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# Authors' response to referees

We thank both the referees for very good questions and comments. They have helped to improve the quality of the paper. In the following we detail answers to the individual questions posed by the referees.

# Anonymous Referee #1

## Specific Comments & Questions:

Section 4
Regarding the discussion of Figures 8 & 9 – vertical sampling – I don't think the temporal sampling frequency is stated. I may have missed something, but it is important to state this number explicitly. Reference is made later to a 0.5 s exposure – is this the measurement frequency?
Author response: Yes, it is 0.5 s. Text has been modified.

Page 10156, line 13 – The dark limb solar zenith angle criteria is confusing as written – suggest you either say it's 107 deg or 108 deg.
Author response: Different authors have implemented different solar zenith angle limits. The illumination flag table (Table 1) gives a dark limb limit of 110 deg and in addition requires 120 deg. solar zenith angle at the Envisat position in order to control stray light. Because these strict limits restrict heavily the amount of available data, more relaxed limits are usually adopted and no evident corruption of results has been found. Therefore, we are inclined to consider the limits in the illumination table limits somewhat too cautious. The investigation of new limits is certainly an important but difficult task. Some elaboration about this topic has been added to text.

Section 5.4
If the dark charge in individual pixels can vary significantly on timescales of seconds then it's hard to see how a single dark measurement every orbit can effectively correct for the variation. I assume that this RTS phenomenon must be modeled and included as a large component of the potential random retrieval error.
Author response: While the RTS change can be large, its probability is low. Consequently, only a small number of pixels change during one orbit. The global contribution of RTS to random noise is not so large.

Section 5.6
Since no correction is done for either the internal or external stray light, does this imply that the GOMOS team is confident that these effects are not a significant source of error for the night-time occultations, or simply that you do not know how to effectively correct for

them? If data users are told to use the illumination condition flag as a possible stray-light indicator, can you estimate for them the potential magnitude of the error in these products?
Author response: The illumination flag table (Table 1) includes information about the possible occurrence of stray light. If the solar zenith angle at the Envisat position is smaller than 120 deg., there is a possibility for stray light even during nighttime occultations. This warning is, however, rarely respected as it considerably reduces the amount of data. No evident corruption of results has been found even if the stray light limits have not been taken into account.

The discussion of background removal for bright limb occultations is presumably given for completeness. I think it is stated in the paper that these data have not been deemed to be of sufficient quality to release publicly, is that correct?
Author response: No. The background is removed for bright limb occultations and then the retrieval is carried out in the same way as for night occultations. As stated in the text this removal operation is not accurate enough and therefore retrieved profiles are not of a good quality. But they are delivered to users in the same way as the night retrievals. We are working to improve bright limb occultation retrievals. In addition, we are developing a new retrieval from the bright limb scattering term (from upper and lower bands In the CCDs). These new results are not yet publicly available.

There is no real discussion of the estimated accuracy of the high-resolution temperature retrieval from the photometers (except the single comparison plot to ECMWF in Figure 23). Has this retrieval been validated? Presumably, since it's not used in any way in the level 2 processing, the assumption is that it is not as accurate as the interpolated temperature/pressure profiles from the ECMWF analysis.
Author response: The quality of the HRTP in the present processing version is still poor and its use for scientific applications cannot be recommended. This and the limited altitude range means that it cannot be used to replace ECMWF + MSIS90. A considerable improvement in the HRTP quality will take place in the next processing version. The real value of HRTP is in the fluctuations it displays, not in the accuracy of the mean profile.

We have been developing the HRTP further. The scientific processor, which includes these developments, delivers results with the following qualities. At altitudes ~18-35 km, HRTP is retrieved with accuracy of 1-2 K. The best accuracy is achieved in vertical occultations of bright stars [Sofieva et al., 2009]. The results of validation of the new HRTP product with sounding, lidar and CHAMP measurements, and comparison of HRTP in GOMOS close collocations are presented in [Sofieva et al., 2007].

Potentially important value of HRTP is the fine structure of temperature profiles (although HRTP can be smoothed also down to a coarser resolution). The validation of the small-scale structures in HRTP using the spectral analysis in presented in [Sofieva et al., 2009].

