

Interactive comment on “On the effect of moisture on the detection of tropospheric turbulence from in situ measurements” by R. Wilson et al.

R. Wilson et al.

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Response to reviewer #2

The authors thank the anonymous reviewer for its constructive comments and suggestions. The paper is certainly improved, thanks to the suggestions of the reviewer.

About the specific remarks:

(1) The Thorpe method is very simple to apply for vertical profiles of potential temperature/density (i.e. unidimensional parameter): it simply consists in reordering the profile in increasing order (with a sorting algorithm). There is a unique reference profile (if data of the sample are identical, then, their positions, i.e. their order in the profile, are

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not altered). The best description of the method can be found in the seminal article by Thorpe (1977). Since then, numerous papers described or discussed in detail the Thorpe method (e.g. Ferron, 1998; Johnson, 2004; Wilson, 2010). Two interesting papers are: K. B. Winters, P. N. Lombard, J. J. Riley, and E. A. D'Asaro, "Available potential energy and mixing in density-stratified fluids", J. Fluid Mech., 289, 115–195, 1995. and Winters, K.B., D'Asaro, E.A., 1996. "Diapycnal mixing and the rate of fluid mixing", J. Fluid Mech. 317, 179–193. These authors applied the concept to 3D fields. They discussed about the physical interpretation of the sorted profile. Thus, we do not think that an additional appendix is necessary for describing the basic procedures.

(2) Following the question raised by the reviewer, we checked for the dependence of error on vertical resolution. The error is here defined as the difference between the directly calculated theta values (i.e. by using the definition of theta) and the theta values resulting from vertical integration of N^2 . There is an effect related to resolution, but such an effect appears to be negligible for our purpose. We found a difference in theta values of about 0.002 K at 15 km altitude (tropopause level) by using 3 m resolution profile. With such a vertical resolution, the error is 0.05 K at the top of the profile (~30 km altitude). Such differences are negligible for our purpose. By using coarse resolution profiles (100 m), the errors are far larger since it reaches 0.2 K at tropopause level and 2 K at 30 km altitude. But even with such a coarse resolution (useless for turbulence detection) the differences between the profiles remain tiny.

(3) Done.

(4) The measurement of humidity remains challenging, especially at low temperature, and improvements are slow. The empirical methods used for detecting saturation thresholds aimed precisely at correcting systematic errors of humidity sensors. We followed the advice of the reviewer and modify the sentence accordingly.

(5) The reference Campos et al (2007, RS) is indeed relevant. The authors noticed that precipitations as low as 4 mm/hr can affect VHF Doppler spectra and can produce

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peaks as strong as clear air echoes (page 9 of 18).

(6) Sentence modified.

(7) In the present work, the radar measurements are only used for assessing the pertinence of turbulence detection from the θ_{*} profile (turbulent echoes are expected to be isotropic), especially for regions presumably turbulent which are not detected from the (dry) θ profile. The comparison of energetics of turbulent events observed by the different instruments is out of the scope of the present work.

(8) Following the suggestion of the referee we added a sentence about radar aspect sensitivity and moisture. We added the following sentence, page 8231, after line 20: "Such characteristics of the radar echoes in cloudy or precipitating regions have already been reported by Vaughan and Worthington (2000) and are fully consistent with the turbulence statistics made in cloudy and/or precipitating regions by Hocking and Hocking (2007).

(9) Corrected.

(10) We basically agree that RH and moisture measurements are not fully reliable. A sentence indicating that the quantitative results should be taken with caution has been added. However, we stressed that the results should be more reliable with the improvement of sensors.

(11) Corrected.

Interactive comment on Atmos. Meas. Tech. Discuss., 5, 8223, 2012.