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Interactive Comment

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

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We thank the referee for his helpful comments. Below we cite each comment (indicated by italics) followed by our answer.

We would like to point out that our main focus is comparing dissipation rates from the Thorpe analysis as recently used by several authors with our own independent method. It is not primarily comparing L_O and L_T.

General comments

1) *Is the comparison between L_T and L_O 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 L_T is a second order statistic estimated within regions which vertical extent is significantly larger than L_T (two to three times). Now, you estimate L_O (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 (L_T) to local estimates of L_O (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 L_T is estimated before comparing L_T and L_O.*

Such an averaging is exactly what we have done. We have averaged both ε and N over the unstable layers detected by the Thorpe method (in the following denoted by averaging brackets $\langle\cdot\rangle$) and then computed the Ozmidov scale via $\overline{L_\mathrm{O}} = \sqrt{\langle\varepsilon\rangle/\langle N\rangle^3}$. The blue barplots in Fig. 3 and Fig. 7 show $\overline{L_\mathrm{O}}$ and $\langle\varepsilon\rangle$, respectively, computed in that way. The cyan curves present the unaveraged quantities $L_\mathrm{O} = \sqrt{\varepsilon/N^3}$ and ε directly as measured by LITOS. We have made the description of our procedure clearer in the revised version.

p. 19040 l. 4 has been changed to: The energy dissipation rate (obtained from LITOS) is averaged over the layer (as detected by the Thorpe analysis of radiosonde data). Such means over a Thorpe layer will be denoted by averaging brackets $\langle\cdot\rangle$. For each unstable layer, the resulting $\langle\varepsilon\rangle$ is plugged into Eq. (2) to infer an Ozmidov scale $\overline{L_\mathrm{O}} = \sqrt{\langle\varepsilon\rangle/\langle N\rangle^3}$ for the layer.

A comparison of measurements can hardly be based on the comparison of outer 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?).*

The size of the window at the computation of ε from the wind fluctuations measured by LITOS can be reduced, e. g., to 2 m with 1 m overlap. Then the ε profile shows more intermittency, as expected. Nevertheless, if the analysis of our paper is carried out with such a higher resolved ε profile, the results are similar, due to the averaging method mentioned above.

A comparison of TKE and TPE would indeed be interesting, as only few observational datasets are available and the fundamental understanding of turbulence may be improved. However, it is outside the scope of this paper.

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

We thank the referee for this suggestion. Originally we focussed on the stratosphere so that we did not consider moisture. In the revised version, we implement the method from Wilson et al. (2013) using the moist potential temperature. However, the difference between moist and dry computation is small. For BEXUS 12, the number of significant unstable layers changes from 259 to 254, the mean layer thickness from 50 m to 53 m, the mean Thorpe length in the troposphere from 26 m to 29 m. The mean dissipation rate has not changed significantly. For BEXUS 8, we originally used only the data from 7 km upwards. Now we have included the tropospheric data as well. Again, the difference between dry and moist computation is small; there is only one saturated layer, and only one significant unstable layer intersects with it, so that the averages do not change significantly.

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At p. 19038 l. 16 we have inserted: Moisture is cared for using the routine given by Wilson et al. (2013). To this end, saturated regions are detected, and a composite potential temperature profile Θ_* is computed by integration of $\partial\Theta/\partial z$ using the moist buoyancy frequency within those saturated regions and the dry buoyancy frequency otherwise.

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 ~ 50 m in the troposphere.*

The Thorpe analysis shown in the discussion paper is carried out as described in Wilson et al. (2011) (assuming a dry atmosphere). The mean trend-to-noise ratio (TNR) is $\bar{\xi} = 2.3$ for the BEXUS 8 flight and $\bar{\xi} = 4.2$ for the BEXUS 12 flight; in the troposphere, we have $\bar{\xi}_\mathrm{Tropo} = 7.2$ for BEXUS 8 and $\bar{\xi}_\mathrm{Tropo} = 12$ for BEXUS 12. For the moist computation, $\bar{\xi} = 1.7$ (BEXUS 8) and $\bar{\xi} = 4.1$ (BEXUS 12). Nevertheless, we have several significant unstable layers with a thickness of 10 m or 20 m. Indeed, it is questionable whether such thin layers can unambiguously be reproduced by a radiosonde with 10 m resolution. But LITOS also shows thin layers of 10 m ... 20 m thickness. Ignoring the thin layers in radiosonde data, e. g., by smoothing of the Θ profile prior to Thorpe sorting, would result in much less coincident layers especially in the stratosphere and, by this, would bias the comparison between both methods. In order to avoid any a-priori biases we take the thin layers in the radiosonde data into account, especially since they fulfil the criteria given by Wilson et al. (2011, 2010).

