Reply to comments of reviewer #1 on the manuscript "An improved NO2 retrieval for the GOME-2 satellite instrument" by A. Richter et al.

We would like to thank the reviewer for his helpful comments and suggestions. In the following, we will reply to them point by point, including the reviewer's text in italic.

Introduction: Please explain the acronyms (GOME etc.).

Done.

p271, line 23: I was confused here. I suggest (if I got the logic correctly) "the settings of the operational product (Valks et al., 2011) are very similar to those of the Bremen standard retrieval. Therefore, the improvements discussed below will likely be analogue if applied to the operational product."

We have changed the sentence as suggested.

p218, line 3: "... spatial detail": please give the ground pixel sizes of SCIAMACHY and GOME2 here.

We have included the spatial resolution of the two instruments in the next sentence where reasons for the differences are discussed.

p218, Monthly Mean Comparison: are the monthly means calculated independently for both instruments, or is there a selection of collocated measurements? Would the latter make a difference? Please specify also for Figs. 10 and 11.

We have not sampled the observations to ensure collocated data, neither for the initial comparison, nor for Figs. 10 and 11. The reason for not doing so is twofold: a) we do not expect a significant impact on the results and b) the number of data points would be greatly reduced as the drift of the orbit of Metop relative to that of ENVISAT leads to time periods of poor overlap. For a comparison of the rapidly varying tropospheric NO2 columns, such sampling has been applied in the past and significantly improves the correlation.

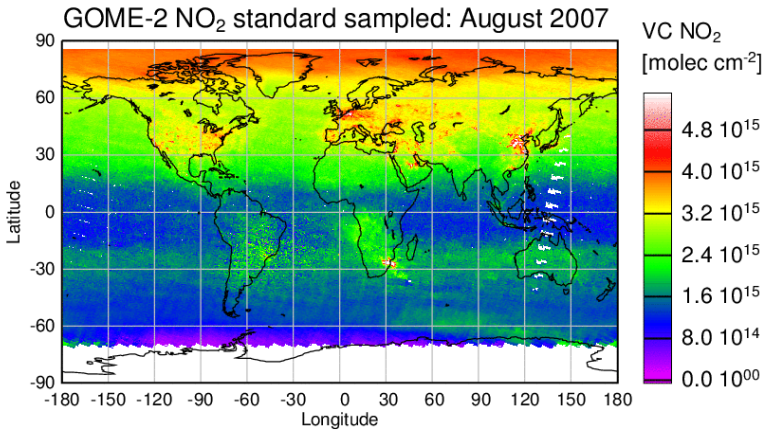

Figure 1: GOME-2 NO2 vertical columns from the standard fit but sampled to include only those locations where also SCIAMACHY data is available on the same day.

In response to the reviewer's point, we have now added an additional GOME-2 figure where GOME-2 data have been sampled according to SCIAMACHY measurements (see Figure 1). This comparison is still incomplete as SCIAMACHY data was not sampled for GOME-2

overpasses but we decided not to do this as we would like to present also the independent data for comparison.

Section 3: How sensitive are the results on the fit parameters (fit window, polynomial degree)?

This is a very general question! We have performed many sensitivity studies and found that NO2 is not very sensitive to the degree of the polynomial or the exact fitting window. The other trace gases included in the fit are much more affected, as well as the overall fitting quality. However, changes in the degree of polynomial can introduce changes in the NO2 background level of several per cent, in particular for the smaller fitting window.

As in the original manuscript we had neglected to discuss the change in polynomial degree between the two fits, we have now added a paragraph on the reasons.

p221, line 1: Why is vibrational Raman scattering not considered explicitely? How would the fit results change if it would be included?

We have not tried to include vibrational Raman in the retrieval so far. Tests on the original fitting window showed improved fits over clean water regions of the oceans and over the Sahara which is indicative of a problem. As we have not yet solved this contradiction, we have decided to continue to use the simple correction using the intensity offset in the fit but hope to be able to use a physical correction based on radiative transfer calculations in the future.

Are the additional absorption spectra for water and soils fixed with respect to their wavelength calibration, or has the fit the possibility to shift them? I would expect some impact on the results from this setting.

