

Atmos. Meas. Tech. Discuss., referee comment RC3
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Comment on amt-2021-183

Anonymous Referee #3

Referee comment on "On the quality of RS41 radiosonde descent data" by Bruce Ingleby et al., Atmos. Meas. Tech. Discuss., <https://doi.org/10.5194/amt-2021-183-RC3>, 2021

Review of manuscript by Ingleby et al., "On the quality of RS41 radiosonde descent data", submitted to Atmospheric Measurement Techniques

This paper presents interesting results describing the quality of RS41 radiosonde measurements made during descent after balloon burst, with and without a parachute. One finding that should be repeatedly stated in the manuscript is the reduction in positive temperature biases during descent when a parachute is used. An attempt is made to develop and apply a correction to these biases, based on descent rates, that includes descents with and without parachutes, but it seems like the very wide dynamic range of descent rates may require two correction equations - one for very fast descent rates (no parachute) and one for slower descent rates (parachute).

I had hoped for some deeper and more quantitative discussion of how bias-corrected RS41 descent data help improve NWP forecasts, but the paper is already quite long. I was also hoping for more results and discussion of ascent/descent RH comparisons, even though these are restricted to the troposphere and are therefore made more difficult by the high variability of RH below the tropopause.

In general, there are too many figures (27) in this manuscript, making it a lengthy and arduous read. Perhaps photographs of bursting (Fig 3) and burst (Fig 4) balloons can be moved to the supplement? I have also indicated in my comments where I think some figures can be eliminated.

My specific comments are:

Table 1: For me, numerical station codes are meaningless unless they are further identified by location in the caption. It would be interesting to know which stations launch

the larger balloons, and why.

Figure 7: This would be an excellent place to show the variability of descent rates in each country. A horizontal error bar (± 1 std deviation) on each mean profile at 2-3 altitudes would be illustrative. Slightly offsetting the error bars in the vertical will allow them all to be viewed with clarity.

Line 183: When I calculate the reciprocal of 14.9 seconds I get 0.07 Hz (not 0.06 Hz).

Line 184: This sentence tends to imply that the increase (decrease) in period (± 0.6 s) is linear with an increase (decrease) in tether length, which it is not.

Line 228: Here I think "ascent" should be changed to "descent"

Line 239: The errors introduced into descent wind data are largely dependent on the vertical (temporal) averaging window. Errors can be made very small by using an averaging window that is similar to the period of the pendulum motion. If higher resolution wind data are required, the magnitudes of the errors become quite dependent on the averaging window.

Figure 11 and Line 283: To me, the "fit the background more closely" in line 283 implies that the mean O-B differences for descent in Figure 11 should be smaller than those for ascent, particularly at upper altitudes, which they are not. I think your statement in line 283 is instead referring to the variability of O-B differences, not the mean differences. "More closely" makes me think of reduced bias, not reduced variability. Perhaps "fit the background with less variability" is a better description of what is shown in Figure 11. Is less variability or smaller biases in O-B differences more important overall? I would think smaller biases.

Line 289: What is the typical (range?) of averaging windows used to reduce pendulum effects on wind data? Do these change from flight to flight? For a given flight are the averaging windows different for ascent and descent?

Figure 12 and Line 294: The low vertical resolution in Figure 12 makes it difficult to distinguish between pendulum motion and noise. Can you instead display some sections of the profiles at higher vertical resolution so the pendulum motion becomes more obvious?

Line 297: Does "more pendulum motion" imply a higher frequency or a larger amplitude of

oscillations? Which has the greater impact on the standard deviations of O-B averages?

Table 2: Are these statistics for all radiosonde sites, a subset of sites, or a single site? I assume "Sample" is the sample size for either RS-RO or RS-B observations, but don't know which. Adding the sample sizes for the observations not represented by "Sample" would be helpful (and will show your point about the smaller number of RS-RO differences).

Figures 14 and 15: I don't see the need for Figure 15 when Figure 14 clearly shows the warm temperature biases of the descent profiles. Lines 325-326 are adequate to explain why a simple time lag argument for the biases is not adequate.

Line 331: "by 300 hPa" is confusing. Less confusing is "below 300 hPa".

Figures 18 and 19: I don't see the need to have a Figure each for specific and relative humidity differences when the radiosondes directly measure relative humidity. Also, specific humidity calculated from relative humidity measurements requires pressure and temperature data that can themselves produce biases in specific humidity values. It's difficult to argue that 2 separate figures are needed to support 4 sentences in the text.

