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Atmospheric Measurement Techniques Discussions

Interactive comment on "On the estimation of vertical air velocity and detection of atmospheric turbulence from the ascent rate of balloon soundings" *by* Hubert Luce and Hiroyuki Hashiguchi

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The authors have measured the ascent rate of weather balloons, along with corresponding radar and other measurements. They point out that when atmospheric turbulence is present, the drag coefficient of the ascending balloon is reduced, and the ascent rate increases as a result. They argue that this effect is stronger that other effects, and therefore these fluctuations in ascent rate actually indicate the strength of turbulence, except in the case where turbulence is very weak. For the three case studies that they discuss, I found their arguments plausible.

They also suggest that the same arguments explain previous results, in particular the experiments by McHugh, et al (JGR, 2008), which showed an increase in ascent rate near the tropopause over Hawaii. Thus they suggest that these increases in ascent rate over Hawaii are really due to turbulence rather than a local increase in vertical velocity. However I am unconvinced that turbulence really can explain the previous results of McHugh et al. The results here show a change an ascent rate on the order of 1 m/s, but McHugh et al found an increase that was at times more than 7 m/s, meaning the balloon ascended more than twice as fast for a short distance. I am unconvinced that turbulence can cause this large of an increase. Most of this increase I think is indeed due to an increase in vertical velocity. The authors arguments don't really contradict this, as their own data only shows small increases. However I am now convinced that the large increases in ascent rate were partially due to turbulence, and thus the increase in ascent rate is overpredicting the local velocity.

I think the paper is publishable with minor revision. The revisions should include rewording the discussion of McHugh et al results with some comments about the size of the change in ascent rate.

The writing was fine. I have added a few other relatively minor issues below:

1. In figure 8, I can clearly see the difference in structure between the troposphere and stratosphere in the profiles of V_B, but it is not clear to me that the difference is simply waves versus turbulence, as is suggested. I think that waves are still important in the troposphere.
2. Figure 9, the 'peak' is quite broad and difficult to align with the critical R_i of 0.25 for stability. Is the breadth of this feature due to experimental error, or is the concept not quite right?
3. On page 8, '...in R_i value bands of 0.25 in width' is not an adequate description of analysis that results in figure 9c,d. What was done exactly to the data to get

this figure?

4. Why is Figure 10 rotated by 90 degrees when compared to figure 9?
5. Figures 5,6, and 7 I found to be a bit too messy, with different panels not separated by any space. It was hard to tell where one panel ended and the other began.

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