Atmospheric Measurement Techniques Discussions

Interactive comment on "Fine-scale turbulence soundings in the stratosphere with the new balloon-borne instrument LITOS" *by* A. Theuerkauf et al.

A. Theuerkauf et al.

theuerkauf@iap-kborn.de

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Reply to referee F. Dalaudier

We would like to thank the referee for the detailed and valuable comments.

In the submitted manuscript, important information is given so that the proposed technique appear as a valuable tool for the investigation of the turbulence (and more generally of the dynamics) of the stratosphere. Nothing is said about the troposphere

where this technique could also (potentially) be applied. Furthermore, I believe that the possible range of "dissipation rate" which can be measured with this new technique should be discussed in this manuscript, even if the instrument is obviously still subject to future improvements.
According to the comment of the referee, we add a sequence in the discussion section including the range of dissipation rates which can be measured with LITOS. Furthermore, we point out that the method can also be applied in the troposphere.

The section about experimental method should start with an overview of the method stating clearly from where the dissipation rates will be deduced. Such statement will allow to understand better why the technique needs very high resolution measurements.
For a better understanding we included following sentence at the beginning of the experimental method section: "As mentioned above the precise calculation of energy dissipation rates requires to resolve the inner scale, i.e. spatial scales of a few mm. This measurement resolution can only be achieved by in-situ soundings."

For instance the sensor is placed 20 cm above a (cubic ?) box with sides 35 cm (possibly stabilized with a vane). I am not convinced that the air-flow is not perturbed by the box, especially for the case of the Kiruna (BEXUS) flight where the device was apparently attached to a larger gondola.
We must admit that we have not explicitly described the difference between the gondola used for Kühlungsborn and the BEXUS gondola. For the BEXUS gondola we used a long tube (2 m long) attached to one edge of the gondola. The sensor was attached to the top of this tube. Since the gondola has a side length of 0.6 m, the sensor was located 1.4 m above the gondola. Thus the influence of the gondola was minimized. Our estimation is that also the sensor of the Kühlungsborn payload

is not influenced by the box, which in this case is stabilised with a vane. The cubic (styrofoam) box has rounded edges which reduces the occurrence of separation eddies caused by the atmospheric flow passing the box. Furthermore, since the box ascends, the layer with the separation eddies is even more reduced and pointed away from the sensor. We have included a more detailed description about the difference between the payload in Kiruna and Kühlungsborn in Section 2.

Section 2.1: The distance of 60 m for the case of the largest balloon is not sufficient to guarantee that part of the detected turbulence is not produced by the balloon wake. However, using the low resolution wind profile, it is possible to estimate, for each altitude, the horizontal distance between the gondola and the "center" of the wake.
Based on the paper Barat et al., 1984, the gondola will be outside the wake, if the distance between the gondola and the center of wake is $\geq$ 4 times the radius r of the balloon. The balloon in Kiruna has a volume of 10000 m^3, i.e. a radius of $\sim$ 20 m (assuming a spherical shape). The distance between the gondola and the wake was not below 80 m (i.e. $4 \times r$) and typically $>$ 150 m during the ascent phase (except the first 3 km after launch). Thus we conclude that the measurements in the gondola altitude are not influenced by the balloon wake. We have clarified this topic in the revised manuscript.

Furthermore, the problems associated with the pendulum motion of the gondola are not sufficiently discussed. On page 3460 line 3, there is a sentence "Ignoring pendulum motions for a moment", but the question is not discussed later. It is necessary to give the amplitudes (in m and in m/s) of this pendulum motion. I believe that this motion can produce relative speed larger than 2 m/s which takes into account only the wind shear.
The referee is right, that pendulum motion and rotation might affect the quality of

the measurements. We have calculated a period of 15 - 20 s for the pendulum oscillation of the different gondolas. Furthermore we studied the acceleration of the gondola which has been measured by another instrument on the same gondola. Their measurements reveal a typical pendulum velocity of 1.5 m/s (maximum 2 m/s) which affects the observation as a bias varying sinusoidal with a 15 - 20 s period. This bias is removed before spectral analysis. For further examinations of this possible bias a special developed housekeeping device measuring the rotation and pendulum motion of the gondola has been already included in the latest version of LITOS. We have included a sequence referring to this issue in Section 2.

