Review on 'On the quality of RS41 radiosonde descent data' by Bruce Ingleby et al., (ATM-2021-183).

This paper presents detailed analysis carried out to test the quality of the descent data obtained from RS41 radiosonde over several stations from Europe. Initially they compared between ascent and descent profiles of all the meteorological variables (T, RH, U and V) and later with independent ECMWF short-range forecasts and also radio occultation profiles. Difference between ascent and descent profiles has been attributed majorly due to pendulum motion besides large terminal velocities. Finally, it was concluded that descent data is much smoother and can be used for data assimilation in NWP model, that can be obtained (descent) with NO additional cost. Overall, the manuscript is well written and sound enough both technically and scientifically. The main topic of research is worth investigating and fits well within the scope of Atmospheric Measurement Techniques (AMT) Journal. However, there are several aspects that remain unclear at this stage and I recommend for its acceptance only after taking care of the following major and minor comments/suggestions.

Major comments/suggestions:

1. Authors have missed one important aspect of estimating the descent rates using balance between gravity and drag forces by considering actual dimensions and weight of RS41. This has important consequences on the measurements of radiosonde during the descent. It does not matter whether a parachute is attached or not. It purely depends on the amount of weight left with the busted balloon. If more weight is left than the radiosonde weight, smooth profile can be expected.

2. Similar study was made long back by Venkat Ratnam et al., (2014) where they assed radiosonde descent data from a tropical region. Not much focus is given to this original research and mentions many places in the current manuscript in such a way that this analysis is made for the first time. Proper credit should be given to the original research at many places throughout the manuscript.

3. Initially as soon as the balloon is busted, the terminal (fall) velocity increases drastically and it takes some time to stabilize. Since density at those altitudes are very low, a busted balloon will have much higher velocities and as density increases at lower altitudes it slows down. Thus, previous works have recommended to use data from 5-10 km below from the busted height. I strongly feel the same thing will be valid in the current works also. Authors should explicitly discuss this aspect.

4. When the busted balloon weight is high, it will not allow the radiosonde to drift freely with the background wind. Instead it drags the radiosonde. In this case, whether wind velocities measured by the radiosonde during its descent will be realisable? In this case note that wind velocities are not from a freely floating body. Same problem may arise even for temperature and humidity when the response time is not good enough. When a busted balloon is at high velocities (close to 80-100 m/s, in Figure 5,6 and 7), whether any sensor (Particularly T and RH) in the radiosonde is capable of sensing the background atmosphere? I suggest providing details on these aspects in the manuscript.

5. Unable to see big difference in T (Figure 14 and 15) and RH (Figures 18 and 19) between ascent and descent profiles over any country. Then the question arises what will be its impact in NWP if assimilated? Again later, I am surprised to see the effect of assimilation of all the descent data you have seen here (Figure 27). I am unable to see a big difference In T and RH between ascent and decent profiles. Then from where this effect has come?

6. I suggest authors show the diurnal variation in all the meteorological variables first using ECMWF data before making any conclusive statement (Lines 381-385 also lines 389-391.). If the variation is much higher than the expected diurnal variation, then only one should talk about bias. Diurnal variation is too small over higher latitudes hence big difference is not expected in the ascent and descent profiles. However, the situation will be completely different over tropical or equatorial latitudes. In fact, similar exercise needs to be done over low latitudes before making any conclusive statements.

Specific comments/suggestions:

1. Lines 28-29. This was first reported by Venkat Ratnam et al., (2014).

2. Lines 40-41. As soon as the balloon bursts, it will come much faster, similar to the release of a satellite from a rocket. It cannot be much faster whether one uses a parachute or not. It is to be noted that the role of the parachute comes only after stabilising that may take about 5-10 km below from the busted altitude.

3. Lines 50-51. It is rightly mentioned that a balloon will be advected horizontally by the background wind and typically travels 50-300 km…In fact drift depends on the background winds and will be different in regions of the world. Since most of the stations used in this manuscript fall under mid-latitudes, balloon drift mostly depends on the wind speed of sub-tropical jets. I suggest plotting the balloon drift something similar to Figure 3 and 4 of Venkat Ratnam et al., (2014) season wise and discuss the same at relevant places.

4. From Figure 2 it is not clear out of how many profiles, those descent profiles were obtained.

5. Lines 83-84. Upper part of the descent is close to the upper part of the ascent in both time and space. Then what about the role of large fall velocities?

6. Lines 108-111. It is mentioned that ‘On ascent the sensor boom projects above the radiosonde, so that it samples air that has not flowed over the body of the radiosonde. On descent, with a working parachute, it should be in a similar position - so it may sample air that has flowed over the radiosonde body, which has the potential to introduce biases or contamination. It is not known how a radiosonde descending without a parachute is orientated, or if it may be tumbling.’ I do not fully agree with these statements. The orientation of the radiosonde fully depends on the weight of the busted balloon. If the weight is much higher than the weight of the radiosonde, it is quite common to think that the radiosonde will be dragged with the busted balloon coming down first.

7. Lines 123-125. Again, it depends on the weight of the busted balloon?

8. Lines 151-152. Again, it depends on the weight of the busted balloon? This reason is already suggested by Venkat Ratnam et al., (2014).

