Reply to Reviewer 2, Prof. J. McHugh

John McHugh (Referee)

john.mchugh@unh.edu

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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.

We deeply thank Prof. McHugh for his comments. His main recommendation is to revise the discussion we proposed about the interpretation of the strong localized increases in balloon ascent rates around tropopause over Hawaii reported by McHugh et al. (2008). Our study convinced him that turbulence may have contributed to these increases but still less than upward air velocity produced by mountain waves around their critical levels.

McHugh et al.'s interpretation in terms of gravity wave effects was made plausible owing to comparisons with a mesoscale model providing evidence of mountain waves in the conditions met by the balloons. We agree that interpreting McHugh et al.'s observations in terms of turbulence effects only may be speculative but we believe that it is not unrealistic. We discuss more thoroughly this hypothesis from additional materials and examples shown below.

The discussion about McHugh et al. results (lines 261-267) has been rewritten as follows:

"McHugh et al. (2008) interpreted isolated peaks of V_B of several ms^{-1} of amplitude near the tropopause and at the jet-stream level in terms of W disturbances around critical levels associated with mountain waves. The absence of corresponding negative disturbances was explained by the three-dimensional nature of the flow. Even though our hypothesis remains speculative in absence of additional and independent measurements of vertical air velocity, we suggest that turbulence effects may have also contributed to the observed increase in ascent rates since critical levels are generally associated with turbulence. A careful scrutiny of their figures 3-7 indicates that V_B increased at altitudes where the horizontal wind shear was enhanced and temperature gradient was close to adiabatic (so that Ri was likely small)."

It must be noted that, even if mountain activity was highlighted by McHugh et al. during the balloon flights, the vertical wind disturbances produced by the model (up to +/- 0.2 m/s, their Figures 8b and 9) were much smaller than the maximum values of ascent rate increases (a few m/s). Therefore, as noted by the authors themselves (page 8), the model did not confirm such large updrafts. In absence of independent measurements confirming or not the presence strong updrafts produced by waves, we cannot deal with the issue. However, as partly stated in the manuscript, the main arguments in favor of turbulence are:

1) all the "narrow" maxima in ascent rates (Figure 3-6 of McHugh et al.) seem to be associated with horizontal wind shears (speed and/or direction shears) and nearly adiabatic lapse rates, so that the Richardson number may be small enough for shear-generated turbulence. It would be consistent with our observations. Because this assertion is based on a simple and inaccurate visual inspection of the figures, calculating Ri profiles for the cases shown in Figures 3-6 would be useful for a possible confirmation.
2) The absence of decrease in ascent rate is consistent with a reduction of the drag coefficient due to turbulence effects. Strong three-dimensional mountain wave effects were suggested by the authors in order to overcome the absence of negative disturbances.
3) Prof. McHugh noted that the changes in ascent rate reported in our manuscript were $\sim$1 m/s (section 3 and section 4, statistics) but McHugh et al. observed increases up to 7 m/s, suggesting that turbulence effects alone cannot explain the phenomenon[1].
    a) The value of $\sim$1 m/s is approximately the mean value obtained from statistics (Figure 9c). It means that it can be larger on many occasions. The distribution (scatter) obtained after removing the mean value is positively skewed (especially around Ri~0.25, Figure 10), possibly indicating a remaining contribution from turbulence effects.
    b) According to Figures 1, 2 and 3 of Gallice et al. (2011), the drag coefficient c_D strongly varies with the Reynolds number up to a factor ~ 4 in the range $10^{-6} - 10^{-5}$ for both idealized and experimental conditions [the drag crisis being inexistent for the experimental curves]. Then, based on these results and because $V_z \sim c_D^{-1/2}$(expression 3 of Gallice et al.), an increase in the ascent rate by a factor up to ~2 can be predicted if Re strongly and quickly varies. For a standard ascent rate of 5 m/s in still air, a maximum increase of ~5 m/s is then theoretically possible. In our manuscript (section 3), we reported balloon measurements with slow ascent rates in still air (~2 m/s) because we used underinflated balloons. Therefore, ascent rate increases cannot theoretically exceed ~2 m/s. We reported ~1-1.5 m/s in Figures 5 and 6 (V_z in still air was $\approx$1.8 m/s) and ~2 m/s in Figure 7 ($V_z \approx 2.3$ m/s). As a result, the differences between the changes in ascent rates reported in section 3 and

[1] These values may depend on the method used for their estimations. Disturbances of ~1-2 m/s and ~ 3-5 m/s with respect to a "slowly varying background" can be estimated from Figures 5-7 of our manuscript, and Figures 3-6 of McHugh et al., respectively.

in McHugh et al. can be primary due to the different ascent rates of the balloons in still air (~2 m/s and ~6 m/s[2] in our manuscript and in McHugh et al., respectively).

