Reply to Prof. A. Drager and Prof. P. Marinescu

Reply to: Comment by Aryeh Drager (Colorado State University, aryeh.drager@colostate.edu) with contributions from Peter Marinescu (Colorado State University, peter.marinescu@colostate.edu)

We thank Prof. Drager and Prof. Marinescu for their interest to the present work and for carefully reading our manuscript. We think that our main conclusion is not very different from their point of view. There is a general agreement, based on the analysis described in section 3, that balloon ascent rates can increase due to the decrease of the balloon drag coefficient by turbulence. We somewhat differ on the strength of this effect. In particular, they suggest that the statistics shown in section 4 are not dominated by turbulence but by deep convection (i.e. the positive disturbances in ascent rates for low Richardson numbers are mainly the signature of vertical updrafts in convective cells). Without providing irrefutable evidence, we propose here additional clues and arguments suggesting that turbulence effects may rather be dominant in most cases. We hope that this information will open further discussions and investigations.

Summary:

This manuscript adds to a growing body of literature on the retrieval of vertical air velocity using balloon-borne, GPS-equipped radiosonde ascent rates. One unique aspect of this study is that it attempts to provide a robust independent validation of the radiosonde-derived vertical air velocity in the form of radar-derived vertical velocity from a vertically pointing middle and upper (MU) atmosphere radar. The focus of this study, however, is the effect of atmospheric turbulence on ascent rates. Atmospheric turbulence is assessed using both the MU radar and Unmanned Aerial Vehicles (UAVs). The authors' interpretation of the data is that the effects of turbulence on balloon ascent rates are comparable in magnitude to the effects of actual vertical air motions. The authors therefore conclude if the amount of turbulence and/or the turbulence's effects on ascent rates are unknown, then it is impossible to retrieve the atmospheric vertical velocity from the radiosonde ascent rate.

The authors also present a separate analysis of the flights of several hundred balloon-borne radiosondes from a recent field campaign. Rather than measuring atmospheric turbulence directly, the authors use the moist Richardson number (Ri)—calculated from the temperature, humidity, and horizontal wind measurements from the radiosondes—as a proxy for the likely amount of turbulence. The authors show that low Ri in the troposphere, which is associated with greater turbulence, is associated with greater (~+0.5-0.9 m s−1) balloon ascent rates. They therefore infer that the balloon ascent rates in the troposphere are affected significantly by turbulence.

Reply: The range of greater ascent rates ($\sim +0.5-0.9\ m\ s^{-1}$) mentioned above refers to the mean values shown in Figure 9c. In the submitted manuscript, this range was assumed to be "statistically representative of the turbulence effects" (line 243). However, it is somewhat incorrect and we removed the sentence. Indeed, the mean value $\langle V_{Bc} \rangle$ of ascent rates should include all contributions (waves, convection, large scale billows and decrease of the drag coefficient by turbulence). For example, if we assume that turbulence effects produce a mean increase of X (> 0) m/s and that all other contributions can be equally positive or negative (so that the mean increase due to these contributions is 0)[1], then the total mean increase will be less than X m/s. Therefore, we believe that ascent rate increase due to turbulence effects may be significantly larger than +0.5-0.9 m/s on some occasions.

From comparisons with vertical velocities measured by MU radar (section 3), we reported ascent rate increases in stratified and clear air conditions larger than $+0.5 - 0.9\ m/s$: namely, ~1-1.5 m/s (Figs. 5 and 6) and ~2 m/s (Fig. 7). Very importantly, these values were obtained with underinflated balloons (V_z in still air was estimated to be $\approx$1.8 m/s and 2.3 m/s, respectively). Referring to Gallice et al. (2010) and references therein, the drag coefficient can vary by a factor ~4 for the expected range of Reynolds number so that ascent rate can increase by a factor ~2, i.e. the ascent rate disturbance can be as large as the value of V_z in still air, but not more. Thus, for standard balloon inflation (V_z~5 m/s, as is the case in section 4), the disturbance can theoretically reach ~5 m/s. The detailed reply #4 to Prof. McHugh (reviewer #2) describes cases for which values up to ~ 4 m/s are plausible (Fig. 2. See also figure A1 below).