When discussing CTA (section 2.2), the problems with the mounting of the hot-wire is not discussed. Is it possible to detect the measurements where the wire is "behind" its mounting with respect to the relative wind ? I believe that because of the pendulum and rotation motion of the gondola, this situation may be frequent. I also think that the measurements obtained during such periods should be discarded.
We performed laboratory measurements to investigate the probe response on changing sensor yaw and pitch orientation. Depending on the angle the variation in the evaluated velocities equaled about $15 - 20\,\%$ due to the yaw effect ($0 - 180°$) and $5\,\%$ due to the pitch effect ($\pm 10°$). Since we have stabilised our Kühlungsborn gondola with a wind vane, we can assume that the velocity is almost always directed normal to the wire axis. Thus, the effect of the yaw angle is considerably less the $15 - 20\,\%$. The BEXUS gondola is not stabilised, so slow rotation occurs. That pendulum and rotation would cause distinct peaks in the spectrum at lower frequencies ($\ll$ 1 Hz). The measured spectra show no significant signal at that frequency.

There should be a clear statement that CTA measures the modulus of the wind (not the components) perpendicular to the wire axis. Furthermore, I cannot believe

that the component of the wind along the wire does not contribute to the heat removal. Since this component is large (5 m/s) and NOT constant, there should be a discussion of its effect.

Laboratory measurements reveal, that at a pitch angle of $90°$ and with a ascend rate of 5 m/s the wire would see an effective wind of 1 m/s. Hence, the component along the wire has an effect of 20%. The ascending rate during the BEXUS flight was constant within in a range of $\pm 1\,m/s$. Consequently, we can assume a constant ascending rate within a data sequence we use for a single spectrum (4 - 20 s). By removing a spline trend from the signal, we eliminate the influence of the constant ascending rate. Thus it is well justified to assume that the component along the wire has no significant influence on the spectrum of relative voltage fluctuations.

Please write what instrument is used in order to measure the "true" speed of the wind in the wind tunnel and give an idea of its uncertainty.

We included the following sentences in the paper: The wind calibration unit uses the effect of laminar flow behind a nozzle. By measuring the pressure drop across the nozzle the flow velocity can be calculated. Possible uncertainties of the calculated flow velocity can be caused by temperature changes during the experiment and by the measurement accuracy of the pressure sensors. Hence, the temperature has been observed during the experiments and its effect is considered in the equation for the determination of the flow velocity. Thus the remaining uncertainty is only caused by the accuracy of the pressure sensors. This accuracy is specified with $\pm 1\%$ resulting in a maximum uncertainty of the flow velocity of ± 0.2 m/s.

The way you discuss "heat transfer coefficient" and "Nusselt number" which are only defined in the appendix is confusing.

To achieve a better understanding we changed the order of some sentence most

appropriate for our case.

Influence of humidity : it should be the mixing ratio ~ 5 ppm in the stratosphere which is important and not the relative (typo) humidity which is defined with respect to saturation partial pressure.

We agree with the referee and included the value of the mixing ratio instead of the relative humidity.

While the clear separation between turbulent and non turbulent layers is stated at various places, I am not sure that the situation in the atmosphere is so clear. While Figure 5 and 6 show data sample for turbulent and non-turbulent regions, the "sharp" transition is not shown. I expect that the analyzed data sections contains only one kind of situation. Is this always the case ? Furthermore, for the case of turbulent sections, it is difficult to ensure that the turbulence is sufficiently "homogeneous" (epsilon constant) for the whole section. Please clarify.

We have replaced the Fig. 5 and 6 with a Figure showing both turbulent and non-turbulent layers and the boundary in between. The presented spectra are taken from regions well within the turbulent and non-turbulent layers. In order to derive epsilon profiles (which are presented in a subsequent paper) we calculate spectra based on data sections of $\sim$ 40-100 m vertical extension. An objective algorithm differentiates between turbulent regions and others where no energy dissipation rates can be calculated. By this it is assured that only layers with sufficient vertical extension of turbulence are evaluated. Nevertheless, the referee is right that within the 40-100 m layers the turbulence is inhomogeneous and the spectra might contain contributions from stronger and weaker turbulence. This averaging contributes to the observational error even if this error is small due to the high vertical resolution. However, we would like to note here that the special requirements to derive whole profiles of energy

dissipation rates are outside the scope of this publication.

The sentence (P3462L14-16) : "From the spectral slope ... dissipation rate" is incorrect since epsilon is not deduced from the spectral slope but from the inner scale. It is true however that this is the scale where the slope change.

The sentence (P3462L16-17) "these numbers ... length scale" must be clarified. Which numbers, which length scale ?

