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Atmospheric Measurement Techniques Discussions
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Interactive comment on "The arctic seasonal cycle of total column CO_2 and CH_4 from ground-based solar and lunar FTIR absorption spectrometry" *by* Matthias Buschmann et al.

Matthias Buschmann et al.

m_buschmann@iup.physik.uni-bremen.de

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We thank the anonymous reviewer for the feedback and suggestions. Point-by-point responses to the individual issues raised are listed below.

General Comment:
One potential source of systematic bias is due to the fact that the solar spectrum observed on the moon is a solar disc-averaged spectrum, while TCCON observes disc-centered spectra (and this is assumed in the GFIT analysis also, therefore the

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lunar spectra need to be processed with different settings).
Response:
Sunlight reflected at the lunar surface will have a (solar-)disc-averaged spectrum, i.e. the solar lines will be broadened as a result of the different Doppler-shifted contributions from different parts of the solar disc. GFIT includes a setting, that switches to a calculation of a disc-averaged spectrum, when the moon is selected as the source, therefore no bias is expected. This will be clarified in the revised manuscript.

General Comment:
A discussion of the two crucial items (accuracy budget and of the target accuracy and precision) should be discussed in the final version of the paper.
Response:
The referee is correct that the accuracy and precision of the lunar retrievals are crucial. Within the frame of this study the solar TCCON measurements are considered to be correct. Section 4 of the manuscript shows the validation with the TCCON data. Daytime TCCON data has been compared with the nighttime lunar measurements and it is assumed that diurnal variation can be neglected. We assume this is valid given that the model outputs for that time period show small variabilities in the order of 0.2 ppm (1.0 ppb) for xCO2 (xCH4) (see Tab. 2). The accuracy of the lunar measurements can be determined via the bias of the lunar compared to the solar measurements and can be deduced from Tab. 4 as well. In March 2013 the difference between solar and lunar measurements is 0.66 ± 4.56 ppm for xCO2 and -1.94 ± 20.63 ppb for xCH4. In the September 2013 campaign a bias of 1.01 ± 8.52 ppm for xCO2 and -3.36 ± 41.13 ppb for xCH4 can be observed. The diurnal variability of the lunar measurements is used to define the precision. As the later measurements have a higher precision, a typical value achieved in the 2014/2015 winter is used. This is given in the Conclusions as a standard deviation of the daily mean of 2 ppm (10 ppb) for xCO2 (xCH4), in both cases corresponding to about 0.5 %. This discussion will be added to Section 4 in the final

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manuscript and the values for the bias added to the abstract to further emphasize their importance.

The target accuracy on the other hand is more difficult to determine. As suggested by the reviewer, the detrended year-to-year wintertime variability in the models can be used as a proxy. In the smoothed, detrended MACC CO2 and CH4 model the arithmetic mean of the first week of January differs by 0.55 ppm in xCO2 and 9.84 ppb in xCH4 between 2012 and 2014. At the same time, the standard deviation of all values for the first week of January between 2012 and 2014 is about 1.8 ppm for xCO2 and 18.8 ppb for xCH4. However, these estimates are potentially subject to unknown biases in the model, i.e. the model could be biased similarly every year. Additionally, the seasonal variability surely is an upper limit for the target precision. Here the seasonal cycle amplitude measured by solar FTS is about 15 ppm for xCO2 and about 40 ppb for xCH4.

This will also be added in the revised manuscript.

Comment:
Abstract: also provide an estimate for the accuracy (bias with respect to solar TCCON measurements) of the lunar measurements.
Response:
An estimate of the accuracy as addressed in the answer to the previous comment will be added to the abstract.

