R E P L Y

to the Referee Comments for manuscript: acp–2013–446 by N. M. Gavrilov "Turbulent diffusivities and energy dissipation rates in the stratosphere from GOMOS satellite stellar scintillation measurements".

Type: Research Article.
Referee: Dalaudier Francis
(francis.dalaudier@latmos.ipsl.fr)
The referee is a member of the
ESA "GOMOS Quality Working Group".

First, we would like to thank Dr. Francis Dalaudier for his useful comments helping to improve our paper. Answers to his comments we give in italic font placed inside the original review.

Content of the manuscript
The submitted manuscript uses a (small) portion of the database of spectral parameters estimated from GOMOS stellar scintillation measurements. This database contains estimations of 4 spectral parameters (2 vertical wavenumbers ko, kw and 2 spectral levels Cw, Ck) for specific location, altitude range and time corresponding to selected GOMOS observations, along with parameters (from ECMWF) characterizing the atmosphere. The author further processes this database in order to obtain derived parameters characterizing the turbulence and its effects (scales, dissipation rates, Eddy diffusivity). The formulas used in order to derive these parameters come from published theoretical works, models and experimental studies. Since the altitude range where these parameters are derived is difficult to observe, and is consequently poorly known, the estimation of these turbulent parameters is a scientifically important contribution. The values obtained by the author are in satisfying agreement with previous published estimations.

General comments on the manuscript
The general objective of the manuscript is important and a publication properly covering these objectives would be valuable. However, in its present state, the submitted manuscript contains many weaknesses (see specific comments below) that should be corrected before a possible publication. The exact nature and physical signification of the spectral parameters (from the database) as well as the hypotheses necessary for their estimation should be discussed carefully. The coverage (geographic, time, altitude, number of estimations) and the resolution of the used database needs to be detailed as well as associated uncertainties (which are included in the database). The parameters calculation needs to cover the full database (all latitudes) and at least one year of measurements in order to cover seasonal and latitudinal variations.

Please consider the paper as it is. I do not belong to the "GOMOS community" and have no direct access to the GOMOS database. Therefore, I will have no possibilities to extend this data analysis in the nearest future. I agree with the importance of studying seasonal variations and climatology of turbulent parameters at all altitudes from the GOMOS database. However, to do so, we first need to be convinced in our hypotheses, formulas and results. Therefore, as a first step, we obtained formulas for estimation of turbulent Thorpe scale, energy dissipation rates and diffusivities using parameters of anisotropic and isotropic spectra of atmospheric perturbations. Then we selected a portion of the most reliable data of the GOMOS database and estimated mentioned characteristics.. We obtained new information about turbulent characteristics in the stratosphere and made detailed descriptions of our approach allowing everyone from scientific community to understand and check our results. In the future, we are going to make climatological analysis of turbulence, if scientific community accepts our ideas and we will get appropriate funding for these studies.

The hypotheses underlying the various formulas used in order to derive the turbulence parameters need to be discussed carefully. This is particularly important since some of these hypotheses are different (and sometimes contradictory) from the ones used in order to estimate the original spectral parameters. The exact nature and physical signification of each derived turbulent parameter need to be better discussed. Some confusion about the various derived length scales need to be clarified. Conversely, some paragraphs describing already published technical details should be removed from the manuscript and replaced by appropriate references.

Careful discussion of underlying hypotheses and formulas is one of our main goals. Therefore, we thank the reviewer again for his useful critical comments helping to improve our paper.

Specific comments

- Because of the large spatial and temporal variability of the turbulent parameters (including length scales), covering sometimes more than one decade (your figure 5), the statistical distribution (like in your Figure 2) of a parameter is more informative than its average and its standard dispersion (as given in table 1). This is especially true for parameters with strongly non-Gaussian distribution, as clearly visible on your Figure 2.

I agree with this statement. In the paper, we gave both statistical distributions and average characteristics. We also emphasized the importance of distribution analysis in the discussion of Figures 2, 5 and Table 1 in the revised version of the paper, which we are preparing for submission after public discussion of the paper.

- The physical signification of the parameters used in this study (from the GOMOS database) as well as of the calculated parameters need to be discussed. Specifically, all the 2 parameters in the database are "effective" parameters obtained by spectral fitting. This means that the model assumes that the whole atmosphere is filled with homogeneous turbulence with constant parameters. This is obviously not a physically realistic hypothesis and the obtained values cannot be assumed to represent accurately the "average" value of the parameter.

I do not understand exactly which part of the paper is discussed (unfortunately the reviewer does not specified page and lines numbers). However, we agree that we obtain the “average” values of “effective” parameters and added this statement throughout the revised text. Signification of obtained parameters are confirmed by their agreement with in-situ measurements of turbulence in the stratosphere.

