Authors' response

RC1. Review on 'On the quality of RS41 radiosonde descent data' by Bruce Ingleby et al., (AMT-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.

**Thank you. On the issue of cost, there is a minor cost from keeping the ground station operating during the descent. If there are changes specifically for the descent data (adding a parachute, a pressure sensor or an additional receiving station) the cost is increased. Hence, we prefer to say that the additional cost is small (rather than zero).**

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.

**The descent rate is important, we make that clear. We don't understand "It does not matter whether a parachute is attached or not." because a parachute does slow the descent. We now mention the descent rate estimates of Venkat Ratnam et al., (2014), but because of the unknowns (balloon mass present, orientation) such estimates seem to give an upper bound for the descent rate (figure 6 of VR14). Because we have the measured descent rates having such an estimate does not add much.**

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.

**We now discuss the two previous studies in the introduction and provide more details of Venkat Ratnam et al., (2014) [VR14] in particular, we also added a mention in section 2.4.**

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.

**At the end of VR14 it says "Thus, the data within a few kilometers from the balloon burst may be biased because of improper sampling." (which we now quote). Rather than restrictions based on height relative to balloon burst it seems better to use the fall rate (but this is not currently available within the ECMWF NWP system, so a simple pressure limit is used). At the end of the paper we have added "Descent data should be used with caution and sections with high descent rates particularly so, however different users have different tolerances and we expect that improved processing will increase the proportion of usable data."**

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.

**It is unclear if the reviewer is thinking of the balloon remains as a compact ball or as causing drag like a parachute (the latter seems more likely). Regarding the winds we mention "an 'inertial' correction for the delayed response to horizontal wind shear (Appendix of Hock and Franklin, 1999)" – not currently applied in MW41 processing. Stratospheric radiosonde humidities are not assimilated anyway. We have added notes on temperature sensor response times to section 3.3.**

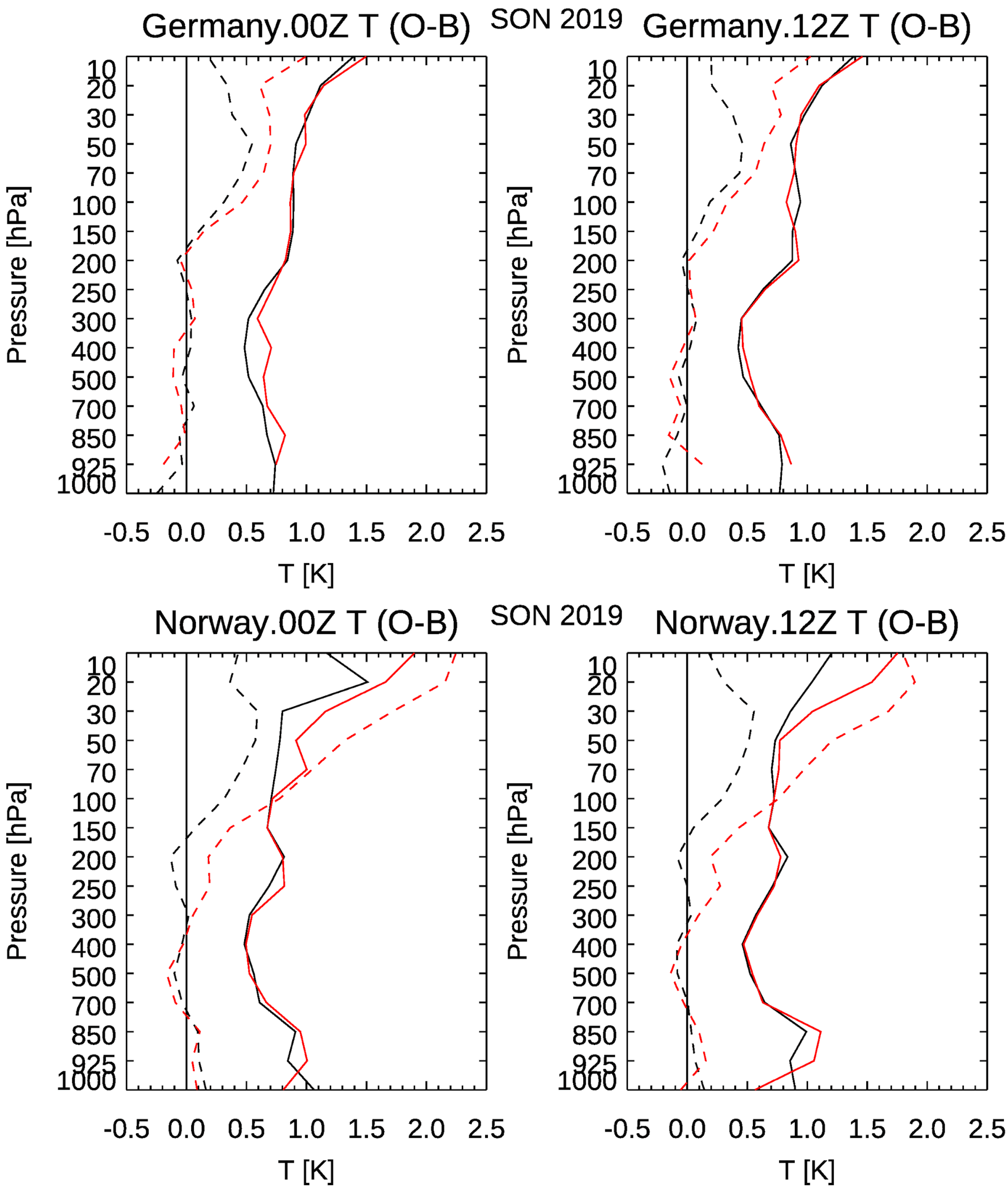
5. Unable to see big difference in T (Figure 14 and 15) and RH (Figures 18 and 19) between ascent and descent profiles over any country. Then the question arises what will be its impact in NWP if assimilated? Again later, I am surprised to see the effect of assimilation of all the descent data you have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