4) In section 4 of our manuscript, we showed scatter plots made from a large amount of balloon data from pre-YMC campaign without focusing on individual cases. In addition, Figure 8 was not clear enough for evaluating the changes in ascent rates[3]. The balloons were inflated for a standard ascent rate of 5 m/s in still air, comparable to the conditions described by McHugh et al.

   Figure 1 below shows 8 consecutive profiles of balloon ascent rates V_B acquired on 19 December 2015, every 3 hours from 00:00LT above the altitude of 10 km[4] and shifted by $(n-1)*5\ m/s$ where n is the flight number. For easy reference, a profile of V_B shown by McHugh et al. (Figure 3) is superimposed to the profile at 06:00 LT (dashed blue line). Multiple fingers of strongly enhanced V_B values can be seen below the cold point tropopause CPT (blue dots). The enhancements are typically ~2-4.5 m/s and are thus now similar in amplitude to those reported by McHugh et al. The peaks of V_B are very often associated with Richardson numbers below the critical value (altitude ranges where Ri <0.25 are indicated by the red segments). Therefore, we feel that turbulence may produce ascent rate increases similar to those reported by McHugh et al. around the tropopause but we also agree that these observations are not sufficient for concluding that these increases are due to turbulence effects only.

   We have additional arguments suggesting that the ascent rate increases shown in Figure 1 are mainly due to turbulence effects and are not the signature of updrafts produced by gravity waves. Except maybe at 12:00 LT, V_B was systematically enhanced between CPT and a secondary strong temperature inversion below (indicated by red dots). This systematic increase is recognizable in the mean profile of V_B in the height range 15-16 km (thick solid line on the right side of the figure). During the campaign, the 47 MHz Stratosphere-Troposphere (ST) Equatorial Atmosphere Radar (EAR) was operating at Kototabang (Indonesia), located about 450 km North-West from the balloon launching site (Bengkulu). EAR provides similar information as MU radar with a time resolution of about 3 min and a range resolution of 150 m. Time and range resolutions of ST radars are very well adapted for studying horizontal and vertical wind disturbances produced by mountain waves (e.g. Röttger, *ST radar observations of atmospheric waves over mountainous areas: a review, A*nn. Geophys, 18, 750-756, 2000) and internal gravity waves in general. Therefore, strong gravity wave disturbances as those suggested by McHugh et al. can be detected by EAR and the interpretation of the increase of V_b in terms of vertical air motions produced by waves can be tested.

   Figure 2 shows time-height cross-sections of Signal to Noise Ratio SNR (dB), Doppler variance $\sigma^2\,(m^2/s^2)$ and vertical air velocity W (m/s) obtained from the vertical beam for 10 consecutive days (15-24 December 2015) in the height range 13-20 km (at time and range resolutions of 3 min and 150 m, respectively). A very persistent layer of turbulence was observed around the tropopause, between the two temperature inversions (blue and red dots) as indicated by the morphology of the SNR pattern (top panel) and, more importantly, by the persistent enhancement of Doppler variance, signature of dynamic turbulence (middle panel). The remarkable persistence of this turbulent layer (more than 10 days) and the presence of the two temperature inversions

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[2] According to Figures 3-7, it seems to be larger than 5 m/s if we assume that the background (minimum) velocity is mainly due to the free lift.

