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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "Turbulent diffusivities and energy dissipation rates in the stratosphere from GOMOS satellite stellar scintillation measurements" *by* N. M. Gavrilov

Anonymous Referee #2

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General Comment

In this paper turbulence parameters are derived in the stratosphere from stellar scintillation data of the GOMOS instrument on Envisat. These results are important because estimation of turbulence parameters in the stratosphere is difficult, and observations are sparse. The paper is therefore of general interest for the readership of ACP.

Since the paper is not overly long I do not share the concerns of Francis Dalaudier that the derivation of equations should be removed (24 pages in discussions style will be only about 8 pages in two-column format). In addition, this more technical part may help the readers who are not experts in the topic.

One main shortcoming of the paper is the limited amount of data that is presented, as already mentioned by Francis Dalaudier in his review. Of course, the discussion of seasonal variations and a larger latitudinal coverage based on the full GOMOS data set would be highly interesting. Nevertheless, the paper provides useful information and can be considered a demonstration of the method. Therefore, I think that in spite of this shortcoming the paper is basically publishable in ACP.

There are however several concerns regarding details how the results are presented, three of them major:

(MC1) Given the fact that only a small amount of data is presented, you have to be more specific! The way the paper is written suggests that your results are representative for larger latitude ranges and whole years, which is not the case.

(MC2) You are mixing things! In several of your figures you combine data for the whole altitude range 30–45 km, ignoring the strong altitude dependence of the different parameters.

(MC3) On p.18019 the discussion on the interaction of IGW with turbulent spectra is very speculative. In particular, the statement that variations seen in Fig.4 would contradict the saturation of monochromatic IGWs is not well supported.

For details see also the detailed comments below.

Before publication in ACP these concerns have to be addressed.

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Interactive Discussion

Discussion Paper

Detailed Comments

(DC1) p.18008, l.13-14: Of course, the location of the continents will play a certain role, however the mentioned maxima of turbulence parameters should rather correspond to regions of strong wave dissipation. Probably these maxima are more directly correlated with high activity of convective gravity waves in the mentioned regions. In particular, the region 90-180E is mainly above ocean. Nevertheless, high activity of waves is observed there, which would correspond well to the observed enhancements of turbulence parameters at the same location. The other two regions of high energy dissipation rates that are mentioned in the abstract are more centered around 0E and 60-120W (see Fig.4), and not at 30–100W and 0–60E, as stated in the abstract.

Please correct the numbers and refer to wave dissipation instead of geography.

(DC2) p.18008, l.22: Please include also the more recent reference Alexander et al., QJRMS, 2010, which summarizes recent advances in modeling and observing gravity waves.

(DC3) p.18008, l.23ff: Here the introduction is somewhat out of balance. Some important older references are cited, and a large number of references for the GPS technique is given, but recent advances in deriving gravity wave momentum flux using infrared limb soundings are completely ignored. Momentum flux is more directly related to wave dissipation and onset of turbulence than temperature variances or potential energies that are discussed in the references that are only given. Please include, for example, the references Alexander et al., JGR, 2008 and Ern et al., JGR, 2004, 2011.

(DC4) p.18014, l.14: It should be mentioned that also c has a large error range. For example, Clayson and Kantha (2008) use a value of c^2=0.3 (c=0.55).

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(DC5) p.18015, l.9–13: How many soundings for a fixed altitude level, say 30km, are entering your analysis during September-November 2004, and how many for January 2005?

(DC6) p.18015, l.25 and everywhere else, please be more specific with the time ranges. Your data cover only September-November 2004 and January 2005!
General comment: the global distribution of gravity waves has strong seasonal variations. Consequently, one would expect that also turbulence parameters show similar seasonal variations. The assumption that the September-November average would be representative for the whole year 2004, or January 2005 for the whole year 2005, is therefore not valid. I agree with Francis Dalaudier that a longer time series of turbulence parameters would be very valuable. Possibly there is good correspondence between seasonal variations of energy dissipation rates, variations of turbulence parameters and seasonal variations of the distribution of gravity waves. This could however also be subject of a follow-up study.

(DC7) p.18015, l.28: at 30km altitude in the latitude interval 34N–36N for January
Similar problem as in the previous DC: Please, be more specific! I would not call 34–36deg representative for (the whole range of) middle latitudes!

(DC8) p.18016, l.8: This formulation is somewhat misleading!
The values in Table 1 are for January, and not representative for the whole year! Also a comparison of radiosonde estimates for the narrow range of longitudes 86W-104W with the zonal averages from GOMOS is possibly not very meaningful. There could be variations in the turbulence parameters with longitude. I understand that observations of these parameters are sparse, and you have to rely on the comparison with Clayson and Kantha (2008). The shortcomings of this comparison however (different spatial and temporal coverage) have to be more clearly stated.

(DC9) p.18018, l.12: for which latitudes, longitudes and seasons were the observations from the space stations carried out? Are these compatible with the spatial and temporal coverage of the GOMOS data considered here?

(DC10) p.18018, l.21-25: related to the previous DC: please be more specific about differences in spatial and temporal coverages.

(DC11) p.18019, l.7ff: Fig.3 shows ϵ and C_k averaged over the altitude range 30–45km. This does not really make sense! These parameters depend strongly on altitude (factor 10!).
I would suggest to do this comparison for a fixed altitude. The averages in Fig.3 are dominated by the altitudes where values are highest, anyhow, and do not represent the whole altitude range.