- The original database needs to be fully described. In this database, all latitudes and all seasons are sampled for more than one year and with the 30 brightest stars occultations. The part that was used for the present manuscript uses only 2 stars (and 60 days of data) in order to cover the "low latitudes" (-20°, +20°) and only one star (and 25 days of data) in order to cover the +34° +36° latitude band. Such coverage is clearly insufficient in order to be representative of "the stratosphere" (as stated in the title) particularly when the data exists and is available.

I used only part of the GOMOS database with the most reliable data for the most bright stars. I afraid, it would be not appropriate for me to describe full details of the database, which I never see as a whole. The GOMOS database is fully described in numerous publications, which are cited in our paper. ***We extended description of the part of GOMOS database used in the present study.***

The title of the paper just specifies that measurements are performed in the stratosphere. Careful study of full turbulent climatology from complete GOMOS *database is an extensive job. Recently, we found possibilities to develop algorithms and apply them to an analysis of only part of most reliable data. We assume, this analysis gave a new important data about stratospheric turbulence and it is enough to make conclusions about reliability and quality of obtained estimations. If these results are acceptable, we will search possibilities and funding for more extensive analysis of the GOMOS database.*

- The original database contains an uncertainty for each estimated parameter. This uncertainty is not used (and not discussed) in the present manuscript. When average values are calculated, weights from uncertainties need to be taken into account.

Because of complicated nonlinear dependences of turbulent parameters, it is difficult to estimate exact weights caused by uncertainties. From the GOMOS database we selected only most reliable data for two most bright stars. The uncertainties for these data are minimal. We assume that the weights for all these data are almost equal. This is described in the revised text of the paper. We checked our criteria once more during preparation of the revised version of the paper.

- The statement about "*Standard deviations of the mean values*" P18015L22 is incorrect because the distribution of the considered parameters is not Gaussian (see Fig. 2).

The phrase is removed.

- The wavenumber kw P18011L2 is associated with the (small scales) "decay" of the anisotropic contribution. This decay can result from the transition to isotropy for small scales or from a slope change of the 3---D anisotropic spectrum (or both). The relation between such wavenumber and the turbulence properties need to be discussed.

We changed the description on P18011L2 and added discussion about relations kw and turbulence.

- The relations between the various scales that are introduced need to be discussed : The transition scale (associated with kt) which is usually known as the "buoyancy" scale (Lumley), the Thorpe scale, the Ozmidov scale. The Fukao reference P18014L07 refer itself to Hocking 1987 who considers measurements in the 80---120 km altitude range.

Our results show that direct comparisons of mentioned scales seem to be questionable. The wavenumber k_t is just mathematical coordinate in Fourier transform domain, where anisotropic vertical spectrum is equal to isotropic one. The Thorp scale, L_T, is more physical quantity and reflect effective vertical displacements of fluid particles, which corresponds to effective thicknesses of turbulent layers of temperature perturbations in stable stratosphere. Why the scale $L_{kt} = 2\pi/k_t$ should be equal to L_T? Many scientists, who assumed this, have contradictions and confusions in comparisons with experiments, where frequently $L_{kt} >> L_T$. To solve the problem, we obtained mathematical formula (12) relating kt and L_T. This formula shows that at high Brunt-Vaisala frequencies $L_{kt} >> L_T$ even within hypotheses of locally isotropic turbulence. We consider this formula as a new, central and most important theoretical result of the paper. This formula gives better understanding relations between different turbulent scales and is very promising for different applications besides GOMOS data analysis. We added discussion about this and reference on the Hocking's paper.

- The equation (10) in Fukao et al. 1994 used to define "beta" is followed by the sentence "It must be noted that (10) gives a local value of K for (locally) homogeneous turbulence (inside a layered or patchy region) and that different formulations of K may be more appropriate for different spatial and/or temporal scales (e.g., larger than those of layered structures)." Questioning again the exact physical meaning of the calculated parameters.

I agree that formulae and coefficients used in the present study, as well as the formulae for anisotropic and isotropic spectra estimated from GOMOS measurements are valid for the model of locally homogeneous and isotropic turbulence. We used these formulae and coefficients for realistic atmospheric turbulence. If atmospheric turbulence differ from locally homogeneous and isotropic conditions, both estimations of spectral scales from GOMOS data and our estimations of turbulent parameters give not real, but "effective" values. These values may serve as indicators of overall activity of turbulent energy dissipation rates, diffusivities and scales of turbulence. In this sense, assumptions of the present paper are consistent with assumptions for anisotropic and isotropic GOMOS spectra. We added respective discussion into the revised paper.

- The material in section 2 and 3.1 (and part of section 3.2) is mostly a duplication of already published equations and derivations. It is sufficient to give appropriate credit and references. The duplication of the derivations is not useful.