For a better clarification, we changed the sentence to: "From the spectral slope of the observed fluctuations, we deduce information about the stratospheric turbulence. For instance by determining the inner scale, we derive the energy dissipation rate. It is important to note, that ε does not depend on absolute velocity values, but rather on the particular inner scale of the spectrum of relative fluctuations."

Successive statements at the end of section 3.1 appear contradictory : "linear dependence", "no influence", "the temperature influence has to be corrected". This part need to be clarified.

We regret the confusion. Similar to Figure 3 and 4 we have added figures showing the results of the climate chamber tests. The first figure shows the CTA voltage signals together with the King's law fits as a function of velocity. In contrast to the vacuum chamber results they reveal a linear relation between the CTA voltage signal and the temperature of the fluid.

Similarly, the statement P3464L4 "we demonstrate later in this paper" is not convincing in my opinion.

The influence of changing background winds on the sensitivity of the sensor and on the spectra is considered in the discussion. We have changed the mentioned sentence

accordingly.

At the end of section 3, the limitation for the use of CTA due to the transition from continuous to molecular flow is not the only condition to be fulfilled. I expect for instance that the radiative cooling of the wire that is presently neglected becomes more and more significant.

Based on Durst (2008) the radiative transfer is defined by:

$$\dot{Q}_{rad} = \sigma \; A(T_w^4 - T_a^4) \tag{1}$$

where σ is the Stefan-Boltzmann constant, A the heat-radiating surface of the sensor, T_w the wire temperature and T_a the temperature of the ambient fluid. For the BEXUS 6 launch, we have calculated a radiative heat loss between between $7.53\text{x}10^{-5}$ W and $7.95\text{x}10^{-5}$ W. The entire heat transferred from the sensor to the surrounding fluid is given by (Durst, 2008; Jørgensen, 2002):

$$\dot{Q}_E = I^2 \cdot R \tag{2}$$

where $I = U/(20 + R)$ is the current, R is the overheat resistance and U the output voltage. We retrieve a total heat transfer between 0.0108 W and 0.0061 W for the BEXUS 6 launch. Thus the heat transfer due to radiation amounts to max. 1.3% of the total heat transfer. Consequently it is well justified to neglect the effect of radiative cooling.

In section 4.1 the observation of turbulent layers is discussed. It is not sufficient to observe non-turbulent layers in order to guarantee that all the turbulent ones are from atmospheric origin. Please discuss what is the minimal thickness (data length) of such layers in order to apply the proposed spectral analysis method. How do you check that the observed layer is statistically homogeneous ? The data shown does not

allow to state "these results indicate that the turbulent regions are defined by sharp boundaries to the non-turbulent regions and therewith represent the layered structure of stratospheric turbulence".

We regret that we did not show a profile with both turbulent and non-turbulent regions. We replaced Fig. 5 and 6 by a new Figure showing a longer profile with both regions proving the mentioned statement. Typically spectra are calculated from 80 m sections of data. The turbulent layer has to fill a large part of this section to produce a clear spectral signal. Nevertheless it is not required that the section is completely filled with turbulence as the first and last part of the section are suppressed by a hanning function. The 80 m sections are shifted by about 50 m for the subsequent spectrum. For geophysical applications single "turbulent" spectra preceded and succeeded by spectra without turbulence which are exlcuded because of potentially erroneous data. As mentioned above the geophysical interpretation of epsilon-profiles is outside the scope of this technical paper. Inhomogeneous turbulence might result in some smearing of the spectra especially in the transition region of inertial and viscous subrange even for this high resolved observations. By this it might affect the determination of the inner scale and by this the energy dissipation rate. We discuss the potential error sources in more detail in the revised paper.

In section 4.2 the velocity used in order to convert from time frequency to spatial scale is NOT the balloon velocity but the TOTAL (relative) velocity, which includes the horizontal velocity (also from pendulum motion) and can thus be much larger. The balloon velocity would be appropriate for strongly anisotropic horizontal structures, but the inner scale which is used here is certainly fully isotropic. Furthermore, according to your equation 3, the value of epsilon varies as the fourth power (!!) of the transition scale. Consequently the value of the relative wind must be known accurately in order to reduce the error (bias) on the value of epsilon.