9. Figure 7. It looks like there is a latitudinal effect on the descent rates that is shown in figure. Can you check the effect of background temperatures/densities at those latitudes?

10. Lines 196-197. It also has strong seasonal variation. I suggest plotting the maximum drifts at all the stations to make a conclusive statement.

11. Lines 229-230. It depends on the weight of the left-out balloon after the burst.

12. Figures 9b and 10b. It is not the vertical velocity. Vertical velocity has a different meaning in meteorology. Suggest replacing it with 'descent speed' or something similar.

13. 259-260. I do not fully agree with this statement. It depends on the left-out balloon weight after it busted and does not depend on whether the parachute is attached or not.

14. Line 269. Your ECMWF forecast is at 9 km horizontal grid spacing. How is the comparison done when the balloon drift is too high (>300 km) which changes with the altitude?

15. Lines 283-285. It is mentioned that 'One surprise was that the descent profiles (in red) fit the background more closely than the ascent profiles (in black), particularly at upper levels. Comparing individual ascent/descent profiles the descent winds generally appear smoother and this appears to be the cause of the better fit to background.' First of all, I am unable to see smoother profiles as mentioned. It can be due to descent rate being not affected while using GPS satellites which move much faster. Also, both time (within a few minutes) and space (sensing the same background) is very low, so no variability can be noticed. Descent profiles look much smoother because it is not freely floating but dragged instead? I suggest providing details on each of these aspects in the revised version.

16. Line 310. Cool by 0.5^{o}C. I suppose this is within the uncertainty of measurement?

17. Line 314-315. (also Table 2). I do not understand what do mean in these lines. Note that RO is limb technique and there will be a maximum difference of 300 km from the top of the profile (~40 km) to the surface. The statement 'the RO data is much closer to the ascent temperatures then the descent temperature' is not correct. It depends on whether you are considering rising or setting occultation. You should consider the mean latitude and longitude of the total profile in RO data and then check whether it is within the limits of selection criteria (within 1^{0} or 100 km).

18. Lines 331-332. In the previous section, you have mentioned that the time and space difference at upper levels will be minimum so that a good comparison is seen. But in these lines, you are mentioning that the large top-level ascent-descent difference has disappeared by 300 hPa which is in contradiction. Be specific here. Further, offset of 0.2 or 0.3^{o}, is it not this is within the uncertainty of the measurement? If not, what about temporal variation? Have you checked whether this offset is positive or negative at other

timings? I suppose it becomes negative if we consider evening profiles rather than morning profiles.

19. In Figures 15 and 16. I suppose the large difference in T at higher descent rates may be due to sensor problems (Unable to sense that fast?).

20. Lines 362-364. These statements may not be true if you make a comparison during day and night times. Once should consider expected diurnal variation before making such statements.

21. Line 365-366. I think the bias can also be due to sensor response. In one second, radiosonde travelled almost 100 m in which background temperature may not be the same. We need a high response sensor to check this.

22. Lines 493-494. Even if someone wants to assimilate, you have already reported higher differences in all the parameters due to large descent speeds?

23. Line 502-503. Very late these references (check the spelling of Venkat) have appeared in this paper. I suggest giving proper credit to the original works being carried on this topic. These references should be very well discussed in the introduction section itself. Further the statement ' ..latter did not discuss the cause……' is not correct. It was mentioned very clearly that due to large descent rates, sensors do not respond that quickly to make meaningful measurements. Further, one cannot combine these two references. Note that the latter reference already attributed differences due to the diurnal variation. Several reasons are mentioned in the Venkat Ratnam et al., (2014) and I suggest authors to go through the paper very carefully before making such statements.

24. Lines 517-518. I do not agree with these statements. Note that descent winds are not from free floating of the balloon but dragged due to acceleration due to gravity in addition to the background wind.

25. Lines 519-520. One cannot think of having accurate measurement of wind during descent as it is dragged by acceleration due to gravity that depends on left out busted balloon and weight of radiosonde in addition to the background wind.

26. Line 521. Here references are needed for those studies. Also, I guess, those studies have been neglected due to known reasons (dragged winds rather than free floating along with background).

27. Lines 525-527. Can one think of having a good quality balloon (almost sphere rather than oval after filling the gas) to make the balloon stable?

28. Line 537. I doubt the descent winds for the reasons mentioned above.

29. Lines 539-540. I strongly suggest having a GRUAN descent product for the RS41 (for that matter any radiosonde type) as one more profile is being obtained at no cost. This is very

useful particularly launched in the tropical altitudes as the expected diurnal variation is very high.

30. Line 542. Same holds good even for T. Check the response time of the temperature sensor at higher descent speeds.

31. Line 546. I suggest providing a reference for this statement. Also, I do not agree that using parachutes will give some improvement.

32. I suggest to smooth the data with 100 m to take out the random motion of the balloon unless there is a need for very high vertical resolution. Further, 1s sampling particularly at higher altitudes (lower densities) may not be sufficient to sample the background atmosphere. I do not think that any NWP model requires parameters with very high vertical resolution for assimilation.

References:

Venkat Ratnam, M., N. Pravallika, S. Ravindra Babu, G. Basha, M. Pramitha, and B. V. Krishna Murthy (2014), Assessment of GPS radiosonde descent data, Atmos. Meas. Tech., 7, 1011–1025.

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