**One of the main points of Figure 14 and 18 is that (apart from temperature biases) the descent and ascent quality are similar – so we have more usable profiles. Because the ascent and descent positions/times differ they have different background (B) profiles.**

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.

**In the ECMWF O-B statistics the B values take the (background) diurnal variation into account whereas in section 4 the Praha ascent-descent statistics (those discussed in lines 381-391) will sample the diurnal cycle. Figures 16 and 17 show some ECMWF O-B statistics by time of day (colour coding). In practice ECMWF O-B statistics show no clear diurnal variation (example below). We agree that it would be good to see similar results for low latitudes, but do not expect tropical results to be "completely different".**

**I have looked at figures like the one below - showing 00 and 12 UTC statistics separately for Germany and Norway, note that some points have small samples - and concluded that there is no clear diurnal cycle in the O-B statistics. It is normal to briefly report a result like this rather than present lots of figures.**




**Specific comments/suggestions:**

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

**Venkat Ratnam et al., (2014) is now mentioned in the introduction.**

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.

**Figures 9 and 10 plus S4 and S5 suggest that sometimes the parachute opens suddenly after falling fast and sometimes it seems moderately effective immediately after balloon burst.**

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.

**Given that the paper is already long (noted by reviewer 3) and that the manuscript is not about balloon drift per se we do not propose to add any more figures on drift – an indication is given in Figure 1.**

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

**I think the question is about the ratio of the number of ascent profiles to descent profiles. The last sentence of the paragraph has been modified to read 'Some ascents, usually less than 5%, do not have a corresponding descent report, …'.**

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?

**Large fall velocities are dealt with later in sections 3 and 4.**

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.

**This seem to assume that the balloon remains form a compact (heavy) ball, but it is also plausible that they form strips – waving above and slowing the descent (see 8 below). Arguably figure 6 of VR14 suggests that remains trailing above are more common than a ball below. It seems reasonable to say simply that we do not know the orientation.**

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

**These lines about pendulum motion are primarily about the ascent.**

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).

**Added 'Venkat Ratnam et al. (2014) suggested that the balloon remains sometimes act as a parachute.'**

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?

**As we say 'The Norwegian radiosondes fall faster than those from the other countries studied - it is unclear why they fall faster than the Finnish radiosondes.' We tried asking radiosonde experts**

**from these countries but no clear explanation was available (it could relate to the type of balloon used, or whether the radiosondes were 'hardshell' or the newer, lighter 'softshell' type). We think this is a question for a later study – ideally with some tropical data.**

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.

**Not in the scope of this study.**

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

**The main difference between the fall rates in figures 9 and 10 appears to be due to the parachute, which we mention.**

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.

**Changed to 'descent rate' for consistency.**

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.

**The statement is: "The motion on descent may be more consistent if the radiosonde could be 'cut free' of the balloon remains and fall on its own without a parachute." We think this is very likely (if the balloon remains are small and there is no parachute then cutting the string would make little difference). The balloon remains are mentioned at various points in the text.**

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?

