Response to the review of Hugh Pumphrey

immediate

May 1, 2013

We thank the reviewer for the very helpful comments and suggestions on our paper. They have all been considered and they helped to improve the article. Please find a point to point response below; we print the referee's comment first followed by our response.

Abstract *"due to very low CO concentrations below approximately 80 km altitude in summer, profiles can only be retrieved during Antarctic winter." That isn't strictly true. It may be the case that your retrieved profile has sufficiently large error bars and a sufciently small mixing ratio that it is not statistically different from zero. But you can nevertheless obtain the profile.*
True. We have replaced ...can only be retrieved ... by ...are only presented...

Abstract *The wording should make it clear that the DOI presented at the end of the abstract (http://dx.doi.org/10.5285/25d329ad-69de-4bd3-846d-427863b27781) is for accessing the data.*
Done.

Page 3 Line 18 *"The profiles presented are stored in the database of the British Antarctic Survey." It might at this point be worth giving a URL by which the reader can access the data. It is probably most robust to give http://dx.doi.org/10.5285/ DE3E2092-406D-47A9-9205-3971A8DFB4A9 Although the DOI has been given at the end of the abstract, not everyone is yet aware that URLs of the above form can be used to locate datasets. It is worth noting that this URL currently points to a page that describes the data and has a button at the top labelled "Request data" which takes you to a form to be filled in. However, if you scroll down the page you discover that it is not necessary to fill in the request form; you can download the data immediately.*
We have added the doi to the text again and the "Request data" button on the webpage has been removed.

Page 6 line 2 *". . . the baseline can be approximated by a 6th order polynomial curve fitted to the data." Approximated how well? The authors should explain why they chose a 6th order polynomial and not any other order. They should also explain how they prevented the polynomial from fitting part of the spectral line itself. When I worked on this data I found that fitting the baseline with a quadratic (over a smaller frequency range) tended to give this sort of problem. It may be that by choosing a large enough bandwidth the polynomial uses all of its fitting potential to fit the standing waves and none to fit the lineshape. Or maybe the authors excluded the line centre region when determining the polynomial coefficients.*
No, we do not exclude the line center when determining the polynomial coefficients. In addition, the polynomial baseline fit is performed at the same time as the fit of the spectral line, during the optimal estimation.
The 6th order polynomial was found by systematic trial and error. For the 40 MHz of band width the testing showed this to be a good compromise between a stable retrieval without oscillations and fitting too much of the line itself which would lead to a loss of sensitivity at the

lowest altitudes. An impression of the performance of the baseline fit is given in Fig. 1. The plot shows every 300th retrieved spectrum together with the polynomial fitted to it.

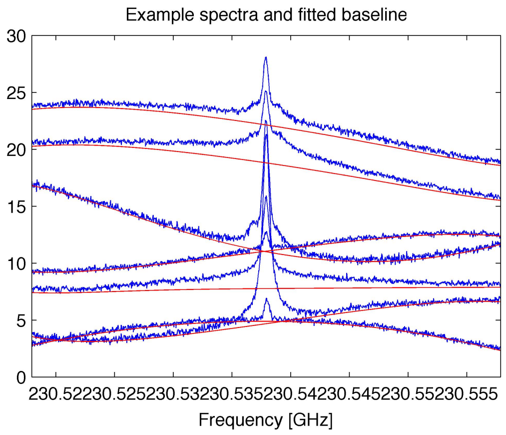

Figure 1: Examples of spectra with the polynomial baseline fit.

To the paper we made the following changes:

- Replaced Fig. 3 in the paper by Fig. 2 here.
- **Page 6 line 7:** ...by a low-order polynomial curve fitted to the data as shown in the top panel of Fig. 2.
- **First paragraph of section 3.1.**: For the baseline fit we choose a polynomial of 6th order as systematic testing showed this to be a good compromise between a stable retrieval without oscillations and fitting too much of the line itself which would lead to a loss of sensitivity at the lowest altitudes.