We have added at the end of Sect. 2 (p. 19039 l. 2): The mean trend-to-noise ratio

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(TNR) is $\bar{\xi} = 1.7$ for the BEXUS 8 flight and $\bar{\xi} = 4.1$ for the BEXUS 12 flight.
Several thin layers of only 10 m or 20 m passed the significance test. We are aware that this is on the edge of radiosonde capability. Nevertheless, LITOS also shows many thin layers. Ignoring the thin layers, e. g., by smoothing the Θ_* profile prior to Thorpe sorting would result in much less coincident layers especially in the stratosphere and, by this, bias the comparison. In order to avoid any a-priori biases we take the significant thin layers in the radiosonde data into account.

Specific comments

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. 19043, l. 9: *Again, I do* [not? author's note] *agree with the assertion that the instable layers detected by the Thorpe method are driven by convective instabilities*

Indeed, strong three-dimensional wind shear can also produce a potential temperature inversion. Such a negative Θ gradient is by definition a static instability. But in this case you would not call the static instability the driver. We have changed our phrasing as follows:

p. 19035, l. 3–4: The evaluation uses the method developed by Thorpe (1977, 2005) to detect static instabilities as a proxy for turbulence.

p. 19043, l. 9: Not all turbulence is related to static instabilities. Even if initially a negative potential temperature gradient may have occurred, it is removed by the turbulent motions which outlive the instability;

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.*

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Estimates of L_T and L_O are reported.

We thank the referee for pointing out this paper which had eluded our notice. We take it into account in the revised version and have rephrased this sentence as follows:

But for the atmosphere there are only few examinations of the proportionality (e. g. Gavrilov et al., 2005; Kantha and Hocking, 2011; Wilson et al., 2014).

p. 19038, l. 11: *Please, explain this interval for epsilon (it does not correspond to the interval for l_0, i. e. fit error)*

The errors for $l_0 = c\sqrt[4]{\nu^3/\varepsilon}$ and ε are related by Gaussian error propagation,

$$\Delta l_0 = \sqrt{\left(\frac{\partial l_0}{\partial \varepsilon}\right)^2 \Delta\varepsilon^2 + \left(\frac{\partial l_0}{\partial \nu}\right)^2 \Delta\nu^2} \approx \frac{c}{4}\frac{\nu^{3/4}}{\varepsilon^{5/4}}\Delta\varepsilon = \frac{l_0}{4\varepsilon}\Delta\varepsilon,$$

where the approximation neglects the error in the kinematic viscosity ν, i. e. $\Delta\nu = 0$.

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?*

The decision whether a spectrum is regarded as turbulent is made automatically based on objective criteria. First, the noise level is detected to select the fit range. If the noise level detection fails, the spectrum is sorted out. The fit of the Heisenberg spectrum is performed and then a set of criteria is applied to sort out bad fits, which occur for non-turbulent spectra. Those criteria are:

- The inner scale l_0 has to be within the fit range.
- The value of ε has to be greater than zero and less than 100 (plausible values).
- The mean distance between the data and the fit has to be less than a given threshold.

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p. 19038 l. 11 has been changed to: A spectrum is regarded as non-turbulent if the noise-level detection fails, if the inner scale l_0 is not within the fit range, if ε has implausible values (less than zero or greater than 100 W/kg), or if the mean distance between the fit and the data is larger than a fixed threshold. That means the decision is made automatically based on a set of objective criteria.

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

As described above (comment 3), we performed a significance test as suggested by Wilson et al. (2010). We agree that such thin layers are at the edge of the capability of the radiosonde analysis. Nevertheless we take these data into account to avoid a bias compared to our LITOS data showing similar thin layers.

p. 19039, l. 21: *The observation of turbulence at 21.73 km is done for a single height interval (~ 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.*

The referee is right that such thin layers are at the limit of radiosonde capabilities. Mainly we want to point out that there are turbulent layers not visible for the radiosondes (Thorpe method). For the revised version we select another altitude region which shows a larger turbulent layer (~ 80 m thickness) detected by LITOS but not by the Thorpe method.

