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Atmospheric Measurement Techniques
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Discussions

Interactive comment on "Assessment of GPS radiosonde descent data" *by* M. Venkat Ratnam et al.

Anonymous Referee #1

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general comments:

-a very nice study addressing directly the assumption in current radiosonde practice that descending data are redundant. It is clearly shown that processing descending data can have additional value, especially when studying rapid changes in the atmsphere in campains that have multiple balloon ascents per day.

specific comments:

-p.10365, line 4,5: resolution of the measurements is not equivalent with sampling interval. Therefore sampling interval of 1 s does not translate to a 5-6 m resolution in altitude, but rather to a 5-6 m sampling interval in altitude. For a proper interpretation of the data in terms of resolution it would be usefull to have an overview of the response

times of the different sensors (currently only a range of 0.12 to 10 s for the NTC thermistor is mentioned) This directly translates to the hysteresis effect seen in Fig. 9. If these numbers are not well known this should be clearly stated as well. In addition some discussion on the accuracy of the altitude measurement by the GPS system should be added as well, since this is directly relevant for comparison of ascending and descending data.

-p.10370, line 24: the use of the terms 'edge wise and no-edge wise' is not well defined here. From the later discussion this appears to be related to the dimensions of the radiosonde box itself. This should be clearly defined at the moment of first use of these terms.

-p.10371, line 6: here it is suggested that the higher drag may be caused by a tumbling behaviour of the radiosonde box. However, the falling system also still contains the ruptured balloon and the connecting line, so it could also be the case that the simple Cd estimate is just wrong (and only a lower estimate). This should be mentioned as well.

-p.10371, line 15: here the result is presented that the descent rate differs for different seasons. How significant is this? This actually is an important result in my view, since it may show that there is a relation between the drag coefficient Cd and atmospheric conditions. This is especially relevant for other studies that attempt to retriwve vertical wind motion from radiosonde movements. Or do I interpret this wrong, and is this caused by a systematic difference in maximum altitude reached in different seasons?

-p. 10371, line 18: here you mention that a 100 m smoothing is applied to remove errors arising due to random motion of the balloon. This puzzles me. What random movements are there that impact the instrument measurements? Do you have evidence of such mothions? If so please mention them. On line 12 of p.10372 you again mention a probably 'wild fluctuation in descent rate', which you attribute to tumbling. Please show the data that proves this, and discuss the accuracy of the GPS to show that this is a real effect. Maybe you are trying to remove other effects/errors as well, like digitisation errors in the signals, and in the GPS position readings? Of so, please state this clearly.

p.10374, line 25: the 'inadequate sensor response' mentioned here may indeed show up in the data. However, you could also take this as a source of information, and estimate the temperature sensor response from it for different atmospheric densities. Does this result in readings similar to the ones reported by the manufacturer?

p.10375, line 19: you state that the standard deviation of 2 K between descending and ascending 3 hours later is small compared to the ascending and descending data of the same sonde. However the differences mentioned in the previous section are 0.5, 1 and 2 K, depending on altitude, with a standard deviation of 4-6 K. This wording is somewhat confusing. In stead of saying 'Note that this mean difference and standard deviations are small when compared to the ascent and the descent data of the same sounding mentioned in the last sub-section.' you could better leave out 'mean difference' here and only say: 'Note that the standard deviations are small when compared to the ascent and the descent data of the same sounding mentioned in the last sub-section.'

technical corrections:

-p.10370, line 12: the reference to Fig. 5d seems a mistake. Should be 5c.

-p.10391, fig.5, panels 1 and b: what horizontal bin size is applied here? I guess it is 15 minutes? It would be better to state this explicitely in the caption.

-p.10392, fig. 6, panel b: I would suggest to also overplot the mean observed descent rate in this plot for comparison.

-p.10394, fig.8 which data is plotted here, the ascending or descending data? Or both? It would be nice to note here that this is interpolated data, but not between vertical lines. Could you overplot the actual time at which the different soundings reach a given

altitude? i.e. overplot them as tilted black lines? Or did you actually assign the data of each sounding to the moment of release? If this is the case, please mention this.

p.10395, fig. 9: It may be good to mention here that the upper left plot clearly shows the hysteresis effect in the temperature observation, and that the humidity measurements below -40 C so above about 12 km become unreliable and should be ignored in the upper right plot.

p.10396, fig. 10: you should mention here that subsequent soundings are shifted by 10 K to make them visible as separate lines. In addition, it may be usefull to add a 3rd panel showing them all overplotted without additional shift. This will show how similar or dissimilar they are. If they are very similar, you could also consider to create a mean temperature profile from these profiles and subtract this before plotting them. The will strongly enhance the visibility of the small features that you are referring to.

p.10397, fig. 11: I guess some temperature profiles are again shifted by 10 K here (based on my experience in the previous plot) but I am not sure. Please make this explicit.

Interactive comment on Atmos. Meas. Tech. Discuss., 6, 10361, 2013.