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, 2019, AMTD

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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.

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).

- 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? 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 V_{Bc} 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.

- 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. 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). 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.

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=2015-11-29-T18%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."*

- Line 173: You seem to be assuming that V_z 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 V_z should be assumed to have a single value throughout the troposphere and lower stratosphere.

- 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.