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Atmospheric Measurement Techniques Discussions

# *Interactive comment on* "Can one detect small-scale turbulence from standard meteorological radiosondes?" *by* R. Wilson et al.

**Anonymous Referee #1**

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This is a well-formed analysis of LR and HR radiosonde profiles to investigate the occurrence of potential temperature (Thorpe) overturns in the troposphere and stratosphere using co-existing data sets. The de-noising analysis seems relatively straightforward and certainly timely. The English syntax needs a bit of editorial work, but I had no problem with the general flow of the development.

The MS is certainly timely and worthy of publication. I have some issues that should be addressed and clarified. Comments are itemized below:

1. The authors fail to carefully distinguish between the occurrence of overturns and the existence of turbulence. The radiosonde measurements detail just the overturn events. There is no direct measurement of turbulence per se. Yet an assumption of a one-to-one association between these two phenomena is made often throughout the text. For



example, in lines 33-4, the statement of convective instabilities producing turbulence is reasonable. However, that process is only one (static) method of generating turbulence. Is it not reasonable to point out that turbulence generation can also occur from dynamic processes (Richardson Number criteria arising from shears)? Cannot such dynamic processes occur in the absence of overturns? And is there really an established one-to-one relationship between an overturns and turbulence? For example, what is the overturn disappears before turbulence processes are fully developed? What about a discussion of the initial non-linear growth of a gravity wave that can generate an overturn before the static turbulence begins?

Lines 56-7 appears to allow gravity wave overtuns without invoking turbulence. The next sentence, however, points out that artificial overturns can overestimate turbulence (overestimate overturns seems more reasonable).

Lines 71-2 asks whether one can detect small-scale turbulence directly using radiosonde data (wouldn't it be more correct to ask whether overturns can be accurately detected? (again, radiosondes do not detect turbulence directly.

Line 84 also uses turbulent overturn selection (merging the two processes into one) .

These are merely only a few examples of the too-close association that occur throughout the text. I feel that this manuscript would be much clearer if the authors would take the time to clarify this criticism.

2. Line 67 Please define the term 'first differences' for those of us who are unfamiliar with the term. 3. At this point, I will skip to the Results and Conclusions sections, which, while they are very informative, I feel they don't go far enough. The consequences of this work are pretty far reaching in my view. If I have interpreted their results correctly, both the troposphere and stratosphere can be described more in terms of the large amounts that are overturning than in terms of the regions that are quiescent. To support this comment, going carefully through Figures 7-8, I calculate that the six flights produced a total of about 40,900 meters of overturning (the sum of the number of oc-

currences for each size multiplied by the median size). Assuming that the radiosondes obtained data to about 30 km, this suggests that, on the average, more than 68% of the region is supporting overturnings, with the remaining 32% being quiescent. This is a pretty remarkable number. It is also consistent with earlier results by Cho et al.(JGR, 108, #D20, 2003: ‘Characterizations of tropospheric turbulence and stability layers from aircraft observations’), who suggest that > 54% of the troposphere is unstable when measured down to scales of 10 m.

While I recognize that this manuscript does not examine the implications of their analysis in details, I think that a discussion of this point would be useful.

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Interactive comment on Atmos. Meas. Tech. Discuss., 4, 969, 2011.