Reply to Reviewer 1

Anonymous Referee #1

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Precise measurements of vertical winds are an important topic with many meteorological applications. The paper by Luce and Hashiguchi deals with the calculation of vertical winds from radiosonde ascent rate measurements. Recently, different publications described new methods to separate the different parameters influencing the ascent rate, like vertical winds, drag coefficient of the balloon and other effects. Nevertheless, direct comparisons of retrieved vertical winds with independent observations are rare. In the first part of their paper, Luce and Hashiguchi make use of collocated UAV measurements of atmospheric turbulence and vertical wind measurements by

radar. This analysis is limited to altitudes below 7 km because the drift of the balloon and local inhomogeneities make further comparisons arbitrary. In the second part, they make a statistical analysis of a series of 376 radiosondes, confirming their results that the stability of the atmosphere influences the ascent rate of the balloon. In their main conclusion, the authors state that in a turbulent atmosphere the vertical winds can hardly be calculated without detailed knowledge of turbulence parameters. On the other hand, the ascent rate profile can be used to identify turbulence in the atmosphere. The paper is generally well written and concise. The arguments are described comprehensively and clearly. In the following, I describe only some minor comments that should be clarified before publication.

We thank the reviewer for his/her positive comments.

Minor comments:

ll. 82-83: I do not see the results of Gallice et al. (2011) limited to Tu=4%. The main "problem" is that they do not account for inhomogeneities in the turbulence field.

The statement is indeed wrong and has been suppressed in the revised version. We apologize for this erroneous interpretation. The experimental drag curves described by Gallice et al. (2011) "present a qualitative shape similar to the curves by Son et al. (2010) at *Tu=6%* and *Tu=8%*." (incidentally, not 4% as stated). They further indicated that their drag curves suggest a turbulence intensity (Tu) of the atmosphere of the order of 6% to 8 % (page 2241). This statement indicates that inhomogeneities in the turbulent field were not considered.

Lines 82-83 have been corrected as follows:

"Their drag curve presented qualitative similarities with the curves by Son et al. (2010) for a mean turbulent state of the atmosphere at Tu=6 % and Tu=8%. The fact that the model proposed by Gallice et al. does not consider the variability of turbulence with height is likely a weak point because turbulence is generally confined into layers of variable depth in the troposphere and the stratosphere."

The corresponding sentence, line 274, has been shortened. “This feature was likely not well appreciated by Gallice et al. (2011) who considered a mean value of turbulent intensity over the whole atmosphere for establishing a model of c_D.” -> “This feature was likely not well appreciated by Gallice et al. (2011).”

Incidentally, in section 2.1, time sampling was $\Delta t = 2\ s$ and not 1 s as wrongly stated and a 20-sec rectangular window has been used for smoothing (not 10 sec). It is now corrected.

ll. 174-175: The agreement between V_Bc and W is expected from the calculation of V_z from the difference of W and V_B, and the definition of V_Bc. Is the calculation of V_z done in a different altitude than the V_Bc / W comparison?

No, it isn’t. We agree that the estimation of V_z is not fully reliable because it is simply based on segments in the V_B profiles for which energy dissipation rates are low and Richardson numbers are high (i.e. “minimum of turbulence”). For these segments, it is assumed that the free lift and vertical air motions are the dominant contributions to V_B.

l. 179: I am sorry, but I cannot identify the oscillations from below 3.8 km in the MCT layer above 3.8 km. Looking at the dashed lines the higher frequencies seem to dominate. Please explain.

We agree that it is not as clear as in Figure 7 where fluctuations produced by a MCT layer were stronger. We simply removed this description because it does not provide any substantial information.

l. 235: Please explain in short, why <V_B>_ST is not exactly the ascent rate in still air in the stratosphere.

The calculation was made by including *all* balloon data above 17.2 km assuming that the balloons were not at all affected by turbulence. This hypothesis cannot be true and, on some occasions, we were able to identify positive disturbances of vertical ascent rates that may result from turbulence effects. Therefore, the mean ascent rate in still air estimated from data above 17.2 km should be slightly overestimated. This overestimation produces the negative centered values for tropospheric data, below 16.3 km (Figure 9c).

ll. 267-270: Houchi et al. (2014) state in Section 6 a) that turbulence should broaden the ascent rate profile but not induce a tendency to purely higher ascent rates. Here, mainly the influence of turbulence on the drag coefficient is emphasized, yielding a higher ascent rate but not a broadening of the distribution. This seeming contradiction may be a question of the scales of turbulence cells. I suggest adding a clarifying sentence.

Yes, we agree. The broadening of the distribution was indeed attributed to turbulence by Houchi et al. (2015, p. 1810). It is possible that turbulence does not only produce aerodynamic effects but also advection effects due to billows of scales much larger than the balloon diameter. These effects should be similar to those produced by Kelvin-Helmholtz waves at early stages of the shear flow instabilities. Figures 9 and 10 do not only show an increase of ascent rates when Ri $\lesssim$0.25, but also a broadening of the ascent rate distribution, consistent with both effects occurring at the same time.

Figure 10b has been removed (because not useful).

Around lines 267-270, the text has been corrected as follows:

Figures 10 show $V_{Bc} - < V_{Bc} >$ vs Ri for the troposphere. A larger scatter is observed between Ri=0 and $Ri_c = 0.25$. The broadening of the scatter, as noted by Houchi et al. (2015), cannot be explained by the decrease of the drag coefficient and is necessarily due to both positive and negative vertical velocities. It is thus more likely due to turbulent billows of scales much larger than the balloon size. In addition, Kelvin-Helmholtz (KH) waves can also produce updrafts and downdrafts up to a few ms^{-1} when Ri reaches Ri_c (see, e.g. Fukao et al., 2011). Therefore, the enhanced variability of V_{Bc} when Ri is small (Fig. 9a) is presumably the combination of turbulence effects and vertical air motion disturbances produced by large scale billows and KH waves.

The following sentence (243-245):

"Assuming that the mean curve shown in Fig. 9c is statistically representative of the turbulence effects, then the scatter plot shown in Fig. 10a should also be statistically representative of W fluctuations produced by shear flow instabilities if other sources of vertical air motions are negligible."

has been removed, because it was misleading. The scatter plot in Fig. 10a (now Fig. 10) still contains a contribution from turbulence effects.

Fig. 8: Please provide a scaling for the ascent rate and the offset.

Technical comments and typos:

Done. Please note that half of the profiles were missing. They are now shown in the corrected figure.

We thank the reviewer for his technical comments and typos. The errors have been corrected.