Relevant references:
Sofieva, V.F., J. Vira, F. Dalaudier, P. Keckhut, C. Retscher, Y.J. Meijer, M. Guirlet, and G. Barrot (2007): Validation of high-resolution temperature profiles (HRTP) retrieved from bi-chromatic scintillation measurements by GOMOS, Proceedings of the Envisat Symposium 2007

Sofieva, V.F., J. Vira, F. Dalaudier, A. Hauchecorne and the GOMOS team (2009): Validation of GOMOS/Envisat high-resolution temperature profiles (HRTP) using spectral analysis, in New Horizons in Occultation Research, Studies in Atmosphere and Climate, eds. A. Steiner, B. Pirscher, U. Foelsche, and G.Kirchengast, pp. 97-107, doi:10.1007/978-3-642-00321-9_9, Springer-Verlag Berlin Heidelberg

Section 11.2
Regarding the decision to fix the neutral density (Rayleigh scattering component) to the ECMWF rather than retrieving it from the data – the fact that the retrieved total density shows a large bias relative to ECMWF (the magnitude of the bias is not quantified, but presumably it's large enough that the retrieved density is not believable) could mean that this component is absorbing a systematic error in the data. Fixing the Rayleigh to ECMWF could translate that error into other components, most likely aerosols since they are spectrally similar in extinction. In other words, retrieving an unrealistic to- tal density might make the other species more stable. Has this been considered, or quantified?
Author response: An aerosol model that is not perfect (this is always the case ) causes a systematic model error in the retrieval of air. The simultaneous retrieval of two species that are spectrally similar results in random retrieval errors on both species that are anticorrelated. However, we fix air to ECMWF. Assuming that this ECMWF profile is of good quality, the aerosol retrieval will have a much smaller random error component; the systematic error will still be present in the aerosol retrieval since it is inherent in the imperfect aerosol model. But at least it is not propagated to the air retrieval. Retrieving air will result in systematic errors on air and aerosols. Furthermore random retrieval errors will be larger.

Section 11.3
This is the only section of the paper I find unsatisfying. The retrieval of O2 and H2O from these measurements is indeed quite complicated, as the authors point out, but the details presented here on how this algorithm works in practice seem simplistic and vague. The effective transmission is not only pressure-dependent (and I think the dependence is more than "slight", as stated) but temperature-dependent as well. It's hard to believe that you can capture this complicated dependence by using a few simple reference atmospheres. What is the nature of the "background" fit by the last two terms in Eq (44)? At these wavelengths you need to account for ozone, Rayleigh and aerosols in addition to the target gasses. Presumably the ozone is constrained from the UV/visible retrieval, and the ECMWF data is used for Rayleigh, but this should be mentioned. So I assume that this background term accounts for aerosols only? Finally, I think it would be useful to the reader to summarize the accuracy of these retrievals at a top level – can GOMOS indeed retrieve O2 and H2O profiles that are scientifically useful?

Author response: We agree with this criticism and in the final version of the paper we will enhance the description of the IR retrieval including the comments of the referee.

## Minor comments and corrections:

Section 4, sentence 2 – This sentence reads awkwardly. Suggest transposing a few words, e.g., "This is especially important for limb viewing instruments where the accu- racy of the vertical geolocation directly affects the accuracy of vertical profiles."
Author response: Corrected.

Section 4, page 10156, line 8 – The text should say "Table 1 provides. . ." rather than "The following table provides. . ." so that the reader knows exactly what table you're talking about.
Author response: Corrected.

Figures 13 – 15 lack a label and units on the ordinate axis.
Author response: Corrected.

Section 6, page 10163, line 26 – I am not sure what this sentence means. The word "punctual" seems out of place here. Can you please make it clearer?
Author response: Corrected.

Figure 20 – the y-axis should be labeled. Are these absolute or relative (%) differences?
Author response: Corrected. Absolute. Now mentioned.

Figure 22 – the units for the red lines (vertical profiles) should be cm-3, not cm-2 as stated.
Author response: Corrected.

Figure 23, left panel – the caption should specify the wavelength associated with this aerosol extinction. Also, there is no scale on the x-axis.
Author response: Corrected.

# Anonymous Referee #2

"Especially important this is for ..." Should be "This is especially important for . . ."
Author response: Corrected.

"Figure 6 shows the tangent altitude change in a short occultations" – no "s" on occultations
Author response: Corrected.

"The following table provides" – Do you mean Table 1?
Author response: Yes. Corrected.

"Almost all scientific work are using only dark limb measurements and the dark limb is often defined by requiring the solar zenith angle to be larger than 107 deg (or 108deg.)." - Why does this value vary?
Author response (same as for referee 1): Different authors have implemented different solar zenith angle limits. The illumination flag table (Table 1) gives a dark limb limit of 110 deg and in addition requires 120 deg. solar zenith angle at the Envisat position in order to control straylight. Because these strict limits restrict heavily the amount of available data, more relaxed limits are usually adopted and no evident corruption of results has been found. Therefore, we are inclined to consider the limits in the illumination table limits somewhat too cautious. The investigation of new limits is certainly an important but difficult task. Some elaboration about this topic has been added to text.

"New values in ADU (Analog to Digital Unit) are interpolated from neighbouring pixels by a median filter but they are not used in the further processing." Why bother to interpolate if you don't use the data?

