Atmos. Meas. Tech. Discuss., 3, C1728–C1734, 2010
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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.

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Received and published: 18 October 2010

Referee Comment about Atmos. Meas. Tech. Discuss., 3, 3455–3487, 2010 manuscript entitled "Fine-scale turbulence soundings in the stratosphere with the new balloon-borne instrument LITOS" by A. Theuerkauf, M. Gerding, and F.-J. Lübken

Reviewer : Francis Dalaudier, LATMOS

Scientific Significance: Good (2) This manuscript contains the description and some preliminary results for a new technique for balloon-borne in-situ measurement of the turbulence within the stratosphere. This technique allow to observe the viscous cutoff of the turbulent velocity spectra and thus to deduce the dissipation rate ε with few hypothesis.

Scientific Quality: Fair (3) The description of the methods used and the discussion of the hypothesis is not always clear. Some important information is not properly presented or sufficiently discussed in my opinion.

Presentation Quality: Fair (3) The most important results and conclusions are properly presented. However, I am sure that the manuscript can be improved in order to reach more detailed and accurate conclusions that could be useful to a broader scientific community. I will give more details and examples below.

General comments

The manuscript describe a new instrumental technique for the measurement of the turbulence in the stratosphere and presents some preliminary results in order to demonstrate its practical feasibility and to prove its potential scientific value. The Journal "Atmospheric measurement techniques" is thus particularly well suited for its publication. 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. However, other important questions are not sufficiently discussed in my opinion (details are given below). I believe that the manuscript should be revised in order to give more detailed discussions of some experimental issues. 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.

Specific comments

The introduction contains a discussion of the role of turbulence within the stratosphere (and the atmosphere in general) as well as short review of various techniques used

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for its measurement. I do not always agree with some of the sentences or with the way to present information. However this is not an important issue for an experimental paper. The motivation for the development of a new experimental technique is properly discussed.

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. More generally, the logical connection between scientific or technical requirements and the choices for the geometry of the experiment does not appear sufficiently within this part. Some statements are not convincing. 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.

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. 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.

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.

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.

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.

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

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

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.

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 ?

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

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to be clarified. Similarly, the statement P3464L4 “we demonstrate later in this paper” is not convincing in my opinion.

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.

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”.

IMPORTANT : 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.

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.

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Is the (spectral) noise level of your instrument P3466L5-6 constant for all altitudes ? 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.

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.

What is the range of epsilon values determined for the various stratospheric turbulent layers that were observed with LITOS instrument ?

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 ?

Technical corrections

- I feel that "wind" should describe large scale motion while "velocity" is more appropriate for 3D turbulence

- P3456L8 typo : weights

- P3458L8 at least -> at most (?)

- P3458L9 typo : up to now

- P3459L3 plated -> coated ; typo : 5 m -> 5 μm (also P3460L26)

- P3460L11 2 s resolution ; P3467L18 1 s sampling rate (inconsistent ?)

- P3467L7 unit for ε (dissipation rate) is W/kg = m2/s3

- P3461L8 in particular -> mostly

- P3461L6 flow velocity and temperature

- P3462L6 give lower range for density ; upper range for temperature

- P3463L2 according to Figure 3, at pressure lower than 750 hPa, calibration wind is always larger than 6 m/s (not 3 m/s).

- P3472L14 the inequalities are inconsistent and the range of Kn for transition flow is missing. May be the Figure 11 would be easier to read with a log scale for Kn. I also suggest to add approximate altitudes corresponding to the pressure scale.

Interactive comment on Atmos. Meas. Tech. Discuss., 3, 3455, 2010.