In our analysis, the only spectral shift allowed is between the solar irradiance and the earth-shine spectrum. All cross-sections are fixed in wavelength.

p222, 2nd paragraph: It is written that the fit can not distinguish between water and soils, unless there is a strong contribution from one of them. However, Fig. 7 looks quite reasonable (values about zero for most vegetation areas). Does the retrieval shown in Fig. 7 include liquid water as well?

Fig. 7 was produced from a fit including liquid water as well as this analysis results in the most consistent overall results. The problem mentioned in the text is linked to regions having neither strong soil, nor strong liquid water signals, mainly over the oceans but also over land. In these cases, both parameters can have non-zero values which are nearly perfectly anti-correlated. However, the retrieved columns are small relative to those over sand and therefore are not apparent in Fig. 7. This problem is only relevant as it limits the detectability of small sand signals, e.g. from aerosols.

I recommend to show an additional map of the results for the liquid water absorption analogue to Fig. 7.

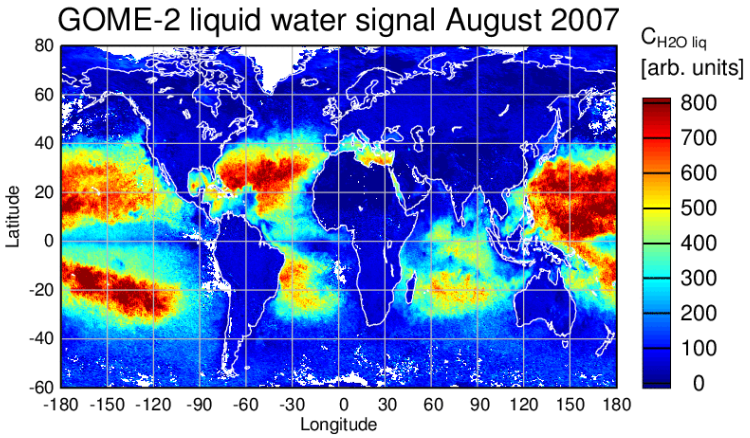

Figure 2: Fit coefficient of the liquid water cross-section for August 2007. Only data with a cloud fraction < 0.2 are included in the average.

A figure showing the retrieved liquid water absorption has been included (see Figure 2) as suggested.

The empirical correction for sand/soils is quite impressive, but should be discussed in some more detail:

As requested also by the other referee, we have added more discussion of the sand correction. However, the main focus of this study, namely improvements to the GOME-2 NO2 retrievals, should not be lost.

- Where and when both spectra have been measured exactly?
- Why can the soil signature be determined from two spectra both over desert?
- What is different for both measurements (viewing geometry, light path proxies as Ring and O4, or even the liquid water response!?)?

Exact co-ordinates and additional information on the two pixels used are now included in the manuscript. The two measurements were selected based on the size of the residual of a fit not using a soil correction, and we assume, that they are taken over different surfaces. As the reviewer points out, other differences might exist between the two measurements which can contribute to the empirical cross-section deduced. In fact, the presence of the Ring structures in the ratio (see Fig. 6 of the manuscript) indicate a difference in atmospheric light path as would be expected for different surface conditions.

We admit that this is an ad-hoc approach that should be replaced by a cross-section derived from independent measurements as soon as possible. However, as we found the same signature in many ratios of spectra we took and as the spatial pattern of the results is so convincing we thought it worthwhile to publish these results.

- Is it possible that there are contributions from vegetation in the denominator spectrum?

Yes, this is possible in principle. However, the behaviour of the fit parameter for the soil signature (no signal over vegetation, large signal over deserts) is not supporting this assumption.

- Are there any absorption spectra of sand in literature?

We are not aware of any soil spectra in the literature which cover this spectral region at sufficient spectral resolution. Low resolution soil spectra are available for example in the

Aster database but have only few points with low signal-to-noise-ratio in the spectral region used here.

- Have you tried to reproduce the sharp bend at 480 nm in the lab?

