. states that the negative overshoots for the VME averaging kernels above 70 km are something usual. Give at least a hint to the reader why this might be the case. The justification that it is not important as column densities are considered is not satisfactory. Particularly not in context with my main concern (see above) as the column density seems to be determined from the profile!

Figure 3: I suggest to cut off the profiles above say 100km as no reasonable information can be expected.

Figure 3: The pressure scale has no minus sign in the exponentials. Something is lost there.

Figure 4 and 5. I suggest to cut anything above 100km

Figure 6 is difficult to read. Use thicker lines.

Figure 6: This is the only place where the authors use accuracy and precision for systematic and random errors. I suggest to indicate the x-axis accordingly and remove the words accuracy and precision as this might lead to lengthy discussions unless you clearly indicate somewhere in the text what you define as accuracy and precision.

Figure 7: I suggest to use 12 months ticks, after all the year still is not decimal.

Figures 12 and 13: I suggest to cut off everything below 50 km otherwise you have to explain the funny shape in the relative profiles. But as you do not retrieve below 50 km it really makes no sense.

Some minor stuff:

l. 1, p. 3911: The dynamics of the mesosphere…

Figure caption to Fig. 2: The frequencies are given relative to the CO…

last line on p. 3925: … relative to the…

l. 3, p.3932: fairly transparency, should that be fairly transparent..