The sort of multi-platform analysis provided by this manuscript is sorely needed in the balloon derived vertical air velocity literature, and therefore, I appreciate the authors' time and effort towards such a study. However, after a careful reading of the manuscript, I take issue with the authors' interpretation of the data, and therefore with their main conclusions. My opinion is that the data are ambiguous as to whether turbulence or vertical air motions are affecting the balloon ascent rates. These concerns are outlined below.

Main major comments:

• *One of the manuscript's key conclusions (stated in, e.g., line 22 and line 255) is that vertical air velocity W cannot be estimated using the ascent rate of meteorological balloons in the presence of turbulence. This absolute statement fails to consider situations, such as turbulent updrafts and downdrafts in deep convective storms, in which vertical motions are so strong (~tens of meters per second) that the error attributable to turbulence (seemingly ~a few meters per second) may become unimportant. The conclusion that it is "impossible" to estimate W from balloons in the presence of turbulence is therefore at best (see next major comment below) only applicable to the comparatively weak vertical air motions examined in the present study. The authors should rethink this conclusion and should, at minimum, clearly state under which atmospheric conditions their conclusions are relevant (e.g., weak versus intense vertical air motions).*

Reply: To the authors' knowledge, vertical motions of several tens of m/s are extremely rare even if they are sometimes sources of aviation hazards. However, we quite agree with the remainder of the comment. Turbulence effects do not necessarily prevent us to detect vertical air motions from balloon ascent rates, especially if these vertical motions are very strong.

Lines 70-73, last sentence of paragraph 4 Introduction, have been modified as follows: "This alternative purpose seems to be more achievable than retrieving *W*, except at stratospheric heights and during very calm tropospheric conditions, as shown by earlier studies, and likely during deep convective storms during which strong vertical motions are expected. "

[1] It is obviously not the case, in particular if upward motion dominates due to convection. This hypothesis is made for a simple description.

The interpretation of the balloon ascent rate is indeed always ambiguous (as rightly mentioned below by the reviewer) except when the vertical wind disturbance significantly exceeds the disturbance expected from the sole turbulence effects (see the previous reply and reply #4 to reviewer 2). In principle, the *absolute* error in vertical air motion W does not depend on W[2] and the *relative* error decreases with W. If turbulence produces and absolute error of +2 m/s when W=+2 m/s and W=+20 m/s, then the relative error is 100 % (apparent W=+4 m/s) and 10% (apparent W=+22 m/s), respectively. The importance of the turbulence effects depends on whether absolute or relative errors must be considered for given applications.

• *One major reservation I have regarding the analyses presented in this paper is the horizontal displacement of the balloon relative to the locations of the UAV and MU radar. These displacements of >10 km are hardly negligible. How do we know that there are not major horizontal inhomogeneities in the turbulence and vertical velocity that are leading to the observed discrepancies between the radiosonde and UAV/MU radar observations?*

Reply: A long horizontal distance between the instruments (and a large time lag between the measurements) can be a cause of uncertainties. This is a perennial problem we tried to minimize in the present work by providing a variety of information from UAVs and radar. It must be noted that the horizontal displacement of the balloons did exceed 10 km when comparing with MU radar data above the altitude of ~5 km, but not in the range of comparisons with UAV data (the horizontal distance was less than ~10 km for the 3 cases, see Figure 1).

We have several arguments indicating that the radiosondes crossed the turbulent layers detected by UAVs and the MU radar.