Line 377: Why wasn't the Prague site included in Table 1 and Figure 7? Here, it just comes out of the blue as a site with many useful descent soundings.

Line 378: What is a "comparable level"?

Line 381: Here, the change in writing style (and presumably authorship) is obvious. The lead author may want to reconstruct some of the sentences to flow as the previous text did and to help with clarity in this section.

Line 384: I have no idea what this means: "was expected warm bias for 06 and 12 terms, and cold bias for 00 term due to the diurnal variation.

Line 387: I presume "dividing the sample into groups of 1000 m" refers to the binning of differences by altitude using 1000 m-thick layers?

Line 390: Does "separating data into 00, 06 and 12 UTC groups" indicate that the Prague

site launches 4x per day at 00, 06, 12 and 18 UTC? Why not simply say that" And say that the data from soundings performed at each of the four launch times were analyzed separately? If launches were performed at 18 UTC, why are those data not shown or discussed? I assume it is because of the large difference reduction in near-surface temperatures between 18 and 20 UTC, but this needs to be stated.

Figure 21: Which launch time is this for?

Line 399: If not from a quadratic fit of the data in Figure 20, how was the "best estimate" determined? What is the uncertainty of A?

Line 401: "the root mean square ΔT is lowered from 1.22 °C to 1.06 °C, indicating that the correction explains 24.4 % of the variance seen". How does the reduction in RMS explain 24.4% of the variance? Isn't that determined from a correlation coefficient?

Table 3: Each coefficient needs an uncertainty estimate or their statistical significance remains unknown. "Best estimate" still needs to be explained since it appears to not be based on a standard parametric fit.

Table 4: What are "compared levels: 527 779" ?

Line 431: "the exact value of the correction coefficient is slightly uncertain." Are you suggesting that the correction should be the same for descents with and without a parachute? I don't understand how that is expected, since descent rates without a parachute are typically much higher than those with a parachute. If you divide the data into types: high descent rates typical of no parachute descents and lower descent rates (parachute) and independently fit each type, can you improve the quadratic fits and lower the uncertainties in "A" for each case?

Line 436: "method is used for processing of the data from RS41-SGP radiosondes" - what method are you referring to and how is it used to process the data? The equation is only used to calculate geopotential height.

Line 438: In order to calculate the pressure profile the geometric altitudes from GPS must be converted to geopotential heights. It is very important to mention this.

Line 439: A radiosonde doesn't "estimate" anything, it measures descent temperatures with a positive bias.

Line 443: "the shift of height still remains in the troposphere levels" - why would you expect it to go away? The effect is cumulative during the entire descent so the geopotential height biases continue to increase down to the surface.

Figure 23: It would be instructive to say that the vertical offsets between ascent and descent temperatures shown are for a RS41-SG and that the offsets are due to what is described in Lines 444-445.

Lines 452-453: The descriptions of curve colors in Figure 24 belong in the caption, not here in the text. The caption for Figure 24 needs some rewording, including a statement of the Figure being based on a single flight or many flights.

Line 454: "and the lines are almost the same". Wouldn't it be clearer to say that the biases and standard deviations in differences between ascent and descent temperature measurements are the same regardless of whether the profiles are aligned using height or pressure?

Line 474: Would the "positive effect" be the reduction in biases between ascent and corrected descent measurements?

Figure 26: The caption for Figure 26 needs some rewording.

Figure 27: The "% differences" units are misleading because they are not % differences. My initial thoughts were that the 100% line (control) was perfect agreement and deviations from this line represented positive and negative biases. Then the caption indicates that values <100% indicate improved forecasts. A clearer explanation is needed here to make the results shown in this figure more understandable.

Line 513: You might check with the US National Weather Service Field Support Laboratory in Sterling, VA, about publications arising from wind tunnel tests of radiosonde temperature sensors. They have decades of experience performing such tests in a wind tunnel environment.

Line 519: "have a closer RMS fit" - this is far from standard terminology. What is an "RMS fit"?

Line 528: "Jimsphere balloons" need to be described here

Line 529: If you have documented evidence that supports this statement please provide it here (along with the reference) otherwise this appears to be opinion rather than fact.

Line 546: "this will reduce with improved processing/bias correction" - it is not clear what "this" is.

Lines 552-554: The text inside the parenthesis needs to be re-worked, as it is not clear what is meant.

Line 557: Remove the space between "n" and "ow"