Dear Reviewer,

Thank you very much for your attention to our paper acp-2009-277 "Influence of scintillation on quality of ozone monitoring by GOMOS". We greatly acknowledge comments and suggestions. Below we present the detailed replies to each comment.

General comments

Reviewer #2

The introduction should define anisotropic and isotropic oscillations before the terms are used.

Authors

We have added one paragraph in the Introduction that describes the structure of air density irregularities in the stratosphere and the scintillations generated by these irregularities.

Reviewer #2

One paragraph about the GOMOS instrument and the EnviSat satellite is necessary in the introduction to supply details relevant to this paper (e.g. orbital altitude). It should be stated that the FOV of the photometer and the spectrometer are exactly the same. A reference to Kyrola et al. (Adv. Space. Res. 2004) would be useful since the optical layout is illustrated there.

Authors

We have reorganized Introduction and Section 2. The Introduction contains now a short description of the GOMOS mission, the instruments, the measurement principle etc. In the revised version, the estimates of the magnitude of fluctuations caused by scintillation (Fig.1) are moved to Section 2. The reference of the paper by Kyrölä et al (2004) is added.

Reviewer #2

Consider hypothetically, if there were no wavelength dependence to the scintillation. Then, shouldn't a DOAS spectral fitting approach be unaffected?

Authors

Yes, DOAS is less affected by scintillations, but this method cannot work for all species that are retrieved from GOMOS UV-VIS spectrometer measurements. In particular, in the current GOMOS operational processing, a kind of DOAS method (so called Global DOAS Iterative method) is applied for NO_2 and NO_3 after the spectral inversion. The details of this method can be found in (Hauchecorne et al., 2005, JGR), but they are not relevant to our paper.

Reviewer #2

Someone who speaks English as a first language should have edited this paper.

Authors

The English of the paper has been checked by a native English speaker.

Specific comments

Reviewer #2

The use of "altitude" where "tangent altitude" is intended should be corrected (including in the Figures (e.g. Figure 10a).

Authors

As explained in our paper, the altitude grid for GOMOS measurements and retrievals corresponds to the tangent altitudes. In the revised version, we note this explicitly (P.4, lines 17-18 of the manuscript with the tracked changes).

Reviewer#2

p12617, L23 (& Fig 1B) Is the trend in intensity vs. time) removed before the rms is calculated? Over which time period is the rms statistic calculated?

Authors

Yes, the trend is removed in the calculations of rms. In the revised version, we write explicitly that we consider relative fluctuations of intensity $\frac{I-\langle I\rangle}{\langle I\rangle}$ (in the caption of Fig.1).

For computing rms, 3 km samples with 50 % overlapping are used for the photometer signal and 6 km samples with 66% (2/3) overlapping are used for the spectrometer signals. This information is added in the caption of Fig.1.

Reviewer#2

p12620,Eq.3 Is there a reference for this equation? Some background is needed otherwise.

Authors

The reference (Dalaudier et al., 2001) is added.

Reviewer#2

p12620,L15 A reference for "Hanning filter" is suggested.

Authors

The reference is added.

Reviewer#2

p12620,L16 The use of a 3 km FWHM should be justified as also mentioned by the other reviewer. Why not use a boxcar with a FW of 0.5 s, equal to the spectrometer integration time?

Authors

The clarification is added, and it was also requested by the Reviewer #1 (see also the reply to the Reviewer #1). Related to your question, we would like to note that the photometer signal averaged down to 0.5 s still contains fluctuations caused by scintillation. It is discussed in details in our paper and illustrated in Fig.1. Similar

fluctuations in the spectrometer channels are intended to be removed. Since the altitude grid becomes denser at lower altitudes due to refraction, the width (in time) of the Hanning window needed for obtaining a smooth signal (i.e. without fluctuations due to scintillation) is not constant.

Reviewer#2
p12622,L14 Re: "absorption effect included", is scattering ignored? If not, you could use the term "extinction"

Authors
Scattering is also included. The text is corrected.

Reviewer#2
p12622,L19 As also pointed out by the other reviewer, the quality of the residuals after scintillation correction is exaggerated. I suggest "2% for altitudes above ~20 km." and a removal of "in the main ozone layer".

Authors
In our statement, we characterized fluctuations by their rms (which less than the amplitude of scintillation modulation), and the rms is below 1% above ~20 km. To avoid ambiguity, we indicated this explicitly in the revised version. According to your suggestion, we removed the words "in the main ozone layer".

Reviewer#2
p12622,L22 Re: Monte Carlo simulations, some details of the method are required (so that I could do something similar). Why are different results obtained for different runs? In other words, which inputs are random (i.e. varied)?

Authors
As stated in p12622 L.23 of our paper, the Monte Carlo runs are for different realizations of scintillations.

Reviewer#2
p12624,L3 Re: Fresnel scale, you find 0.45-0.6 m, but using the following inputs: lambda1=500 nm lambda2=600 nm z_sat=800 km tangent height= 20 km Earth radius= 6378 km L=3254 km I find the Fresnel scale is ~1.6 m.