We have tried to extract the sand signature from a comparison of measurements with a DOAS instrument pointing in the zenith-sky and on a target covered with sand from the Sahara. The results show a structure similar to that deduced from the GOME-2 spectra but without the sharp peak. Use of this spectrum in the GOME-2 data analysis leads to similar maps for the sand-coefficient but significantly poorer fits. As this is based on a single quick measurement, we would like to do some more measurements using a better set-up and different sand targets to gain more confidence in the results before publishing the data.

Concerning empirical sand/soil correction: It would be interesting to look at some kind of NDVI (derived from the GOME2 spectra). This could help to decide on the speculation whether this ratio is rather a "missing vegetation" spectrum than a "sand/soil" spectrum.

Considering the behaviour of the sand correction term (small and constant values over regions with vegetation or water, large values over the deserts) we do not think that a “missing vegetation” could possibly explain the spectral structures we see.

Section 4: The Spike-removal seems to be a powerful method for reducing the noise in the SAA. Did the authors try to apply this method also for the SCIAMACHY retrieval? Though the fit window is smaller, narrow spikes still might be successfully removed without losing too many detector pixels.

We have applied the method both to the smaller fitting window on GOME-2 data and to SCIAMACHY measurements. In all cases, a significant reduction in scatter is achieved in the SAA region, but overall, the larger fitting window including the spike correction yields the smoothest results.

p227, line 3: Please be more specific: "We recommend to use the daily solar spectra for studies on..." Which solar spectrum will be used in your GOME-2 product? Or will you provide both datasets?

In order to create a consistent data set, we think that using daily solar spectra is still the best choice. Fixed solar spectra might be used for special investigations, but in these cases, one will have to decide which is the best solar spectrum to use for the specific case and then rerun the analysis. We have added some more discussion of this point in the revised manuscript.

Please comment more on the GOME-2 degradation. How does the scatter over the Equatorial Pacific and the average fit residual evolve with time?

Since launch, the GOME-2 instrument suffers from an unexpected loss of throughput. This loss is largest in the UV, still considerable in the NO2 fitting range and negligible at the large wavelength end of the instrument. The reasons for this change in throughput are not yet fully understood, but after a heating experiment performed in September 2009, the speed of further degradation has significantly reduced.

The effect on the GOME-2 NO2 retrieval is mainly an increase in scatter following the expected behaviour if photon shot noise is the dominant error source. At the same time, the fit residuals are increasing, and for the improved fitting window both are now about a factor of 1.5 larger than after launch.

In addition, after a throughput test performed in September 2009, some instrument characterisitics have slightly changed leading to larger differences when comparing measurements from before and after the test.

Details on the effects of GOME-2 instrument degradation on lv2 products can be found on the poster by Dikty et al. and in an upcoming paper planned for autumn 2011.

References: Dikty et al.: Please provide this poster via web-link.

Done.

Figure 2: What is shown on the x axis? It must be some kind of normalized (tropospheric?) VCD (Fig. 1 shows values of about 1e15 in this area).

Thank you for pointing this out! The curves have not only been normalised to have unit area, but also the mean value has been subtracted. This information was added to the figure caption and label of the x-axis. The airmass factor used is stratospheric as stated in the caption.

Figure 4: It might be interesting to show the same map for the improved algorithm as well.

Comparison of the fit residuals from the two algorithms is difficult as the fitting window was changed, more cross-sections were included, the degree of the polynomial was reduced and the chisquare over the SAA was reduced by removing those wavelengths which were responsible for the largest values. Therefore, the spatial distribution of chisquare looks quite similar, with lower values over the SAA (by definition) and higher values elsewhere (larger fitting window). We do not think that the readers can learn much from this figure and therefore have not included it in the revised paper.

Figure 5: The H2Oliquid absorption looks quite different than that shown in Pope and Frey. I assume that some high-pass filtering has been applied? Please specify.

Thank you for pointing out that this information was missing. In fact, the original data were interpolated in wavelength and a polynomial of degree 3 was subtracted as was also done for the other cross-sections shown. We have added this information to the figure caption.

Figure 6: How was the ratio smoothed without losing the sharp bend?

Thank you for pointing out this omission. To maintain the sharp bend, we have performed the smoothing separately over two windows left and right of 479 nm, respectively.