Comment:
Page 2, line 28: “The extension of the bandgap . . . reduces the quantum efficiency” - is this true?
Response:

No, it should have stated "the extension of the detector sensitivity" and will be corrected in the revised manuscript. The manufacturer reports values for the noise eqivalent power (NEP), that is the power required to achieve a signal-to-noise ratio of 1, for the 1.7 μm cut-off model of 1.8E-15 $\frac{\mathrm{W}}{\sqrt{\mathrm{Hz}}}$ and 9.0E-15 $\frac{\mathrm{W}}{\sqrt{\mathrm{Hz}}}$ for the 1.9 μm cut-off model respectively. For an uncooled diode with 2.6 μm cut-off, the reported NEP is 2.1E-12 $\frac{\mathrm{W}}{\sqrt{\mathrm{Hz}}}$ (see http://www.teledynejudson.com/prods/Documents/InGaAs_shortform_Sept2003.pdf).

Comment:
Figure 1: It would be instructive to show a lamp spectrum recorded with the standard TCCON detector element also (and to provide some information concerning the noise level achieved with the selected lunar InGaAs diode (cooled and uncooled) and with the standard extended TCCON detector element for the same input signal level, e.g. for the 6000 . . . 6400 cm-1 region, where the CH4 and CO2 bands reside).
Response:
As mentioned above, the sensitivities of the standard TCCON diode and the TE cooled differ by about 3 orders of magnitude. Therefore, given the vast difference, we do not feel that adding a graphical representation of this is necessary.

Comment:
Page 4, line 10 ff: not a sentence.
Response:
Sentence will be reworded to: The differences between the solar and lunar measurements include the detector, the spectral resolution, the integration time and the size of the entrance aperture.

Comment:
Section 3.2: The fact that the noise level is too high in the lunar observations for using the spectroscopically observed oxygen column should be regarded as a severe drawback of the suggested approach.
Response:
This is a misunderstanding. The DMFs used in this study use the O2-ratio approach, the noise here is not 'too high' but merely higher than using the surface pressure approach to calculate the DMFs. However, the error cancellation properties of the O2-ratio approach outweigh the potential lower noise achieved by using the surface pressure. We have attempted to clarify this in the paper to avoid confusion.

Comment:
Page 5, line 11: ". . .for the analysis in section 4." It would be instructive to explain here to the reader which topic is covered in Section 4.
Response:
The sentence will be changed to: In the following, the approach described in equation 1 was used to retrieve xCO2 and xCH4. The second approach, in equation 2, was only used to retrieve xO2 in Section 4, which covers the validation with solar measurements.

Comment:
Page 7, line 10 ff: ". . .one option to decrease measurement time . . . is to increase the velocity . . .". No, this is not the case in the context of optimizing the spectral signal-to-noise-ratio (SNR). Here, only the spectral resolution and the throughput matter. I would have expected (for a given allowed integration time) to see a more pronounced reduction of error bars on the retrieved columns until a further reduction of resolution

starts to decrease the contrast between the lines and the adjacent continuum. Has the spectral SNR been adjusted as function of resolution in this manner (assumption of a certain amount of available integration time)? When comparing different resolutions, one might also take into account that a larger fieldstop can be applied when resolution is reduced (increasing the signal level, favoring shorter scans even more).
Response:
While the increase in velocity does increase the number of scans possible in the same time frame, this has no effect on the spectral S/N. The wording will be adjusted to clarify. Assuming the second comment refers to Figure 6, no, the data set has not been adjusted for the possible number of spectra within a certain time frame. The aim was to understand what impact the spectral noise has as a function of resolution on the retrieval. A figure highlighting the increase in signal-to-noise ratio (SNR) is included below in Figure 1 and will be integrated in the revised manuscript together with the following explanation: Here the increase in SNR was measured as a function of spectral resolution with a Bruker 125 HR, normalized to the SNR at 0.02 cm^{-1}, i.e. a spectrum recorded with 1.0 cm^{-1} resolution has a 10 times larger SNR (see blue line). Additionally, the shorter scan length allows to record more spectra in the same time frame. Averaging leads to an increase in SNR by a $\sqrt{N}$ with N measurements (red line). The resulting black line shows the potential increase in SNR with resolution for a fixed integration time. At lower resolutions the size of the entrance aperture is limited by the size of the image of the lunar disk, rather than the resolution.