**This is described a few lines further down ('and in this study' added as clarification): "Afterwards, and in this study, ascent profiles were split into sub-profiles of 15-minutes each and treated as valid at the time and latitude/longitude of the first point in the sub-profile. Descent profiles are split into 5-minute sub-profiles for comparison with the model."**

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.

**Start of next sentence changed to 'This is illustrated in Figure 12 which …'. I have added plots of four ascent/descent pairs from Lindenberg to the supplement to provide examples.**

16. Line 310. Cool by 0.5°C. I suppose this is within the uncertainty of measurement?

**In the text: "at about 50 hPa, in the extratropics, the ECMWF background is too cool by about 0.5°C (this can be seen against the RS41 ascent data in Figure 14)."  There is other evidence to support this - see Shepherd et al (2018) referenced in the next sentence.**

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 10 or 100 km).

**These sentences have been rewritten: "More recent work on the analysis system has approximately halved the short-range forecast bias (Laloyaux et al., 2020); they included comparison against radio occultation (RO) retrievals. We compared radiosonde ascent/descent pairs with RO retrievals that were within 100 km and 2 hours of the burst point. The RO data is much closer to the ascent temperatures than the descent temperatures - note that the sample size is much smaller than for the O-B statistics."**

**The RO data comes with a single (average) location and we are comparing with the burst location (relevant for upper levels; the 300 km in the comment seems to refer to drift from the launch point, it should be clearer now that we mention the burst point).**

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°, 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.

**As stated above, because the ascent and descent positions/times differ they have different background (B) profiles and the B values include (the model version of) the diurnal cycle. The upper level bias is the 'direct effect of heating', radiosondes without pressure sensors (Finland, UK) suffer from the 'indirect effect of heating' in the troposphere. As we say these issues are 'discussed in more detail in the next section'. [Example 00Z and 12Z statistics are shown above.]**

**A difference of 0.2° would be within the uncertainty of an individual measurement. Here we are looking at an average over many profiles and a crucial but difficult issue is whether/how much the errors are correlated between different profiles/days/stations. Assuming that the errors are partly random the uncertainty in a long-term average will be smaller than that in an individual profile.**

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?).

**Notes on the response time of the temperature sensor have been added and a reference to the Vaisala RS41 white paper. A time lag in the sensor can't cause the systematic difference in the stratosphere, as there is almost no vertical gradient there over the three-month average. We also checked and found that implementing a 1-3 s shift in the profile makes the results worse, not better.**

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.

**As stated above (response to major comment 6), the diurnal variation was examined.**

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.

**See response to 19.**

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?

**The text was rewritten and simplified: "Upper-level winds were used in the trial, but because of concerns about accuracy when the radiosonde is falling fast all descent data with pressure less than 150 hPa are excluded in the current operational system."**

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.

**We agree that it was rather late to mention the previous work and they are now discussed at greater length in the introduction – after rereading Venkat Ratnam et al., (2014). The spelling is corrected.**

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.

**See next response.**

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.

**We disagree. The force of gravity is vertical so does not directly affect the horizontal motion of the radiosonde. During the ascent the radiosonde is pulled upwards by the buoyancy of the balloon (also gravity). If the radiosonde is falling fast then there will be a time lag before it reacts to a change in the horizontal wind – we mention this here using the shorthand 'inertial effects', see section 1 and the Appendix of Hock and Franklin.**

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).

**We wrote "Most studies of radiosondes concentrate on the temperatures and humidities and the winds are somewhat neglected". To take one example, the section on winds in Dirksen et al (2014) is much shorter than the sections on temperature and humidity. Temperature and humidity are seen as higher priority for climate studies, they are more comparable with (most) satellite data, they are also more subject to biases than winds are (radar winds can have a direction bias). I think some authors see radiosonde winds as a 'solved problem', but pendulum motion deserves more attention. I think it would be overkill to give a long list of references at this point.**

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?

**I think the closest is the Jimsphere (which also has roughness elements), but I suspect that it wouldn't go as high in the stratosphere.**

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

**See response to point 26.**

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.

**We would like to see a GRUAN descent product, and this paper is a step in that direction. Resource and priority issues mean that it would take several years.**

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

**See response on point 19.**

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

**Figure 14 now referenced. Compare the German and Norwegian results (or UK and Finnish) to see the benefits of parachutes.**

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.

**In general, the data should be smoothed as little as possible leaving users to decide what processing best suits their application. NWP models do not need 1s sampling (but the vertical resolution of NWP models will improve in future). Some studies, including those of gravity waves, benefit from high vertical resolution (see Geller et al, 2021, and references).**

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.

**Already referenced.**