

## ***Interactive comment on “Comparing turbulent parameters obtained from LITOS and radiosonde measurements” by A. Schneider et al.***

**Anonymous Referee #1**

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General comments

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This paper deals with the comparison of turbulence measurements in the free atmosphere based on in situ measurements under balloon by two independent methods. One method is based on the detection of unstable lapse rates in the potential temperature profile (Thorpe method). For this analysis, the data consists in the raw profiles of standard radiosoundings with a relatively coarse resolution (5 m). The other method relies on wind measurements with very large sampling frequency (8 kHz). The dissipation rate of turbulent kinetic energy (TKE) is inferred from the spectral analysis by detecting the transition scale from inertial to dissipation domain. Mainly, the authors

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compared two outer scales of turbulence, the Ozmidov scale  $Lo$  et the Thorpe scale  $Lt$ . It turns out that no clear relation can be evidenced between these two scales.

Such a work may constitute a very significant contribution about the characterization of turbulence in the troposphere and lower stratosphere. It is potentially interesting to compare various estimates of the energetics of turbulence, especially because the Thorpe method, although indirect, is very easy to perform from the huge data base of daily radiosoundings.

However, there are several major issues that need to be addressed before publications.

1) Is the comparison between  $Lt$  and  $Lo$  relevant in the way it is performed? To my opinion it is not. In the troposphere, the vertical extent of turbulent eddies frequently reach several hundred meters (as the Thorpe analysis confirms). The Thorpe length  $Lt$  is a second order statistic estimated within regions which vertical extent is significantly larger than  $Lt$  (two to three times). Now, you estimate  $Lo$  (Ozmidov length) with a vertical resolution of 10 m within a sliding window of 25 m. It is well known that turbulence is not homogeneous (it is indeed very intermittent as your measurements nicely illustrate). Is it meaningful to compare a second order statistics ( $Lt$ ) to local estimates of  $Lo$  (i.e. of epsilon) within constant height interval of 25 m. I suggest the authors to systematically perform some averaging on epsilon within the spatial domain where  $Lt$  is estimated before comparing  $Lt$  and  $Lo$ .

A comparison of measurements can hardly be based on the comparison of ourter scales if a constant window 25 m depth is used. An other way to compare measurements follows: for each 25 m window, a TKE dissipation rate, epsilon, is estimated. From epsilon, a variance of the wind velocity can be inferred by assuming a Kolmogorov spectrum. How this velocity variance compare to the turbulent potential energy (TPE) (which can be defined within the same window from the variance of the temperature fluctuations?).

2) Second issue: is there cloudy air in the troposphere? If it is the case, the dry

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potential temperature profile used for the Thorpe sorting is not relevant. The effect of water vapor saturation must be considered by using a "moist" potential temperature profile (i.e. Wilson et al., 2013)

3) I have some doubts (other than question 2) about the Thorpe analysis performed in this paper, especially in the troposphere. What is the mean trend-to-noise ratio (TNR) in the troposphere? What is the minimum size of the layers selected as turbulent? (the minimum  $L_t$  being 10 m, it suggests that you retain the inversion of two consecutive bins in the potential temperature profile as significant. Such a two bins layer is very dubious). If the mean TNR is smaller than unity, a pre-processing of the data is likely required. For instance, Wilson et al. (2011) decimated and filtered the potential temperature profile. Consequently, according to these authors, the minimum size of turbulent layers was  $\sim 50$  m in the troposphere.

#### Specific comments

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p. 19035, l. 3-4: the statement about "static instability which drive turbulence" is unclear. The detected decreasing in potential temperature does not imply that static instability is the driving process. Turbulence driven by mechanical (shear) instability will also produce overturns (i.e. decreasing) in the potential temperature profile.

p. 19036, l. 4: A recent paper (Wilson et al., JASTP, 2014) shows few case studies of turbulent layers in the troposphere detected simultaneously by radar and balloon. Estimates of  $L_t$  and  $L_o$  are reported.

p. 19038, l. 11: Please, explain this interval for epsilon (it does not correspond to the interval for  $l_o$ , i.e. fit error)

p. 19038, l. 11: Is there an objective criterion in order to discriminate turbulent and non-turbulent spectra? Is it based on a visual check of each spectrum?

p. 19039, l. 7: The relatively low value for the mean  $L_t$  in the troposphere (26 m) is very

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likely due to the large number of occurrence of the small size inversions (10 or 15 m). According to me, such inversions cannot be detected from radiosondes: a statistics on the range of two or three points is not significant, especially if TNR is small.

p. 19039, l. 21: The observation of turbulence at 21.73 km is done for a single height interval ( $\sim 25$  m). Turbulent layers of such a scale can hardly be detected by the Thorpe method from radiosondes (see Wilson et al. 2011 for instance). The lack of coincidence for such depths is not surprising.

p. 19040, l. 3-4: What is the percentage of turbulent layers detected by both methods? And what is the scale of this simultaneous detection? (in other words, is the simultaneous detection dependent on the size of the turbulent layers?)

p. 19040, l. 15: There is no scale for  $N$  (cyan curve) on the right panel of Fig. 3.

p. 19040, l. 15: The cyan curve ( $N$  ?) on the right panel of Fig. 3 is discontinuous. Why?

p. 19041, l. 5: I suggest the authors to show the distributions of  $L_t$  and  $L_o$ ? Are they similar?

p. 19042, l. 5: I agree with the way the comparison is performed. You could complete this plot with a scatter plot. Is there any correlation between the two estimates?

p. 19042, l. 12: Your study is not limited to stratospheric conditions.

p. 19043, l. 6: What is the size of the non-detected layers by the Thorpe method?

p. 19043, l. 9: Again, I do agree with the assertion that the instable layers detected by the Thorpe method are driven by convective instabilities

p. 19043, l. 13: What is the size of the detected layers by the Thorpe method which are not seen by LITOS? (I suspect that these layers are noise induced, they are likely of small vertical extent).

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p. 19043, l. 21: What is this limit?

p. 19044, l.1: Your work certainly questions the Thorpe analysis (at least in the way you performed it, see remarks above), but also the LITOS results. Before comparing epsilon estimates, I suggest the authors to validate their TKE by comparing with TPE from temperature measurements.

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