Response to the review of Annonymous Referee # 3

immediate

May 10, 2013

Although past the deadline, in the spirit of scientific cooperation we will address the third referee's comments. Please find a point to point response below; we print the referee's comment first followed by our response.

The authors briefly describe the instrument and a correction which was necessary due to a degradation in one of the spectrometers.
As stated on page 6 line 8 of the discussion paper the spectral feature which made the correction necessary is due to an instability of the phase-lock control of the 225.6 GHz local oscillator and not due to a degradation in one of the spectrometers.

However I found only one recurrence to the work of Hoffmann (2012), where quite off-handed is said, that Hoffmann (2012) found also good agreement of the ground based data to the MLS data. I would expect a bit more depth in the discussion of previous work.
On page 12 where we cite [2] we mention that: the SD-WACCM profiles have been found to be in good agreement with MLS. We found this sufficient as this study does not have the main focus on instrumental intercomparisons but on the presentation of the data set. However, we will now also mention the comparison results between the ground-based radiometers in Kiruna and Onsala and satellites and SD-WACCM, respectively.

Also I would expect more information about the data set: In 2009 a severe change in the spectrometer appeared. Nevertheless nothing is said, if the spectrometer has been repaired. The time series stops at the beginning of 2010, but the publication is from 2012. Did the instrument stop working or has it been retrieved for repair?
As the spectrometer was not the reason for the spectral feature it did not require reparation. The phase-lock control of the local oscillator has been repaired after the instrument has been taken back to Europe in Spring 2010 for maintainance. We now clearly mention this in the outlook.

page 2 line 21, only one order of magnitude? Acc. to your gures it would be more.
We change this to: more than one order of magnitude.

page 2 line 25, please add a citation of where you got the lifetime of polar night CO.
We got the life time form Minschwaner et al. 2009 which is cited just one line above. However, we will just cite it again.

page 3 lines 18-20 the publication does not only describe the measurements but also compares them to other data. Please note this and add a citation that this has been done before and by whom.
We have changed the end of the introduction as follows:
The CO measurements of the instruments in Kiruna and Onsala have been compared to data

from Aura/MLS and model output from SD-WACCM and to data of several satellites, respectively [2, 1].
This paper describes ground-based measurements of mesospheric CO taken from Antarctica and compares them to data from Aura/MLS and output of SD-WACCM.

page 5 line 16, the information that there are 98 days without measurements is completely unconnected to the surrounding text. Why is it relevant anyway?
This has been removed.

page 12 line 13, 14, Are your results better or worse than in this study?
We do not understand what you mean by results being better or worse. We assume that you would like us to mention the results of the comparison between the two ground-based radiometers in Sweden and satellites and SD-WACCM, respectively.

page 13 line 1-3 Hoffmann (2012) present a detailed analysis of the correlation analysis. They also tried to find if the correlation coefficients are meaning full. Are you implicitly using the same line of argumentation? If so you should cite it, if not, why not? Why means that the 5-sigma renders the correlation to be significant?
We change 'significant to greater than the 5-sigma level' to 'significant at greater than the 99%confidence level'.
What do you mean by the same line of argumentation? We show correlation coefficients in order to demonstrate that the variations in the time series of the BAS radiometer and MLS (SD-WACCM) are following the yearly cycle of CO.

page 13 line 15. I don't think this conclusion is valid. Because you are changing your a priori using SD-WACCM profiles I would expect that the correlation of the apriori to SD-WACCM is already quite high.
Thanks for pointing this out. However, as we only show our measurements at an altitude range where the area under the AVK is close to unity the contribution of the apriori to our retrieved profile is negligible and therefore the annual cycle and other long term variations are coming from our measurement rather than the apriori profile. Also we are using a climatology of SD-WACCM for the apriori, and a given year may be different than the average over several. Therefore it should not matter that the apriori includes the annual cycle.

I especially worry about the fact, that the correlation to SD-WACCM after the correction is higher in the time when the BAS spectrometer had to be corrected (2009). I think a correlation of short term variation as done in Hoffmann (2012), chapter 4, would reveal much more valid conclusions.
The correction to the channel response function possibly introduces a systematic off-set to the CO profiles, but we do not expect it to influence the correlation coefficients.
However, we have restructured the 'Results and discussions' part:
As in the discussion paper we first describe how we generate a set of coincident profiles. Then we present the time series of the coincident profiles (before: all the data of each data source) with the MLS and WACCM data convolved (before: not convolved). At this point we present the correlation coefficients as well as the correlation coefficients of short term variations and mention the results of [2]. After that we present the profile comparison where we now also mention the comparison results of the studies by [2] and [1].

page 22 figure 5 I guess the AVK are not normalized to the apriori profile?
No they are not.

page 25 figure 8 What is the dashed line in the middle plot? What is meant by the "combined random errors" in the right panel? How are the plots interpreted.
The dashed line in the middle plot shows the systematic error of the BAS radiometer. We added that to the caption.
The combined random errors is the square root of the sum of the squares of the random errors of the two instruments.
The interpretation of the plots is given in the section 'Intercomparison of profiles'.

page 26 figure 9 The right panel does not seem consistent with figure 7. The values of MR (BAS?) and SD-WACCM after 2009 seem less correlated than 2008. Figure 9 show different. Is this because the model values have been convoluted in figure 9?
Figure 9 has been removed as we decided do just mention the correlation coefficients in the text. We now calculate the correlation coefficients for the whole data-set instead of before and after 2008-08-09 as we do not expect the failure of the LO to influence observed variations.
We assume that the reason for higher correlation after 2008-08-09 compared to before was the length of the two datasets. The 'after' dataset is longer and contains more long term variability leading to the higher correlation.

References

[1] P. Forkman, O. M. Christensen, P. Eriksson, J. Urban, and B. Funke. Six years of mesospheric co estimated from ground-based frequency-switched microwave radiometry at 57 ° n compared with satellite instruments. *Atmos. Meas. Tech. Discuss.*, 5:3909–3952, 2012.

[2] C. G. Hoffmann, D. E. Kinnison, R. R. Garcia, M. Palm, J. Notholt, U. Raffalski, and G. Hochschild. Co at 40–80km above kiruna observed by the ground-based microwave radiometer kimra and simulated by the whole atmosphere community climate model. *Atmospheric Chemistry and Physics*, 12:3261–3271, 2012.