Author response: The only reason is to make plotting easier!

Figure 11 – Is it reasonable to conclude from this figure that all of the CCD pixels are now "hot", and so that there is no reason to expect any further increase in Dark charge? How does this CCD compare with other CCD columns?
Author response: Unfortunately no. The fact that a pixel is "hot" does not prevent increase in dark charge. There is no sign that dark charge is "saturating" and this dark charge increase is the main cause of quality degradation.

Figure 12 – Why does this figure only go through 2006, while Figure 11 goes through 2008? Does this curve level off after 2006 as does Figure 11?
Author response: We have now extended this figure up to 2008.

"There are an O2 emission around 760 nm and OH emissions near 940 nm." Grammar.
Author response: Corrected.

"overflow is assumed to be only punctual." I'm not quite sure what this means, but punctual is certainly not the right word here.
Author response: Corrected.

Figure 16 - "The figure shows the time delay effect explained in Fig. 3." I can't really figure out which curves are following which. What magnitude of time delay is expected here?
Author response: The red light from a given structure is detected first when using setting occultations. We can tell that the vertical separation "chromatic lag" varies (≈ exponentially) from 1 m at 45 km to 10 m at 30 km to 60 m at 20 km and 300 m at 10 km The time delay is obtained by dividing by the vertical velocity of Envisat (less than 3.4 km/s). The time delay is then from 3 ms at 30 km and 88 ms at 10 km.

Equation 13 - "At present, delta-SL is set to zero because the stray light correction is not activated." – This does not seem like a good idea. If there is no correction for stray light, there would seem to be all the more reason to include an error estimate for it.
Author response: We do not have enough information about stray light to subtract it. Therefore, we try to use only measurement locations where stray light can be guaranteed to be absent. The illumination table provides limits for the solar zenith angle at the tangent point and at the Envisat position for stray light free occultations.

"This means that we cannot simply use the cross sections as such in the inversion but we must take the instrumental resolution into account." – Is this a confusing way of stating that the forward model must be solved for at several frequencies and then averaged to match the spectrometer? If not, please explain further.
Author response: The convolution of the forward model with the instrumental function is the right way to simulate signal. In the GOMOS retrieval this has not been done for two reasons. First, the convolution would require the knowledge of the stellar spectra at a spectral resolution better than GOMOS can provide (because the same instrumental function is used to measure the stellar reference spectra). The second reason is the high computational requirement that was impossible to fulfill in mid 90's when the basic principles of the GOMOS retrieval were established. Therefore, the instrumental convolution was approximated by convoluting only the transmission function and even in that the convolution was directly applied to cross sections (instead of transmission). A second order term including the convolution of cross sections products (see Eq. (37)) was included as a term improving this approximation.

“Once the density profile is obtained the temperature profile is computed using the hydrostatic equation.” – But this is not the temperature used in the standard retrievals? Or is it? Please be explicit here.
Author response: No, this is different temperature, the high resolution temperature. Text has been improved.

“After the first vertical inversion the effective cross section becomes along the LOS averaged cross section with the local constituent densities as weights.” The grammar here needs some work.
Author response: Corrected.

Equation 41 – So, how many coefficients are actually used in the current retrievals?
Author response: In the current UV-visible retrieval we have the following unknowns: O3, NO2, NO3 column densities and three aerosol extinction coefficients. Together 6 unknowns.

Why does the scintillation only affect the 30 km data in Figure 20?
Author response: The scintillation modification exceeds noise below the 50 km tangent altitude. Below 20 km scintillations get more complex because they are produced by strong scintillation phenomena. Ensuing modifications are more random and their magnitude is smaller. This is why the 12 km curve in Fig. 20 is smoother than the 30 km curve. We have elucidated these aspects a little be more in the text.

Equation 44 – Isn’t there already a 1/lambda term from the aerosols? Why is there another one here?
Author response: Equation (44) is used only for the IR-channels. In the UV-visible wavelengths we use the second order polynomial model. The overall description of the IR retrieval will be improved in the final version of the paper.

Equation 46 – It seems very odd not to use a logarithmic interpolation in density here.
Author response: The reason is that in some cases the GOMOS retrieval produces negative results for physical quantities (because of excessive noise in data) and logarithm does not like negative numbers. Notice also that the interpolation concerns only values between adjacent measurements whose maximal distance is only 1.6 km or smaller.

“The ECMWF neutral density has been eliminated from data as an 100% known contributor.” –What does this sentence mean?
Author response: In the UV-visible spectral problem we assume that the Rayleigh extinction is calculated using the ECMWF and MSIS90 neutral density data. The transmission due to Rayleigh extinction is then removed from the observed transmission before the spectral inversion. Originally the neutral density was one of the unknown variables like ozone.