Please keep in mind two circumstances. First, formulae in mentioned sections are not duplications. I derive the formulae myself. There are many similar formulae in the literature, but I did not find the ones I need. The reviewer also does not give any exact references, where the formulae are published. Second, mentioned sections are already published online in the present paper. Anybody can copy this paper. If I remove mentioned sections, anybody can use the formulae as their own without references to my paper. In any case, I have to keep initial and final formulae. The journal can save about 1 kilobyte of computer storage removing intermediate formulae, but I afraid, many readers will not understand where the final formulae come from and what underlying suggestions are.

- For the spectral parameters of the original database, the geographical sampling, the vertical resolution and the horizontal extent of the sampled volume must be given. The origin of the associated atmospheric parameters must also be given (ECMWF).

We added this information.

- The data in Gurvich and Chunchuzov (2003) and in Gurvich and Kan (2003b) is exactly the same (cited P18018L23). This information was confirmed by Valery Kan.

The data, probably, the same, but co-authors are different. Therefore, I prefer to keep both references.

- The correlation between parameters, which is discussed in section 3.3, can only result from the formulas used in order to calculate these parameters. This section is more related to the properties of the used models (sensibility study) than to the properties of the atmosphere itself. Of course, if the atmosphere behave exactly as predicted by the models (what I do not believe), this study would be relevant. This important restriction about the meaning of the obtained results need to be discussed.

I cannot completely agree that correlations between parameters are only caused by the statistical analysis. This may be more relevant to Cw, kw and k0, which are parameters of the same function and are estimated simultaneously. But Ck belongs to different function and is estimated separately. In addition, there are physical backgrounds for relations between gravity wave and turbulent spectra in the atmosphere. We added a statement about possible restrictions to the revised manuscript.

- When referring to large books (Tatarskii, Monin & Yaglom, Press at al.) please give the section and/or the equation number.

We added required references.

- Within the submitted manuscript, 35 sentences out of 145 (24%) contains the modal verb "may". This gives the strange feeling that the author himself is not really convinced by what is written.

*In my previous publications I did not use the verb "may" at all. Reviewers criticized that I am too much convinced in my results. In the revised version, I decreased the amount from 35 to 9 (6%). Probably, this amount **may be** acceptable.*

Technical corrections

- Please clarify the title and abstract since GOMOS is not a satellite, it is one of the instruments on board the ENVISAT satellite.

The title and abstract are corrected.

- P18008L17 "... IGWs propagating upwards ..." phase or energy propagation ?

It is clarified that "energy propagation"

- P18009L20 stations Salute ---> stations Salyut

Corrected

- P18009L17 optical scintillation method

Corrected

- P18009L18 "higher sampling rates" please specify

Corrected

- P18009L26 reference to Bertaux et al. 2010 is more appropriate.

Reference is replaced.

- P18011L01 "constant parameters" is ambiguous since these parameters are fitted for each spectrum.

The word "constant" is removed.

- P18011L09 write (4π/3) like for equation (3)

Corrected

- P18011L14 "kk is scale ..." No kk is a wavenumber (the same error is present in Sofieva et al. 2007)

Changed

- P18011L14 "smallest isotropic perturbations" is inaccurate since viscous domain is isotropic.

Removed.

- P18011L17 kz << kk (wavenumber is not scale)

Corrected

- P18012L21 one---dimension ---> vertical

Replaced

- P18012L21 in Lumley 1964 paper (an in many following ones) kt is named kb for "buoyancy"

We replaced kt to kb everywhere in the text.

- P18013L07 edging ---> transition

Replaced

- P18016L01 Differences ---> Dispersion (also at other places in the manuscript)

Replaced

- P18017L26 fitting : in linear or logarithmic coordinates?

Coordinates (logarithmic) are specified.

- P18018L15 Ck values can also be compared with Sofieva et al. 2007 and with Gurvich et al. 2007. Gurvich, A. S., V. F. Sofieva, and F. Dalaudier (2007), Global distribution of CT2 at altitudes 30–50 km from space---borne observations of stellar scintillation, Geophys. Res. Lett., 34, L24813, doi:10.1029/2007GL031134.

J. L. Bertaux, E. Kyrola, D. Fussen, A. Hauchecorne, F. Dalaudier, V. Sofieva, J. Tamminen, F. Vanhellemont, O. Fanton d'Andon, G. Barrot, A. Mangin, L. Blanot, J. C. Lebrun, K. Perot, T. Fehr, L. Saavedra, G. W. Leppelmeier, and R. Fraisse (2010) Global ozone monitoring by occultation of stars: an overview of GOMOS measurements on ENVISAT Atmos. Chem. Phys., 10, 12091---12148 Within the ACP special issue on Gomos: http://www.atmos-- chem---phys.net/special_issue153.html

Gurvich, A. S., V. F. Sofieva, and F. Dalaudier (2007), Global distribution of CT2 at altitudes 30–50 km from space---borne observations of stellar scintillation, Geophys. Res. Lett., 34, L24813, doi:10.1029/2007GL031134.

Comparisons and references are added

Yours sincerely. Nikolai M. Gavrilov