The referee is correct that by our method the calculation of epsilon depends strongly

on the accurate determination of the inner scale, and that the relative movement of the gondola has to be taken into account. For that reason we included an accelerometer in our payload. For the conversion of frequency scales into spatial scales the particular ascent rate is used. This rate is modulated by the relative horizontal wind by the root of the sum of squared velocities (vertical and relative horizontal). For the Kiruna flight we estimated an error of the inner scale by $\sim 10\,\%$, resulting in $30 - 50\,\%$ error in epsilon for ignoring the relative horizontal wind. We have added an estimation of potential error sources and strengths in the discussion section and made clear that this error can be reduced by careful analysis of the acceleration data.

About the slope -7, there is more than one theory about this dissipation range. You should state which one you are referring and discuss possible different behaviors. The experimental evidence for such slope on figure 7 is rather weak. However, your results does not depend strongly on the exact slope (in the dissipation range) and you should improve the discussion about the necessary hypothesis.

As the referee pointed out, for the determination of energy dissipation rates it is not necessary to obtain the viscous subrange of the turbulent spectrum. We present here first measurements of the turbulent spectrum covering the transition region to the viscous subrange in the stratosphere. Thus we can determine precise energy dissipation rates by applying the Heisenberg model. This model exhibits a m^{-7} power law in the viscous subrange. Nevertheless, we are aware of the open discussion about the true slope in the viscous subrange. But we feel it is beyond the scope of this paper to contribute to the discussion about the exact slope in the viscous subrange, since it is mainly focussed on the technical description of LITOS as it is the scope of the journal. However, according to the referee comment we included a more detailed description of the Heisenberg model and the respective slopes in the revised manuscript.

Is the (spectral) noise level of your instrument constant for all altitudes?
Yes, the spectral noise level is constant with altitude and has a value of $\sim 2\times 10^{-7}\,V^2/s$.

I suggest to combine the figures 7 and 8 into a single one (but keep different curve colors) which will better allow the comparison between the atmospheric spectrum and the instrumental noise.
We have combined Figure 7 and 8 to one figure (a and b) but kept them separate. The reason is that it would not be possible to distinguish between the different noise level, since one would cover the other spectrum. Thus it wouldn't be possible to compare the noise region of both spectra. Nevertheless for emphasizing the differences, we included the line of the $m^{-5/3}$ and m^{-7} slope in the figure of the non-turbulent spectrum.

In section 4.3, you refer to the (excellent) work of Lübken and collaborators. However, this work is about density spectra and cannot be applied directly to velocity spectra. This question must be properly discussed and the formula used for the velocity spectrum must be given with appropriate references.
The reviewer is correct that the cited work by Lübken should not be applied directly to velocity spectra. Therefore we revised this point and included the following new Appendix C where we derive the appropriate equation for velocity fluctuations in detail: For the calculation of the energy dissipation rate we have adopted the method of Lübken (1992) and Lübken et al. (1993) which was formulated for density fluctuations. Since we measure velocity fluctuations and not density fluctuations we have to modify certain equations. Basically the theoretical spectrum is derived from the Heisenberg model. This model exhibits an $m^{-5/3}$ power law in the inertial subrange and a smooth transition to the m^{-7} power law in the viscous subrange. The 1-dimensional frequency

spectrum is given by:

$$W(\omega) \;=\; \frac{\Gamma(5/3)\,sin(\pi/3)}{2\pi v_r}\,C_v^2\,\frac{(\omega/v_b)^{-5/3}}{\left\{1+((\omega/v_r)/k_0)^{8/3}\right\}^2} \tag{3}$$

where Γ is the Gamma function ($\Gamma(5/3) = 0.90167$), v_b is the balloon velocity and $\omega = 2\pi f$ is the cyclic frequency. C_v^2 is the structure function constant and for velocity fluctuations it is given by:

$$C_v^2 = 4\alpha\varepsilon^{2/3} \tag{4}$$

(e.g. Barat and Bertin, 1984; Bertin et al., 1997; Hocking, 1999). The empirical constant α is taken as 0.5 (Bertin et al., 1997, Antonia et al., 1981). The Heisenberg model "breaks" at k_0, which is the intersection of the asymptotic form of $W(\omega)$ in the inertial and viscous subrange. Tatarskii (1971) has shown that k_0 is determined from the behaviour of the structure function D at the origin and that the structure function is related to the 3-dimensional spectrum by:

$$\frac{d^2}{dr^2}D(0) \;=\; \frac{8\pi}{3}\int_0^\infty \Phi(k)k^4\,\mathrm{d}k \tag{5}$$