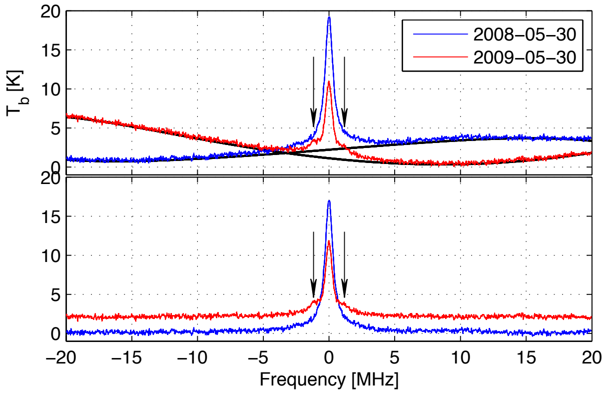

Figure 2: Calibrated CO spectrum after tropospheric correction as measured before (blue) and after (red) 9 August 2008. The black arrows mark the approximate position of the shoulders the spectrum acquired after 9 August 2008. Top: spectrum before baseline correction and 6th order polynomial fitted to the data. Bottom: spectrum after baseline correction. In the bottom plot the spectrum of 2009 (red) has been diplaced by 2 K in order to facilitate comparison.

Page 6 line 4-7 *As noted above, I described the “shoulders“ mentioned here in an internal report which I delivered to Dr. Newnham. The authors should probably acknowledge this report as a personal communication although, as it is grey literature, they can‘t do a formal reference of it. The main recommendation of my report was that the cause of the “shoulders“ should be identified it is good to see that the authors have succeeded in doing this. They should probably state in more detail than there is here, or at page 7 line 20, how the form of the empirical correction was arrived at.*

We have now added the following sentence at line 6:

Initial analysis observed these shoulders and their effect on the retrievals (Pumphrey, personal communication).

The form of the empirical correction has been arrived to by systematic trial and error using the mid-summer data where the line is virtually pure-Doppler and very narrow due to the cold temperatures. This highlighted the shoulders, which showed themselves to be mirror images of the main peak caused by the improper phase lock of the second local oscillator. Basically the phase lock was on a beat frequency which caused the ± side lobes. We now mention this in the paper.

Page 7 line 3 *The authors should state how large the frequency offset was: I got it to be between -50 and -160 kHz most of the time. The authors should explain as far as they can the physical cause of the frequency offset. My own opinion was that it was partly a Doppler shift due to winds in the mesosphere and partly an instrument calibration issue. But I never learned enough about the instrument to quantify the relative sizes of these two effects.*

The frequency offsets we find are shown in Fig. 3. It seems to be mostly beween -40 and +60 kHz drifting towards more positive values with time. This most likely points toward some instrumental drift. If the offset would be caused primarily by a Doppler shift due to the winds in the mesosphere we would expect a strong and repeatable seasonal component to it which is not clearly visible in the data.

In the paper we added the following sentence to the first paragraph of section 3.1.:

The frequency offset we retrieve is mostly between -40 and 60 kHz and, although there may be some component of Doppler shift due to mesospheric winds, this offset is mainly attributed to instrumental drift.

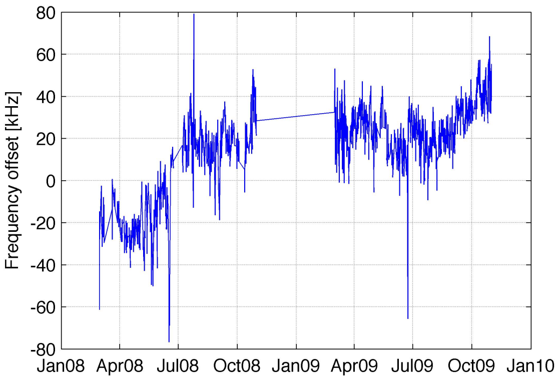

Figure 3: Frequency offset found in the optimal estimation

Page 7 line 7 and Fig. 5 *I note that in both of the examples in Fig. 5 the a priori profile is considerably larger than the retrieved profile. I can‘t tell if that is always the case as the supplied data files do not contain the a priori profile. The authors might like to consider adding an extra*