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?)*

We have added the following paragraph in the discussion after p. 19043 l. 15: Not all

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layers are detected by both systems. Of the significant layers detected by the Thorpe analysis, 86 % (BEXUS 8) and 69 % (BEXUS 12) are also detected by LITOS. For BEXUS 12, the mean thickness of significant unstable layers as detected by the Thorpe analysis is 53 m. The mean thickness of those significant layers also detected by LITOS is 63 m, that of significant layers not detected by LITOS only 31 m. That means that the simultaneous detection depends on the size of the layer; mainly thin layers are detected by only one method. But as this only applies to 30 % of the layers, and those layers were taken out of the comparison, the bias for our results should be small.

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

Both blue and cyan curves show L_O; the difference is that the cyan one has the full resolution of the ε profile from LITOS, while the blue barplot shows the mean over the unstable layers detected by the Thorpe method, $\overline{L_\mathrm{O}}$, cf. (1) under general comments. We make this clearer in the revised version. The discontinuities originate from $L_\mathrm{O} = 0$ which cannot be shown in a logarithmic plot. There is no plot of N in that figure.

p. 19041, l. 5: *I suggest the authors to show the distributions of L_T and L_O? Are they similar?*

We have extended Fig. 5 to show the distributions of the composite dataset of BEXUS 8 and BEXUS 12 (i. e. all data points seen in the graph) on the right and top axis, respectively. The histograms show a generally similar behaviour at scales larger than 10 m. The maximum for the Thorpe length is slightly larger than for the Ozmidov scale. At small scales L_T is limited by the resolution of the radiosonde which produces the cut-off at 10 m, while the histogram for $\overline{L_\mathrm{O}}$ shows a continuous decrease.

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?*

There is no obvious relation between $\varepsilon_\mathrm{LITOS}$ and $\varepsilon_\mathrm{Thorpe}$. The correlation coefficient

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between both is 0.39 for BEXUS 8 and 0.06 for BEXUS 12 (including moisture effects). The numbers will be mentioned in the revised version. We would like to avoid a scatter plot because it contains no additional information compared to Fig. 5 and Fig. 8.

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

We have no overview about tropospheric studies, but want to point out that our study is at least the first for stratospheric conditions.

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

The mean thickness of LITOS layers not intersecting a Thorpe layer for BEXUS 12 is 22 m. As expected this is quite close to the resolution of the radiosonde data.

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).*

The mean width of significant layers detected by the Thorpe method but not by LITOS is 31 m (BEXUS 12), i. e. they typically comprise several data points. As described above, we performed a significance test as suggested by Wilson et al. (2010). In the revised version, this information is in the paragraph mentioned above.

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

Window length and trend removal limit the frequency scales of the power spectral density on large scales (i. e. small frequencies). A reasonable part of the inertial range has to be resolved to enable a fit. Additionally, because the data points lie much less dense at large scales in the loglog plot, only few data points determine the fit at very large inner scales l_0. By visual inspection of real and artificial spectra with random l_0, the limit for a reliable fit is estimated to ~ 1 m for a window length of 25 m. (The limit depends on the window length.) The largest l_0 for the BEXUS 8 and BEXUS 12 flights is 10 cm and 8.7 cm, respectively. That is far away from the limit. In the revised version, the paragraph has been supplemented with this information.

We have changed our phrasing as follows: For large scales (i. e. low frequencies or small ε), the detection limit is determined by the trend removal and the window length. As a reasonable part of the inertial range has to be resolved to enable a fit, the limit is estimated to ~ 1 m. The maximal identified l_0 values of 10 cm and 8.7 cm for BEXUS 8 and BEXUS 12, respectively, were far below 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.*

We do not question the Thorpe analysis as a whole. Fig 6 shows that mean ε are in reasonable agreement with our LITOS results (cf. p. 19044, l. 2–3). For individual layers we find large discrepancies in ε. We would like to point out that these discrepancies are expected, taking all the different published values for c^2 into account. Also Wilson et al. (2014) report estimates of L_{T} and L_{O} for a limited number of layers which lead to values for c^2 varying between 0.1 and 1.6. As mentioned above, the comparison of TKE and TPE is another fundamental topic that is outside the scope of this paper.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/14/C8401/2014/acpd-14-C8401-2014-supplement.pdf

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