(1) From a general point of view, turbulent layers of $10^2 - 10^3$ m in depth can have a horizontal extent exceeding $10^1 - 10^3$ km in stratified conditions, likely because they are usually associated with meso- or synoptic scale sources. There is no extensive literature focusing on this specific topic but earlier observations suggest this feature (e.g. Luce, H., R. Wilson, F. Dalaudier, H. Hashiguchi, N. Nishi, Y. Shibagaki, *Study of tropospheric turbulence from radar observations and radiosonde data using Thorpe analysis*, Radio Sci., 49, 1106-1123, 2014).
According to the MU radar observations, all the layers identified by T1, T2, …, KHI, MCT (Figs 2-4) persisted for at least 1 hour or even much more. Assuming a wind advection (~5-10 m/s in the present case), their horizontal extent should have exceeded ~30 km, i.e. the maximum horizontal distance between the balloons and radar/UAVs within the range of comparisons (Fig. 1).

(2) The comparisons between TKE dissipation rate profiles (Figs. 5-7) give extra-credence to the hypothesis that the 3 instruments detected the same turbulent layers. These profiles estimated from radar data *at the time of the balloon flights* and UAV data show reasonable agreements in shape and levels (Figs 5-7), indicating that UAVs detected turbulent events of intensities similar to those detected by the MU radar in the same altitude range and at the time of the balloon measurements despite a time lag up to about 1 hour for V16 (see Fig. 3).

The detection of the same turbulent layers by all the sensors is a necessary condition but not sufficient. On some occasions, and especially in clouds, we agree that the horizontal inhomogeneity in the vertical velocity field within a turbulent layer may potentially explain differences between W measurements. However, this hypothesis is hardly defensible from a

[2] This assertion may not be true. For example, we do not consider the response time of the balloon to the disturbances it faces. It is thus at best an approximation.

statistical point of view. From the case studies (Figs 5-7) (and some others we did not show), vertical velocities from balloon ascent rates in the turbulent layers are systematically larger than W measured by the MU radar. This tendency is not consistent with a horizontal inhomogeneity of W because the reverse observation should also happen. In addition, if positive vertical air velocities of the order of those indicated by the peaks of V_{Bc} in Figs. 5-7 are, by chance, often detected by the balloon but not by the radar at the same time, they should occur at other times on a statistical basis. Time-height cross-sections of vertical velocities in Figs 2-4 do not show any positive disturbance corresponding to the levels of the peaks of V_{Bc} during the observation time (1-2 hours).

This is of particular concern given the presence of clouds. In general, the conditions that give rise to turbulence should also give rise to inhomogeneous vertical motion, so it is hardly surprising that W and VBc do not agree as well in more turbulent layers. I do not see how one can conclude that the balloon ascent rate is changing solely due to turbulence effects on the balloon rather than at least non-negligibly due to actual vertical air motions given the potential inhomogeneities. I therefore question the paper's main conclusion that turbulent effects dominate the vertical air motion effects on balloon ascent rate.

Reply: Houchi et al. (2015, p1810) reported a *broadening* of the ascent rate PDF attributed to turbulence. This broadening can indeed be due to local updrafts and downdrafts produced by the "largest" turbulent billows (i.e. of dimensions significantly larger than the balloon size). The ascent rate disturbances should be equally positive or negative because they are outside the mechanism of deep convection causing stronger updrafts. On some occasions, during past field campaigns, we experienced such situations with underinflated balloons: balloons were forced to "sink" due to strong downdrafts in the vicinity of intense turbulent layers. Vertical advection by turbulent billows can thus be the cause of the broadening of the scatter plots for Ri<0.25 (Figs. 9-10), in addition to KH waves. This point is now clarified in the manuscript (as required by reviewer 1).

• *In a similar vein, I question the conclusions drawn from the statistical analysis of 376 balloon-borne radiosondes launched in Indonesia during a recent field project. The main result for this analysis is shown in Figure 9c, in which tropospheric radiosonde ascent rates are greater for low Ri than for high Ri by ~+0.5-0.9 m s−1. with a transition near the theoretical critical Ri value of 0.25. Since lower Ri is associated with enhanced atmospheric turbulence, the authors conclude that the greater values of tropospheric radiosonde ascent rate for low Ri are due to turbulence.*

(intermediate reply to the last sentence above): (a) We conclude that turbulence should be the dominant factor on many occasions (see below), but we agree that it cannot be the unique contribution when convection does occur. (b) This conclusion appears quite peremptory outside its context but it is based on the results obtained in section 3: when Ri was low, turbulence was directly observed and was found to be responsible for greater values of ascent rate owing to comparisons with radar data. In section 4, turbulence is not directly detectable and collocated radar data are not available[3]. A low Ri value (<0.25) is used a proxy of turbulence. Without the results described in section 3, attributing greater values of ascent rate to turbulence effects in section 4 would be largely speculative.