column to the data files containing the a priori profile assumed for each file. The difference between retrieved and a-priori profiles is considerably larger than the a priori errors. This suggests to me that it might be possible to relax the a priori errors a little, achieving slightly less of an a-priori contribution to the final result, without the retrieved profiles becoming too ragged.
It is true that the apriori is considerably larger than the retrieved profile. This is because the WACCM model output from which the apriori has been constructed has a positive bias compared to our results. From Fig. 7 in the paper it can be seen that this positive offset in the apriori is present throughout most of the winter. The apriori has now been added to the data files, which meant that a new DOI had to be issued (updated in the new version of the paper). We have tried to use a larger apriori covariance. However, that leads to an oscillation with negative VMR values at the altitude where the Doppler broadening becomes larger than the pressure broadening (approximately 0.1 hPa). Other than that it does not change the results much. Therefore, we decided to constrain the retrieval by using this small, but realistic, covariance.

Page 9 lines 22-24 *For these plots (described below) the values closest to the indicated pressure levels are used without taking differences in the vertical resolution of the datasets into account. This may or may not be a mistake, given the very different resolutions of the ground-based instrument when compared to MLS and WACCM. The authors have done the convolution with their instrument's averaging kernels for the comparisons shown in Figs. 8 and 9, so they should be able to state how much difference it makes to ignore the resolution difference. Also, the authors should state how the MLS data for this plot were selected in terms of horizontal co-location. It is possible that they have used the 1° latitude, 5° longitude criteria mentioned on page 10 line 13; if so, it is worth remarking that there are days when as many as four and as few as zero MLS proles full their criteria.*
After reconsideration we have changed this. We now use the same convolved coincident profiles for this plot as for Figs. 8 and 9. As this is not an atmospheric science paper but an intercomparison between different instruments we decided that it is better to actually present comparable datasets in Fig. 7.
In the text the description of Fig. 7, which includes the coincidence criterion, has been moved to Sect. 4.3.

Fig. 3 *I do not find that this figure conveys the nature of the shoulders described at page 6 line 5 very clearly. They become somewhat obvious when you are working with the data yourself, but it is not easy to construct a figure that conveys them clearly to the reader. My own attempt is in Fig. 1 below. I have used a different range of colours for the "before" (greens) and "after" (pinks) periods, indicated the location of the shoulders with arrows, and included several spectra (each displaced from the next by 0.25 K) in order to distinguish the "shoulders" from the day-to-day variability. Even with all this, you have to study the plot for a while in order to see the nature of the change that occurred on 9 August 2008.*
We replaced this Figure with Fig. 2 which we believe shows the shoulders more clearly.

Fig. 7 *It is easy to see from this figure that the MLS and MR data are correlated on a timescale of months. But it is difficult to infer from this figure whether they are correlated on a timescale of days, or whether all that short-term variability is noise in one or both instruments. I am not sure what solution to suggest, but the authors may like to see whether they can identify one.*
Fig. 9 presenting the correlation coefficients has been removed. We now present all correlation coefficients including those for variations of less than 20 days in Sect. 4.3. when we describe Fig. 7:

> The three different timeseries of coincident profiles on two pressure levels in the mesosphere (0.3 and 0.03 hPa, approximately 56 and 72 km) are shown in Fig. 4