[3] The figure will be corrected. In addition, the submitted figure showed half of the total balloon profiles only (by mistake)

[4] see Figure A1 (response to Prof. Drager) for the full profiles.

on both sides measured from balloons launched 450 km away from the radar site suggests that turbulence was produced over a very large horizontal extent and was observed at both locations. The average profile of Doppler variance $<\sigma^2>$(green curve, right side of Figure 1) shows a peak at the exact location of the persistent increase in V_B (15-16 km). It is thus an additional clue of the turbulent origin of the increase of V_B. In addition, the measurements of W (bottom panel of Figure 2) do not exhibit values larger than +/-0.5 m/s; the profile of W averaged over 1 day on 19 December (thick blue line, $\langle W_{EAR} \rangle$, right side of Fig.1) is associated with small standard deviations (thin horizontal blue lines) *indicating a weak wave activity*. Thus, the large increases in V_B of a few m/s around the tropopause may not be attributed to waves.

These conclusions apply to the present data set and do not necessarily fit McHugh et al.'s observations but we believe that turbulence effects only may be enough for interpreting most part of the ascent increases reported by McHugh et al. It is an alternative interpretation, not a decisive conclusion refuting wave disturbances in the conditions described by the authors.

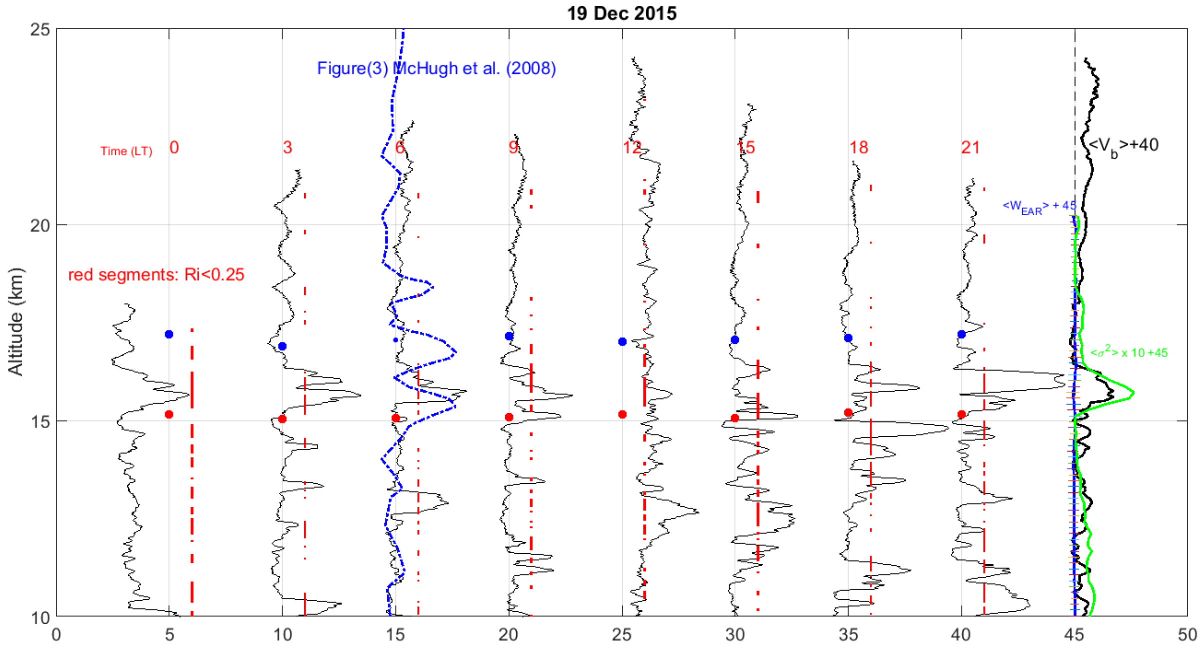

Figure 1. A series of 8 consecutive profiles of V_B obtained on 19 December 2019 at Bengkulu (Indonesia). (See text for more details).