[3] Radar data are shown later in this document but they were not collocated. They are provided for a statistical analysis only.

My concern here is that lower Ri also ought to be associated with stronger vertical air motions, and these motions ought to have a net positive average vertical velocity given that these strong vertical air motions are likely to be associated with convective clouds, and updrafts in tropical convective clouds are stronger than downdrafts (e.g., LeMone and Zipser, 1980).

Reply: The above comments seem to combine two distinct aspects: (1) enhanced turbulence when Ri is low (2) strong updrafts associated with convection.

The range $0 < Ri < 0.25$ characterizes dynamic shear instabilities in a statically stable background and convective instabilities are associated with $Ri < 0$ ($N^2 < 0$). The largest positive disturbances occur when Ri is positive, indicating that they are rather associated with turbulence produced by shear flow instabilities. A more detailed discussion with additional data is given below.

Bengkulu, Indonesia's location near or within the ITCZ, combined with its coastal location (which creates susceptibility for sea breeze convection), seem to have made it a locus for the formation of convective clouds during the observing time period of November/December 2015 (NASA Worldview). Therefore, an alternative interpretation of the presented data is that the enhanced radiosonde ascent rates for low Ri are due to vertical air motions induced by convective clouds rather than due to turbulence.

We do not think that it is the most probable hypothesis for the following reasons:

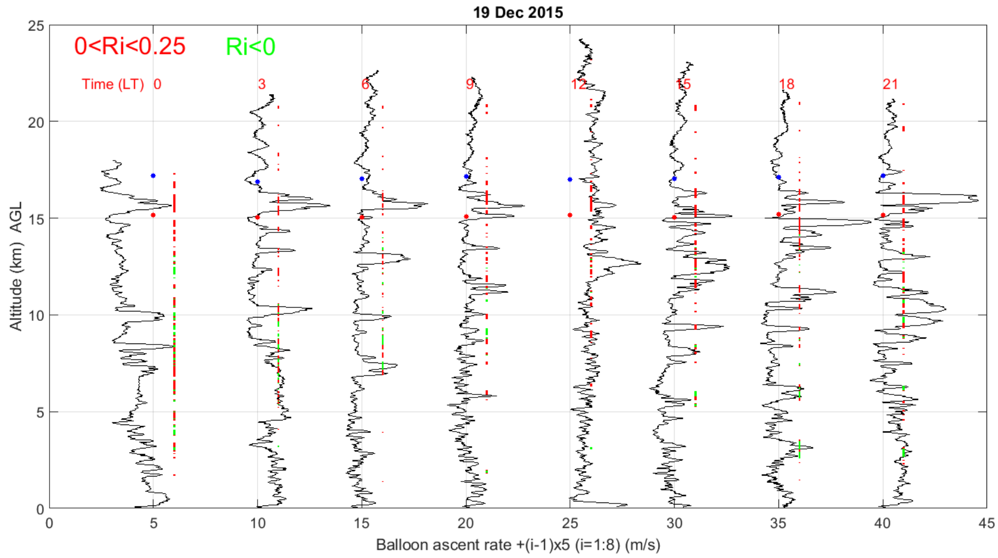

Figure A1. A series of 8 consecutive profiles of V_B obtained on 19 December 2015 at Bengkulu.

