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Atmospheric Measurement Techniques Discussions

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.

richard.wilson@upmc.fr

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Response to reviewer #1

The authors thank the anonymous reviewer for its constructive remarks. The paper is certainly improved, thanks to the suggestions of the reviewer. Thank you also for the technical corrections.

About the specific comments:

(1) For subsaturated atmosphere, the effect of water vapour on static stability is negligible, at least for the mid-latitude free troposphere. We have checked for such an effect on turbulence detection (it was indeed the very first step of the current work). The

results based on the use the virtual potential temperature theta_v for the detection of turbulence were identical to those based on theta_d for the 36 flights of the TANUKI campaign.

However, for consistency with the previous studies raised by the reviewer, the expression of the potential temperature theta_* now includes the water vapor contribution for subsaturated air (see expression 6 of the revised manuscript). As mentioned above, it has no effect on our results but may not be totally negligible for data collected in tropical regions where water vapor mixing ratios can be significantly larger (due to higher temperature).

(2) The possible problem of insufficient ventilation of the sensors was not considered in our study. Balsley et al. (2010) quoted "the absence of reliable humidity values" from their dataset. We do not come to the same conclusion. The ascent speeds of the balloons launched by Balsley et al. are substantially lower than the ascent speed of our balloons (their maximum speed -$\sim$3 m/s- corresponds to our minimum speed). Thus, the problem reported by Balsley et al. is likely less critical for our measurements. As both studies rely on Vaisala RS92 radiosondes, it is possible that the ventilation problems can be avoided by using ascent speed equal or larger than 3 m/s.

(3) The grids are now removed.

(4) The referee pointed out the absence of selected turbulent layers around the altitudes of $\sim$4.0 km 6.0 km (and also below 3.5 km) where radar echoes are very weakly aspect sensitive. Because non aspect sensitive echoes would be a signature of isotropic turbulence, the comparisons do not seem to be consistent. First, it is worth noting that the isotropic nature of the radar echoes can also be a characteristic of the radar echoes at the initial range resolution, i.e., 150 m (not shown). It is thus not a spurious effect resulting from the application of range imaging. However, echo power minima between, or at the edges of, strong power maxima cannot be well estimated from the range imaging technique (whatever the data processing may be, see for ex-

ample, Figure 1 of Luce et al. (2001, JASTP, 63, 221-234)). The intensity of the radar echo minima is not fully reliable and is likely strongly overestimated in some cases (depending on SNR and performances of the processing methods used). Therefore, regions of echo minimum should be interpreted with caution. The thin and isotropic peak at $\sim$2 km should be a signature of turbulence but was not detected by the balloon. It might be due to threshold effects in the selection of the turbulent layers: some real turbulent layers can be wrongly unselected by the hypothesis test. Alternatively, the non-detection of such a layer might be due to the more sporadic nature of this thin layer (it was maybe not observed at all by the balloon sensors).

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