Report #3

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

First, I would like to thank the authors for the thorough response to my comments. In the interest of full transparency, I should clarify that I am merely a graduate student, not a professor.

While preparing my initial comments on the discussion paper, I compiled an extensive list of technical errors and minor suggestions. I ultimately chose not to include this list in order to avoid distracting from the main points Peter and I were trying to make. However, given that the manuscript is nearing publication, it seems appropriate to include this list now. I will include this in a separate document.

Main thoughts:

Perhaps one source of disagreement between myself and the authors pertains to the distinction between turbulence and motions of interest. This seems to be a question of scale: clearly an "updraft" that is much smaller than the balloon should be categorized as turbulence, and clearly a coherent vertical motion 100 km across whose depth spans the entire troposphere (if such a motion is even possible!) should be considered to be a motion that radiosondes ought to be able to measure. But how small must a motion be in order to be considered turbulence? This is perhaps a naïve question, but I think it might be crucial for reconciling my perspective with the authors' perspective.

Detailed comments:

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)[5], 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.

I do not follow why the total mean increase would be less than X m/s. If I add two random numbers, the mean of the distribution of their sum equals the sum of the means of their distributions, yes? Is that not the principle operating here?

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 ≈ 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).

Although Figure 3 of Gallice et al. (2011) does indeed show a factor of ~4 variation in drag coefficient across the entire range of Reynolds numbers considered, it is important to consider that Reynolds number is, for our purposes, a strong function of altitude (due to the relation of both air density and balloon radius with altitude). The Gallice et al. (2011) paper states:

> *In the case of a sounding balloon, whose typical effective radius is of the order of 1 m at ground and mean ascent rate of the order of 5 m s^{-1}, the Reynolds number decreases from ~8–9 × 10^5 at ground to ~6–9 × 10^4 at 30 km altitude.*

My point here is that at any given height, the full range of Reynolds numbers is not realistically attainable, and therefore the full factor of ~4 range in drag coefficient may not be attainable either. For instance, if we look at Gallice et al.'s (2011) experimental drag curves (their Figure 3), the range of c_D for Re = 10^5 is ~0.55 to ~0.85, which corresponds to a factor of ~1.55 range in drag coefficient. This corresponds to a factor of ~1.24 increase in ascent rate, much smaller than the factor of ~2 presented above. The ~1.24 value is likely an underestimate of possible turbulence impacts on ascent rate because it neglects the possible variation in Reynolds number at a given altitude, but nevertheless this analysis implies that the estimated factor of ~2 increase in ascent rate may be too large.

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. "

Our group at Colorado State University has recently conducted a field campaign whose goal, in part, was to measure the strength of vertical motions in supercell storms by launching radiosondes into the storms. So, while vertical motions of tens of m/s are indeed "extremely rare," they are of great interest to us, and they can readily be observed with radiosondes when launches are targeted.

In any case, I am happy with the change that was made to the introduction. I would also like to see a change to the wording of the abstract to remove the word 'impossible.' I suggest the following wording (lines 21–23):

> The presence of turbulence complicates the estimation of W, and misinterpretations of V_B fluctuations can be made if localized turbulence effects are ignored.

• 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).

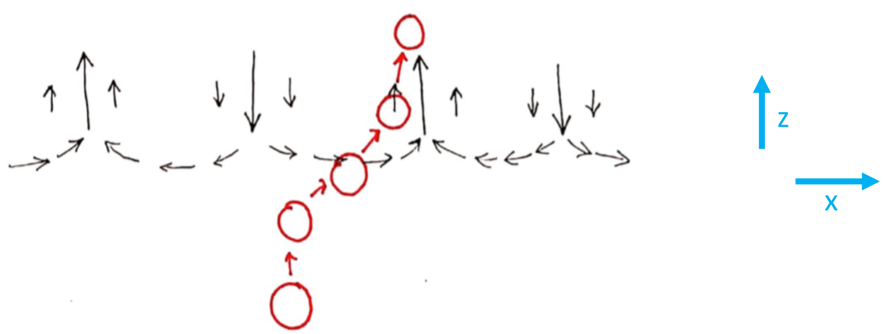
This makes sense.

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 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).

My understanding of what the authors are saying above is as follows:

The balloons are consistently experiencing greater ascent rates than would be expected based on the MU radar vertical velocity retrieval. If the discrepancy between MU radar-derived vertical velocities and balloon-derived vertical velocities were indeed due to horizontal inhomogeneities in the vertical velocity field, then the balloons would be just as likely to experience slower ascent rates (due to localized downdrafts) as they would greater ascent rates (due to localized updrafts). Furthermore, the MU radar-derived local vertical velocities are too weak in general to explain the extent to which the balloon ascent rates are enhanced.

This argument makes a lot of sense. However, I would like to propose an alternative explanation for the observed results:



In the above [very idealized, two-dimensional, not-to-scale] sketch, the black arrows represent winds, and the red circles with red arrows represent the path of a balloon. The bases of downdrafts are associated with horizontal divergence, and the bases of updrafts are associated with horizontal convergence. These regions of horizontal convergence and divergence are required by mass continuity and should occur in three dimensions just as easily as in two dimensions. As the balloon ascends toward the base of a downdraft, the horizontal wind steers it into an adjacent updraft. Therefore, the balloon enters the updraft instead of entering the downdraft, even though it was initially closer in horizontal position to the downdraft!

I will grant that this argument is rather simplistic and does not consider the balloon's own inertia and buoyancy, which prevent it from being a perfect Lagrangian tracer. This argument also does not consider changes of the flow field over time. Nevertheless, my point is that it is *possible* that horizontal winds play a role in steering balloons into the updrafts rather than into the downdrafts, causing the downdrafts to be undersampled and thus underrepresented in the statistics.

In principle, it might be possible to test this hypothesis by releasing multiple balloons with different characteristic ascent rates (via different amounts of inflation). The balloons with the greatest characteristic ascent rates should experience the least susceptibility to horizontal steering and should thus be more likely to sample downdrafts.

My argument here does not address the fact that the MU radar did not observe updrafts strong enough to explain the observed balloon ascent rates.