1. Morphology of the ascent rate profiles

A striking feature (<u>common to each observation day</u>) is the presence of narrow peaks of V_B in the troposphere (Fig A1). They are generally associated with low values of Ri (sometimes negative). In the altitude range 15-17 km (tropopause layer), the peaks are almost systematic and coincide with an enhanced wind shear (and low Ri) persisting for more than 10 days (not shown). Below 15 km, they randomly occur at various heights from one flight to the other (separated by 3 hours). Their interpretation in terms of updrafts produced by deep convection is unlikely: (1) Ri is not low in the lower troposphere. (2) Assuming that the balloons are embedded within large scale convective cells and are horizontally advected by the background wind, narrow spike structures can hardly be produced[4].

2. Statistical comparisons with radar data

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. Despite the very large distance between the EAR and balloon launching sites, we think that the radar observations can help interpret the balloon ascent rates in a statistical sense, assuming that these observations are statistically representative of the conditions met at Bengkulu.

(1) The time-height cross-section of Signal to Noise Ratios (SNR) at vertical incidence from 15 December to 24 December 2015 (10 days) generally show a layered structure, not consistent with strong convection (Fig A2). This feature is consistent with the characteristics of the temperature and humidity profiles measured at Bengkulu: Figure A3 shows the skew-T log-P diagrams for the flights shown in Figure A1 (19 December). The tropopause level was around 150 hPa. The troposphere was close to saturation or even likely saturated (with respect to ice) on some occasions in the upper troposphere but also showed a significant stratification in humidity (large variations of the dew point temperature with height). These humidity gradients, expected to be horizontally stratified, should be the main cause of the stratified echoes. More uniform echoes associated with deep convection can sometimes be observed in the afternoons.

(2) These (short) periods of convection are usually associated with enhanced Doppler variances (i.e. enhanced turbulence) (Fig. A4). Sporadic patches of enhanced variances can be observed in the whole troposphere, consistent with local decreases of Ri in the balloon profiles.

(3) The vertical velocity W measured by EAR does not show significant disturbances (up to ~+/-0.5 m/s), except during convection periods (but not more than +/-2 m/s). The disturbances are either positive or negative (Fig A5).

(4) The histogram of W values shown in Fig A5 (Fig A6 top panel) is not positively skewed, contrary to the histogram of V_{Bc} (Fig A6 bottom panel). The absence of skewness is not consistent with the interpretation of the large positive values of V_B in terms of (real) updrafts associated with deep convection.

(NB:The other days from 08 Nov to 14 Dec show similar features (the radar echoes were sometimes contaminated by rain echoes, producing negative outliers of W)).

This information does not support the hypothesis that the balloon ascent rate disturbances are dominated by updrafts associated with deep convection and is more consistent with turbulence effects.

---

[4] In addition, horizontal layers of upwelling are also not physically acceptable and the shape of peaks clearly differs from the disturbances produced by waves above the altitude of 17 km. All these arguments are rather in favor of turbulence effects.

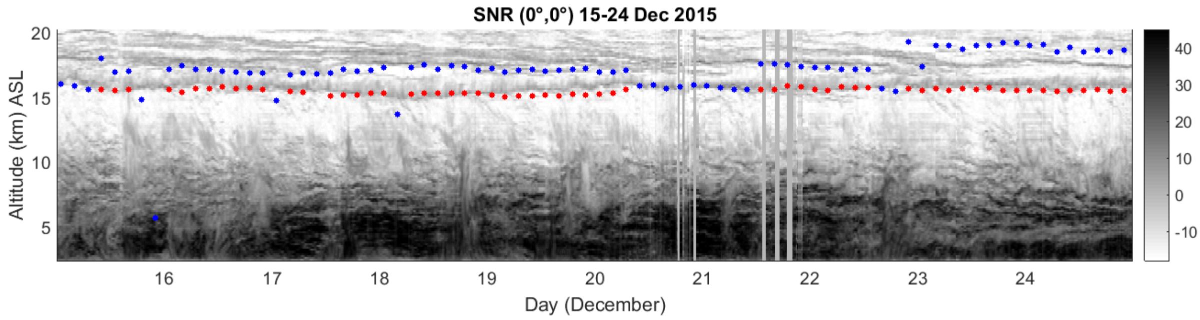

Figure A2: Time-height cross-section of Signal to Noise Ratio at vertical incidence measured by EAR. The blue and red dots show two temperature inversions associated with the tropopause (blue dots: cold point tropopause, red dots: secondary inversion at lower altitudes) measured by the radiosondes at Bengkulu (450 km away from EAR). A long-lived turbulent layer was detected by EAR between the two inversions (see also Fig A3).