Since our measurement axis is perpendicular to the propagation direction, we use the transversal structure function for velocity fluctuations:

$$D(r) = \frac{2}{15}\frac{\varepsilon}{\nu}r^2 \tag{6}$$

(Tatarskii, 1971). Thus Eq. 5 forms to:

$$\frac{4}{15}\frac{\varepsilon}{\nu} \;=\; \frac{8\pi}{3}\int_0^\infty \Phi(k)k^4\,\mathrm{d}k \tag{7}$$

Assuming an isotropic "frozen" random field, the 1-dimensional spectrum can be converted to the 3-dimensional spectrum (Tatarskii, 1971; Lübken et al., 1993):

$$\Phi(k) = -\frac{v_b^2}{2\pi k}\cdot\frac{d}{d\omega}W(\omega) \tag{8}$$

Inserting Eq. 3 and Eq. 4 in Eq. 8 gives:

$$\Phi(k) = \frac{5 \, \Gamma(5/3) sin(\pi/3) \cdot \varepsilon^{2/3}}{6\pi^2} \cdot k^{-\frac{11}{3}} \cdot \frac{1 + \frac{21}{5}(k/k_0)^{\frac{8}{3}}}{\left\{1 + (k/k_0)^{\frac{8}{3}}\right\}^3} \tag{9}$$

Combining Eq. 9 and Eq. 7 and rearranging for k_0 finally leads to the inner scale $l_0 = 2\pi/k_0$:

$$l_0 = 2\pi \cdot \left(\frac{45 \, \Gamma(5/3) \, sin(\pi/3)}{8}\right)^{\frac{3}{4}} \left(\frac{\nu^3}{\varepsilon}\right)^{\frac{1}{4}} = 11.35 \cdot \left(\frac{\nu^3}{\varepsilon}\right)^{\frac{1}{4}} \tag{10}$$

where $\eta = (\nu^3/\varepsilon)^{1/4}$ is the Kolmogorov microscale. Thus we also recalculate the energy dissipation rate for the shown example spectrum and obtain an energy dissipation rate of 29 mW/kg.

What is the range of epsilon values determined for the various stratospheric turbulent layers that were observed with LITOS instrument?
The focus of this first paper about LITOS is the description of the instrument itself and the results of laboratory measurements. Furthermore, by showing one first case study of a turbulent layer we demonstrate the determination of the energy dissipation rate. More geophysical analysis like variations of ε will be the task of a subsequent paper.

In the section 5 "discussion", the limitations (and possible improvements) of the technique should also be discussed : what are the minimal and maximal values of the inner scale which can be observed (due to the noise level or other factors)? Does this range of observable values depends on the altitude? What is the error induced by a poor knowledge of the relative wind?

We have extended the discussion with the following part: Finally we would like to discuss possible uncertainties within the determination of the energy dissipation rate. Small errors in the determination of the inner scale l_0 lead to significant uncertainties of ε, because $l_0 \propto \varepsilon^{1/4}$. The conversion of frequency scales into spatial scales can be affected by the relative movement of the gondola. For that reason we included an accelerometer in our payload. However, for the conversion the particular ascent rate is used, which is modulated by the relative horizontal wind by the root of the sum of squared velocities (vertical and relative horizontal). For the BEXUS 6 launch we therefore estimated an error of the inner scale due to ignorance of relative horizontal wind of $\sim 10\%$ resulting in $30 - 50\%$ error in ε. This error can be significantly reduced by careful analysis of the acceleration data. A second error source is the fact, that the data sequence taken for a single turbulence spectrum is not necessarily filled with homogeneous turbulence. Typically spectra are calculated from 40–100 m sections of data. The turbulent layer has to fill a large part of this section to produce a clear spectral signal. Nevertheless inhomogeneous turbulence might result in some smearing of the spectra especially in the transition region of inertial and viscous subrange even for this high resolved observations. By this the determination of the inner scale might be affected and thus also the energy dissipation rate. For turbulent layers thinner than 40–100 m we assume a maximum error of $\sim 10\%$ for the inner scale, similar to the factor induced by ignoring the relative horizontal wind. Nevertheless, we would like to point out that in contrast to other methods, the calculation of ε does not depend on absolute spectral densities (which may be affected by changing instrument sensitivities) but only on the precise measurement of the temporal variations. The range of ε values which can be observed with LITOS vary between 2.07×10^{-6} W/kg (maximum l_0 of 1 m) and 5.31 W/kg (minimum l_0 of 0.025 m). We would like to underline that this range of energy dissipation rates can be measured by LITOS from the boundary layer up to the middle stratosphere, as the noise level is constant with altitude.

Comments concerning typos or language have been taken into account.

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