(DC12) p.18019, l.11: In the three references given here the mentioned maxima are only weakly indicated. They are much more pronounced in measurements of microwave (MLS) or infrared limb instruments. In addition, momentum flux is better suited for comparison with turbulent energy dissipation rates or turbulence parameters. According references should be added here, for example Jiang et al., JGR, 2004, Ern et al., JGR, 2011 and Ern and Preusse, GRL, 2012.

(DC13) p.18019, l.12-21: I think the statement that Fig.4 contradicts the dissipation of monochromatic IGWs is very speculative and not well supported.

Are the variations shown in Fig.4 robust enough to support this statement? There are probably large uncertainties!

How would the distribution of Kw have to look like if it were indicative for breaking of monochromatic waves? I suppose similar to the distributions of ϵ and C_k in Fig.3 or the distribution of gravity waves in several of the above mentioned studies.
However, you compare energy dissipation rates and C_k averaged over a large altitude range (30-45km) with horizontal distributions of Kw in 3km thin layers. I

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think this comparison is not fair! I have the impression that an average of Kw over the whole altitude range 30–45km would look very similar to ϵ and C_k in Fig.3.

To state this clearly: given the strong variation of Kw with altitude (factor 10!), I think that an average over this large altitude range does not make much sense (see also several other detailed comments).

Therefore:

Comparison between Kw and ϵ and C_k should be done for the same altitude.

Details in the differences between these distributions may not be very reliable. Conclusions about details of the wave dissipation mechanism are therefore very speculative. Either drop this point from the discussion, or mark this point more clearly as being very speculative.

(DC14) p.18019, l.17: Please be more specific! About which horizontal scales are you talking here? Give typical values for “long-wave IGWs” and “short scale” waves!

(DC15) p.18020, l.15: location of continents. → locations where usually strong activity of gravity waves is observed.

(DC16) Fig.2: The soundings from different altitudes are combined in the histograms. I think that this makes no sense!
As your number of observations is not very large it probably makes sense to combine all data of one altitude level and neglect spatial and temporal variations. This shortcoming has however to be clearly mentioned.
Different altitudes, however, should not be combined because the different parameters can vary by a factor of 10, depending on altitude. For example, the skewness of the distributions in Fig.2 could easily be a result of combining different altitudes.

(DC17) Fig.5: Same as Fig.2, but for the scatter plots.

Technical Comments

- p.18009, l.13: fluctuates (oscillates)
- p.18009, l.14: “amplitude of hundreds of percent” may be somewhat misleading, the expression “amplitude” is prevailingly used for oscillations
  suggestion: Relative intensity fluctuations can be as strong as several hundred ...
- p.18009, l.20: with Russian → with the Russian
- p.18009, l.22: also confirmed → and also confirmed
- p.18009, l.26: instruments → instrument
- p.18010, l.4: spectra → spectral
- p.18010, l.4: in years → in the years
- p.18010, l.10: at → onboard
- p.18010, l.11: pass → path
- p.18010, l.15: with three-dimensional spectral density → with the three-dimensional spectral density function
- p.18011, l.1: function → the function
- p.18011, l.2: one-dimensional → the one-dimensional
- p.18011, l.6: corresponds to → corresponds to the
- p.18011, l.7: symmetric → the symmetric

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- p.18011, l.11: braking → breaking
- p.18011, l.14: scale → the scale
- p.18011, l.15: Isotropic one-dimension → The isotropic one-dimensional
- p.18011, l.16: of locally → of the locally
- p.18011, l.19: power low → power law
- p.18011, l.20: of locally → of the locally
- p.18011, l.21: have → has
- p.18012, l.1: of → of the
- p.18012, l.7: Eqs. → in Eqs.
- p.18012, l.9: of anisotropic → of the anisotropic
- p.18012, l.10: with GOMOS device at the → with the GOMOS instrument on-board the
- p.18012, l.20/21: This sentence sounds odd, please rewrite. Suggestion: "Some theories ...(...) introduce the wavenumber k_t for the crossover between one-dimensional anisotropic (Eq.4) and isotropic (Eq.6) spectral regimes ..."
- p.18013, l.5: one-dimension → one-dimensional
- p.18013, l.6: of → of the
- p.18013, l.7: to → to the
- p.18013, l.11: Contribution → The contribution

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- p.18013, l.19: with → with the
- p.18015, l.7: were applied → was applied
- p.18015, l.14: we estimated the crossover wavenumber k_t between ...
- p.18015, l.20, l.25: turbulent characteristics → turbulence characteristics
- p.18015, l.28: methods → methods in the range
- p.18016, l.5: in the year
- p.18016, l.8, l.9: for year → for the year
- p.18016, ll.12-15: wavelength → wavenumber ??
- p.18016, l.24: show → shows
- p.18016, l.27: spectra → spectral
- p.18017, l.9: spectra → spectral
- p.18017, l.9: in year 2004 → for September-November 2004
- p.18018, l.6: radio sound → radiosonde
- p.18018, l.6: for year 2005 → for January 2005
- p.18018, l.14: in year 2004 → for September-November 2004
- p.18018, l.27f: This sentence sounds odd! Please rewrite!
  "This may explain the positive correlation between the parameters Cw and Ck in Table 2 and Fig.5."
- p.18019, l.5: shorter → shorter horizontal scale ??

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- p.18024, Table 1: unit of N^2 is s^{-2}
- p.18030: ... for pairs of the spectral parameters presented in ... for September–November 2004 ...

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 18007, 2013.

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