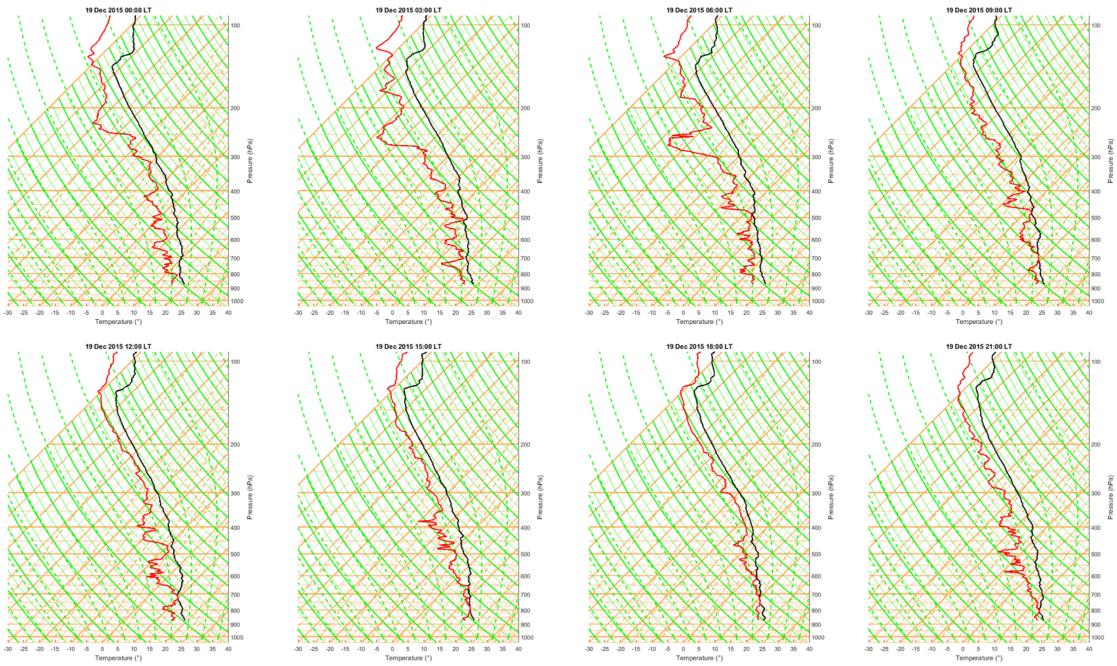

Figure A3: skew-T lop-P diagram for the 8 flights shown in Fig A1. (Black: temperature, red: dew point temperature)

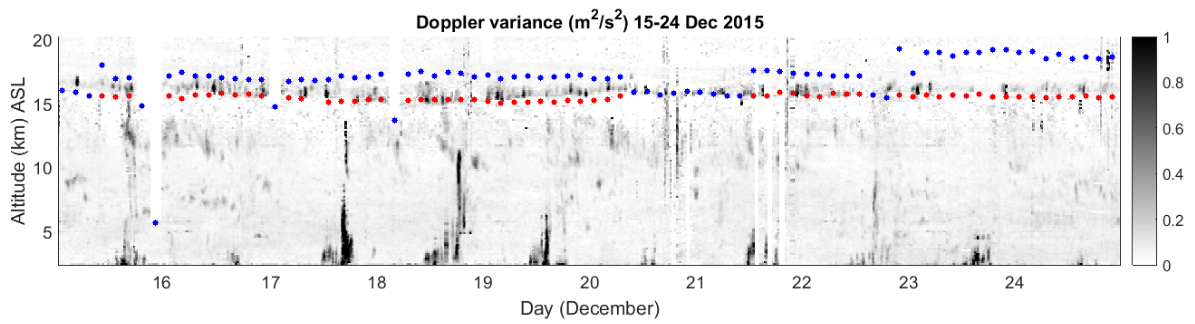

Figure A4: Time-height cross-section of Doppler variance produced by turbulence measured by EAR. The Doppler variance is generally used as a proxy of dynamic turbulence.