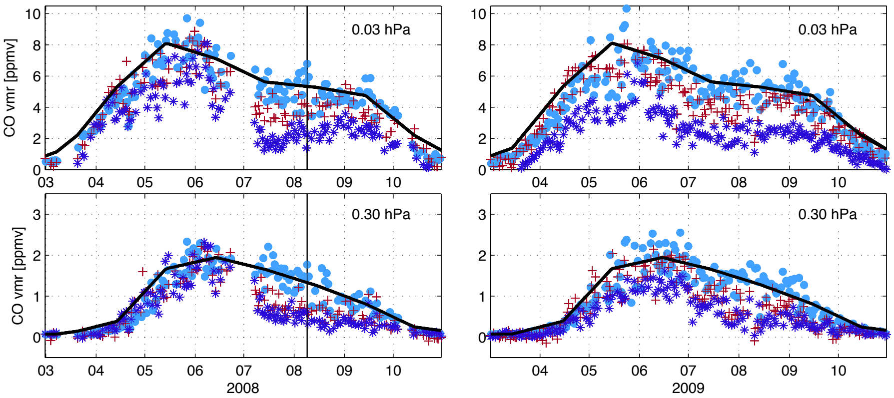

Figure 4: Time series of the BAS radiometer data (blue $*$) at two pressure levels (top: 0.03 hPa, bottom: 0.3 hPa) for the Antarctic winters 2008 and 2009 together with MLS (red $+$) and SD-WACCM data above Troll station (light blue $\cdot$). The black line indicates the apriori VMR used in the BAS radiometers retrieval.

> indicating good general agreement. From this figure it is clearly visible that the MLS (SD-WACCM) and BAS radiometer data are correlated on a timescale in the order of months. The correlation coefficients are larger than 0.9 (MLS) and 0.7 (SD-WACCM) at all altitudes and statistically significant at the 99%confidence level. In order to quantify the correlation on a shorter timescale we calculate correlation coefficients with variations longer than 20 days removed (high-pass filter with a cut-off at 20 days). This results in correlation coefficients larger than 0.6 (significant at greater than the 99%confidence level) between MLS and the BAS radiometer while between SD-WACCM and the BAS radiometer no significant correlation is found. The relatively high correlation of the short term variations between MLS and the BAS radiometer indicate that the short-term variability is not only due noise in one or both instruments but clearly driven by atmospheric variations. These short term atmospheric variations above Antarctica seem to be not well represented in SD-WACCM. **?** did a similar analysis for their ground based radiometer in Kiruna and found similar correlation as us between the short term variations in MLS and radiometer data. However they also found significant correlations in the order of 0.4 for the short term variations between SD-WACCM and ground-based radiometer. This could indicate that the gravity wave parametrization in SD-WACCM is more realistic above the Arctic than above Antarctica.

However, we have not found a good way to present the short-term variations in a plot. For example, an attempt which we do not plan to include in the paper, but we believe that it does clearly illustrate the correlation between the BAS radiometer and MLS, is presented in Fig. 5. This is the high-pass filtered data which has been used for the calculation of the correlation coefficients.

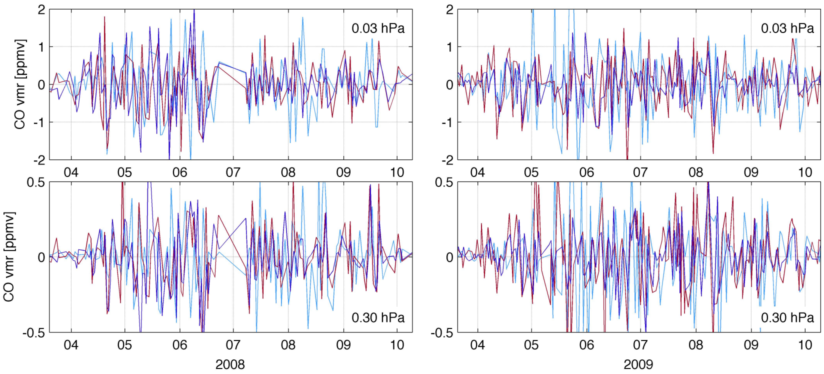

Figure 5: Highpass filtered CO timeseries at two pressure levels (top: 0.03 hPa, bottom: 0.3 hPa) for the Antarctic winters 2008 and 2009. BAS radiometer (blue), MLS (red) and SD-WACCM above Troll station (light blue).