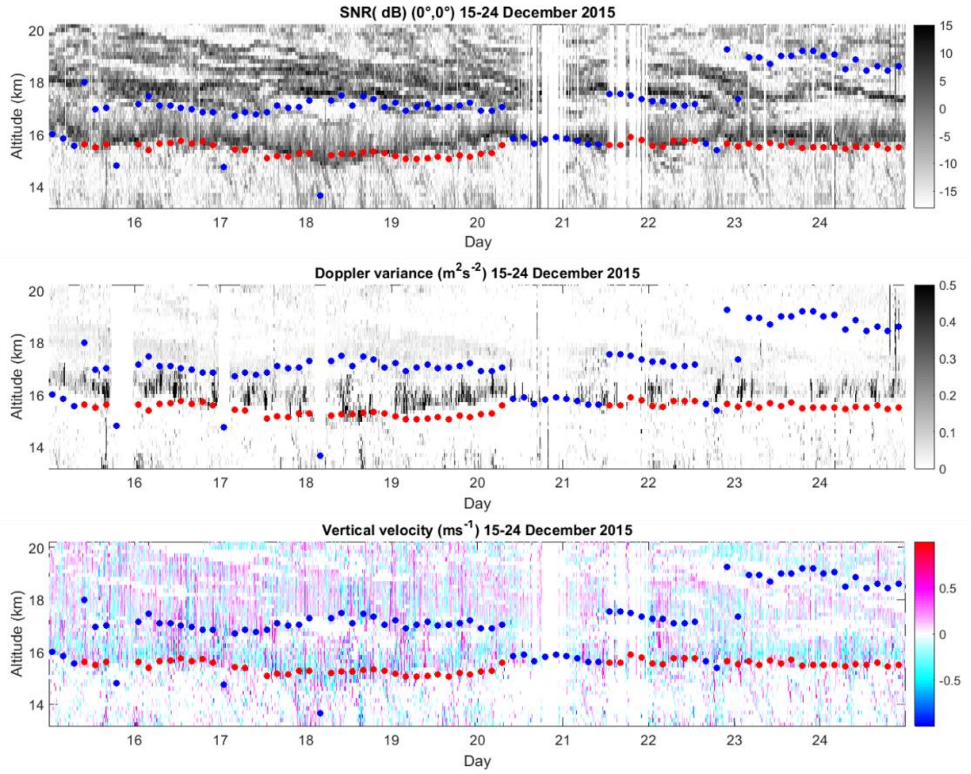

Figure 2: Equatorial Atmosphere Radar measurements of SNR at vertical incidence (top), Doppler variance (center) and vertical velocity (bottom)

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.

Yes, we agree, but the balloon ascent rate should be the result of multiple contributions of variable intensities (wave, convection, and turbulence effects).

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?

We believe that there are several factors that make the smooth transition around $Ri = 0.25$. Without being exhaustive, the dominant factors should be:

1) The Richardson number is a scale-dependent parameter, i.e. it depends on the vertical resolution of temperature and wind profiles. A coarser resolution leads to even smoother distribution.
2) $Ri < 1/4$ is a necessary condition for active turbulence, but turbulence can be sustained up to $Ri = 1$ (thus turbulence effects can be felt even for $Ri > 0.25$).
3) The Richardson number is defined as N_m^2/S^2 where S is the wind shear and N_m^2 is the square of the moist BV frequency, when air is saturated. Contrary to N^2 from dry air, there are various expressions of N_m^2 based on different models and hypotheses on hydrometeor effects. In particular, the Kirschaum and Durran (1994) model used in the present work does not consider the presence of condensed particles but should be more relevant than the BV frequency

calculated from the equivalent potential temperature. Therefore, slight biases on N_m^2 , and thus, on Ri can be expected.

Additional data quality controls (other than made by manufacturer) have not been applied to the data for this statistical analysis. Even if the balloon launches have been performed in accordance with the manufacturer's recommendations, contaminations by balloon wake, especially when wind shear is weak, cannot be excluded. In addition, unwinder problems are not uncommon: the rope length between the balloon and payload may be smaller than the recommended length (30 m) for some flights. This problem increases the risk of wake contaminations and introduces uncertainty in wind shear altitude. These effects may contribute to incorrect estimations of Ri.

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 ?]

'averaged values of V_{Bc} in *Ri* value bands of 0.25 in width' has been replaced by:

'mean values of V_{Bc} averaged over *Ri* segments of 0.25 in width from *Ri*=-2 to *Ri*=9.75'

4. Why is Figure 10 rotated by 90 degrees when compared to figure 9?

Figure 10 is now plotted ad Figure 9 and Figure 10b has been removed (because not informative)

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.

Thick vertical lines have been added for legibility of the figures. The legends have also been removed for clarity and a description of the curves has been added in the figure captions.