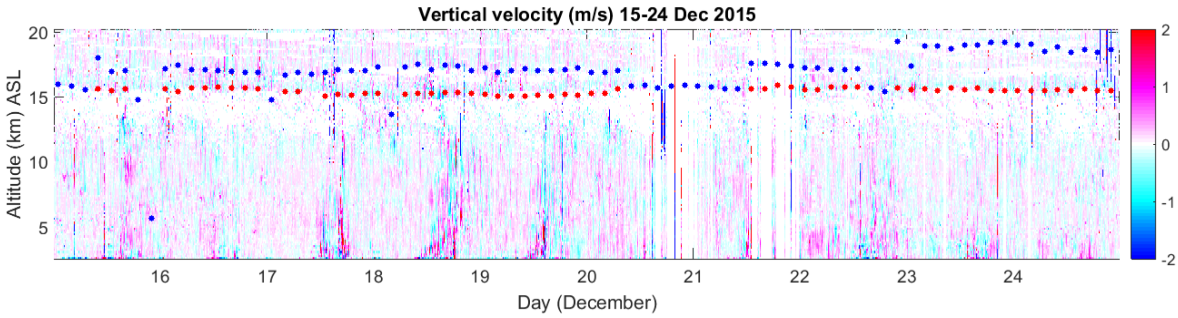

Figure A5: Time-height cross-section of Doppler velocity (m/s) measured by EAR at vertical incidence.

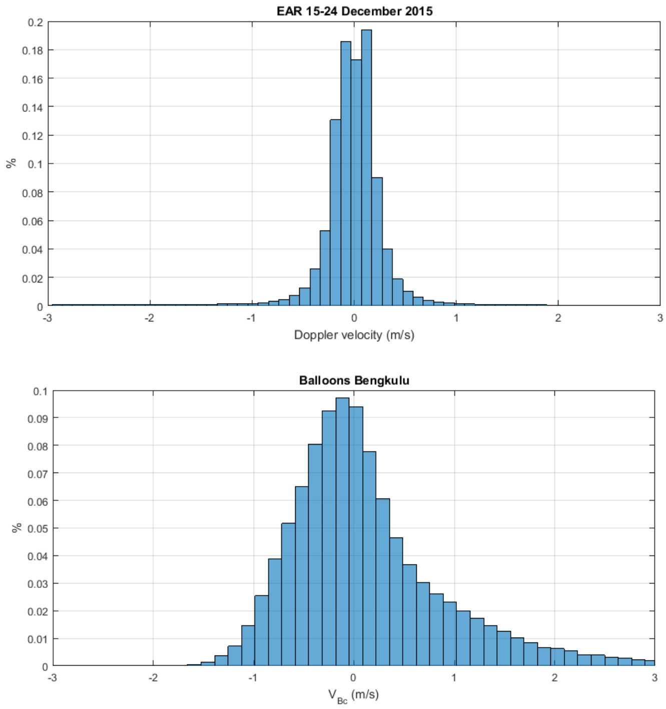

Figure A6: Histogram of vertical velocities measured by EAR (top) (15-24 December) and V_{Bc} (all 376 flights) (bottom).

3. Analysis of another balloon dataset at mid-latitudes

We performed an analysis similar to the one carried out in section 4 based on 59 balloon flights made in September 2011 at Shigaraki MU observatory, Japan, (i.e. at mid latitude where deep convection can be excluded). For various reasons (mainly few data in the stratosphere), it was not possible to estimate V_z for each flight accurately so that the profiles have not been detrended. Despite a smaller amount of balloons, the tendency is similar to the one shown in Figure 9 of the manuscript, i.e. an increase of V_B for low Ri. Therefore, we believe that the results shown in section 4 are "universal" and are not specific to the equatorial region.