Authors
The values you are using, being inserted into our formula for the Fresnel scale $\rho_F \approx \left(\frac{\sqrt{\lambda_1 \lambda_2} L}{2\pi} \right)^{1/2}$, will give the value 0.53 m, as stated correctly in our paper.

Reviewer#2
p12626,L19: Is this equation empirically derived? If so, this should be stated, otherwise provide a reference.

Authors
Yes, this equation was empirically derived. We clarified this in the revised version.

Reviewer#2
p12627, Eq.14: 'p' should be replaced by 'ch' for the vertical chromatic shift, since p is used to represent impact parameter, and since 'ch' is used in Figure 4c.

Authors
Δp was replaced by Δ_{ch}^{ph}.

Reviewer#2
p12628,L18 Why is there a 'noise' component for a simulated quantity?

Authors
It is not "a simulated quantity". Eq. (16) is the GOMOS spectral inversion, where $\hat{\boldsymbol{T}}_{ext}$ are measured transmittances after the dilution-scintillation correction. We remind this in the revised version.

Reviewer#2
p12630,L14 "sampling resolution" -> "vertical sampling"
p12630,L16 "...removing..." -> "...reducing..."

Authors
The text is corrected

Reviewer#2
Figure 4: (caption) "phase screen" is either not defined correctly or not the appropriate term. Given its definition "plane perpendicular to the light rays...", there should be no difference for oblique and non-oblique occultations. Did you mean: #1) the plane orthogonal to the satellite veolcity vector, or #2) the orbital plane?

Authors
We have used the commonly used definition of the phase screen. The phase screen approximation is widely used (see e.g. the monograph by Ishimaru "Wave propagation and scattering in random media", Vol. 2, or papers (Dalaudier et al., 2001; Gurvich and Brekhovskikh, 2001)). In the framework of the phase-screen approximation, the effect of the extended atmosphere is replaced by a plane screen that produces the same phase modulation of propagated light waves. This plane is perpendicular to incident light rays and it contains a tangent point (or, equivalently, it contains the Earth center).
The colored lines in Fig.4 represent trajectories of the intersection points of light rays and the phase screen.

In the revised version, we added the details and gave the abovementioned references.

References:
Ishimaru, A. Wave propagation and scattering in random media. V. 2. Multiple scattering, turbulence, rough surfaces and remote sensing (Academic Press, 1978).

Dalaudier F., V.Kan, and A.S.Gurvich, Chromatic refraction with global ozone monitoring by occultation of stars. I. Description and scintillation correction, *Applied Opt*., 40, 866-877, 2001

Gurvich A.S. and V.L. Brekhovskikh, Study of the turbulence and inner waves in the stratosphere based on the observations of stellar scintillations from space: a model of scintillation spectra, *Waves in Random Media*, 11, 163-181, 2001.

I would suggest that the arrows be removed from panels A-B. In B, the diagonal lines should terminate on the black line (if you meant #2).

Authors

Fig. 4 represents correctly the physics of the process. The arrows indicate the motion direction of the intersection points of ray trajectories in the phase screen.

Reviewer#2

Figure 5: What is the residual at 515 nm, shown to the right but inconspicuous on the color plots (left)?

Authors

The features at ~511.7 nm are seen in both left and right panels (in the electronic version of the figure, it is evident). These spectral features are discussed in (Fussen et al., 2005: GOMOS serendipitous data products: The mesospheric sodium layer and various limb emissions. *Adv. Space Res.*, 36, pp. 967- 972, doi:10.1016/j.asr.2005.03.009), they are not relevant to scintillations discussed in our paper.

Some 'massaging' of data appears to have been done, also since the feature at 630 nm appears in the lower color plot, but strangely not the upper one?

Authors

We have noticed that the horizontal scale in the upper left panel covers the wavelengths only up to 625 nm, while the wavelength range in other plots is extended up to 675 nm. Therefore, the oxygen absorption features at 629 nm were not observed in the upper left panel of the previous version of the figure (these features are also not related to the discussed scintillation effect). This accidental inconsistence was removed in the revised version. Thank you for pointing this out.
We confirm that no "massaging" was applied to the data.

Technical comments:

Authors

Thank you very much for the technical corrections. Nearly all the suggestions are accepted and introduced in the revised version (they are not marked). A few exceptions are:

Reviewer#2

p12617,L5 "Almost exponential..." -> "The exponential..."

Authors: The decrease of air density with altitude is not "the exponential", but it is "a

nearly exponential". We replaced "almost" with "a nearly".

Reviewer#2:
p12617,L15 "...different kinds of ... instabilities." -> "...and other kinds of instabilities."

Authors: Gravity waves are not instabilities…

Reviewer#2
p12618,L25 "In our estimates..." -> "In our correction..."

Authors: We replaced "in our estimates" by "in our analyses".

Please find the file with indicated changes in the manuscript.

As a corresponding author, I confirm that all co-authors concur with the submission in its revised form.

Yours sincerely,

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