Figure and discussion will be added to Section 3.4 of the final manuscript.

Comment:
Page 8, line 4: “. . .white noise were added.”. How has this operation been performed technically? In the interferogram domain before the FFT? Note that this section does not specify (nor treats) the choice of the numerical apodization function, which seems

a further important choice in addition to the scan length if reaching the best possible precision of the retrieved column is so crucial.
Response:
Here, the interferogram was cut to obtain lower spectral resolution and then Fourier-transformed. Then the noise was added to the spectrum. In all retrievals, from solar and lunar spectra, a boxcar apodization function was applied. Note that the retrieval adjusts for the resulting sinc-shaped distortion of the spectral lines in the spectral domain. Using a different apodization function would result in information loss in the spectrum. The usage of the Boxcar apodization is mentioned in the revised manuscript.

Comment:
Page 12, line 10: "air-glow emissions". This study would be especially interesting if lunar spectra taken during twilight would be treated separately (spelling: airglow).
Response:
Unfortunately, all lunar spectra recorded at a time when the sun is higher than -5° elevation have to be filtered out (see sec. 2.3). In twilight, backscatter in the atmosphere leads to a light path through the atmosphere that is not well-defined. This leads to higher retrieved DMFs, this behaviour is only partly compensated by the O2-ratio approach.

Comment:
Figure 12: Despite the fact that no biases were discovered in the September 2013 measurements, one is left with the impression that the lunar CH4 measurements in 2015 are biased high in comparison to the solar observations.
Response:
Yes, we do not see any mechanism that would explain the apparent bias between solar

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and lunar xCH4 measurements in 2015 and 2016.

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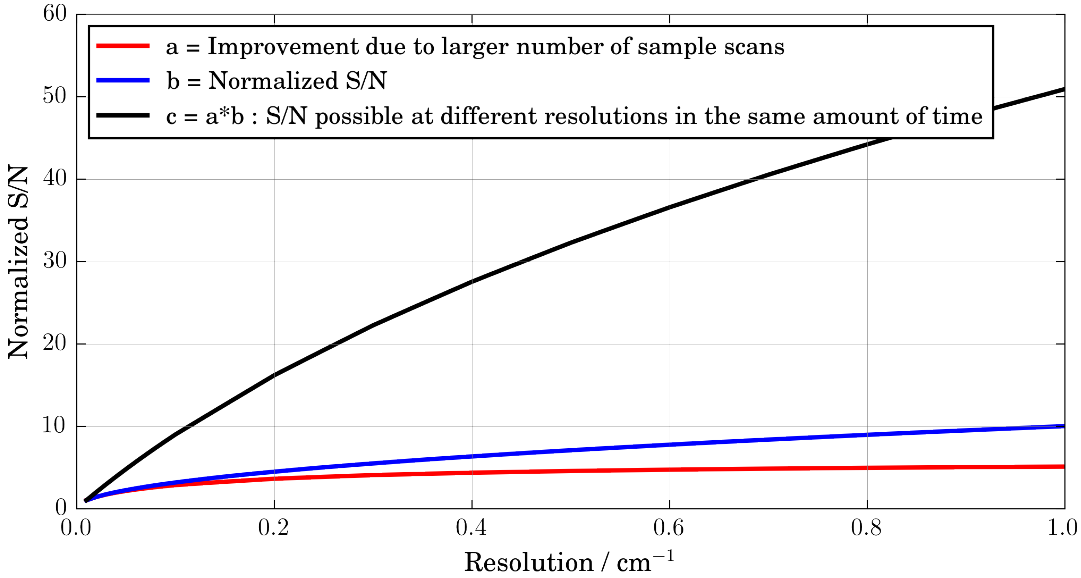

Fig. 1. The SNR improvement of a Bruker 125 HR as a function of resolution for a integration time of 5 minutes.