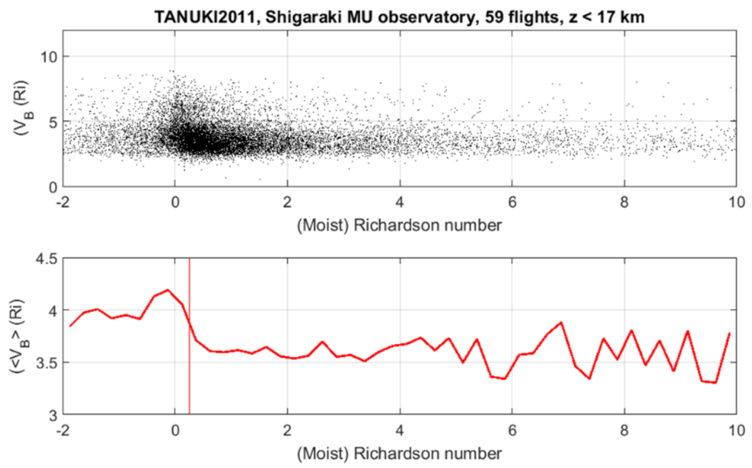

Figure A6: results similar to Figure 9 of the manuscript using 59 balloon flights performed in September 2011 at Shigaraki MU observatory (i.e. at mid-latitudes) (see text for more details).

References: 1) LeMone, M. A., and E. J. Zipser, 1980: Cumulonimbus vertical velocity events in GATE. Part I: Diameter, intensity and mass flux. J. Atmos. Sci., 37, 2444–2457, https://doi.org/10.1175/1520-0469(1980)037<2444:CVVEIG>2.0.CO;2. 2) https://worldview.earthdata.nasa.gov/?v=84.93837017059609,13.306520778973313,118.54774517059609,3.0235573460266867&t=2015-11-29T18%3A26%3A21Z

Other substantive comments:

• *Regarding line 82 and 274: To the best of my knowledge, the Gallice et al. (2011) method does not assume any particular value of Tu. Rather, this method obtains drag curves (drag coefficient as a function of Reynolds number) based on smoothed observed ascent rates and thermodynamic conditions across several launches. From page 2239 of Gallice et al. (2011): "To compensate for this lack of knowledge, and since parameters other than Re, Tu and E – such as unsteadiness or turbulence intensity length scale – are also known to affect the drag coefficient (e.g. Wang et al., 2009; Neve, 1986), an attempt is made here to derive a mean experimental drag curve for sounding balloons, based on a dataset of balloon flights."*

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%*." (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.

• *Line 173: You seem to be assuming that Vz is height-invariant. What is the basis for this assumption? You seem to make the same assumption later in the paper as well, in line ~223. Given the many factors that influence a rising, expanding balloon, it is unclear to me why Vz should be assumed to have a single value throughout the troposphere and lower stratosphere.*

The examples of balloon ascent rates shown in Figure A1, indicate that assuming Vz as a constant is a reasonable approximation.

• *Line ~177-178: Shouldn't using a lag equal to a multiple of the wave's period produce essentially the same result as the non-lagged profile since you are sampling at the same phase each time? I think that using lags equal to multiples of perhaps one-eighth of the wave's period would better produce the variety that you are looking to achieve here. I can think of two additional problems with using a multiple of the wave's period: (1) if the wave propagating relative to the mean flow, then there will be a Doppler effect that produces the wrong apparent period at a given point, and (2) it takes the balloon time to ascend through a given layer – therefore, the phase of the wave it samples at the bottom of the layer will differ from the phase being sampled at the top of the layer.*

We were not able to identify with certainty the type of waves associated with these nearly-monochromatic W fluctuations revealed by the radar images. In any case, the period is only apparent (it is not the intrinsic period if there is a Doppler effect). We have some indications that they can be some kind of ducted waves according to analyses we made from other data sets in similar conditions. We did not expend the analysis because it is not related to the main topic of the manuscript.