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Atmospheric Measurement Techniques Discussions

Interactive comment on "Comparison of Vaisala radiosondes RS41 and RS92 at the ARM Southern Great Plains Site" *by* M. P. Jensen et al.

Anonymous Referee #2

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Review of "Comparison of Vaisala Radiosondes RS41 and RS92 at the ARM Southern Great Plains Site" by Jensen et al.

The paper reports a new intercomparison study of the Vaisala RS92 and RS41 radiosondes at the mid latitude site of the Atmospheric Radiation Measurement (ARM) program site at Oklahoma. A study of this kind is needed to document change management between different radiosonde models and to evaluate the possible impact of this change on long term data sets. It is therefore of very high value.

The data analysis for some parameters seems to have some weaknesses, which must be remedied before the paper can be published. Since there are already a number of publications showing the overall agreement between these two radiosondes, greater care needs to be taken in this study, if it wants to present something new and not just

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repeat, what is already in the literature. I would expect that this takes more time than a typical major revision; therefore, I recommend rejection of this manuscript in its current form. At the same time I strongly encourage the authors to consider the comments detailed below and to resubmit a revised manuscript.

Major comments:

There seems to be a problem in the pressure measurements of the Vaisala RS92 and I suspect that ARM is not following proper procedure in preparing the pressure sensor of the RS92. Proper Vaisala RS92 ground check corrections require a correction of the radiosonde pressure using an independent surface pressure measurement. If this has been done correctly the pressure measurements at launch will be nearly identical to the reference pressure. This reference pressure is also used for the initialization of the Vaisala RS41 pressure and therefore the pressure measurements between the two radiosondes must almost by definition be in near agreement near the surface. This is not the case here. This shortcoming has already been recognized by the GRUAN processing of the ARM data, but apparently not been considered here. All RS92 soundings should be reprocessed using the proper ground check correction for pressure.

I am somewhat concerned about the statistics and the significance of the cloud related results. The authors define 8 could types and end up having mostly just two soundings per cloud type. One type only has one sounding. Furthermore, there are daytime and nighttime soundings, which will further reduce the number of soundings per cloud type and time of day. Only for daytime soundings would I expect a significant cloud influence on the comparison, thus, only daytime soundings should be used here. Since the differences are generally small, basing statements on a low number of soundings is not ideal. For example, Figure 12 shows very large differences in cc6 (two sondes total, one daytime, one nighttime) compared to the other cloud groups. I would speculate that a single comparison strongly influences this statistics. Likewise, the nighttime comparison of pressure seems to be significantly different than the daytime comparison, which is unexpected. I can speculate that here as well a single comparison influences the

AMTD
8, C3979–C3985, 2015

statistics. The statistics of the entire cloud discussion should either be improved or the entire discussion should be removed.

The conclusion and abstract state, that there will be no significant impacts in the switch from the RS92 to the RS41. This statement only applies to the currently used MW31 version 3.66, but not for the MW31 version 3.62, which has been used until 2011. The changes between these two Vaisala RS92 versions strongly impact long term climate data series, and therefore, this change must be evaluated as well. The authors must point out that there already is an inconsistency in the RS92 record, which has not been properly accounted for. The ARM radiosoundings are also part of the GRUAN network and these data have been processed by GRUAN using an independent processing system, which considers these inconsistencies. All ARM RS92 data have been or are scheduled to be reprocessed using this processing system. Therefore, the comparison between RS41 and RS92 should also refer to the GRUAN processed ARM data, which do not suffer from changes in processing version (3.62 to 3.66) or changes in sensors (coating of humidity sensors). A comparison to the GRUAN processing of the same data is required if the authors truly want to make a statement that there will not be significant impacts on the long term series in switching from the Vaisala RS92 to the Vaisala RS41. As is, their statement is somewhat misleading and only refers to the changes between Vaisala RS92 processing MW31 3.66 to Vaisala RS41 processing MW41 2.1, which is valid only for the last four years, but not for the entire ARM RS92 data record.

Minor comments:

Page 11326, lines 21-23: Please better describe the difference between the RS41 and RS92 humidity sensor. The expression ‘heating functionality’ is not clear, since both sondes use sensor heaters.

Page 11327, line 1: The authors should also note that the Vaisala ground software for the RS92 can be configured such that the GPS measurements are used for pressure

AMTD
8, C3979–C3985, 2015

calculations of the RS92. Although this may not be practices widely, it could lead to a similar performance of pressure determinations in the RS92 as the RS41.

Page 11327, lines 27-28: The ground check device of the RS41 only reconditions the RS41 humidity sensor comparable to the RS92 reconditioning, but no 0% ground check is performed, since no 0% RH reference is involved. This sentence should be deleted or modified accordingly.

Page 11328, lines 1-3: This statement is highly speculative. The operations have certainly been simplified and remove some chance for operator error, but recommending less training may lead to new additional operator errors compensating the gains of the new system. Time savings is probably minimal. These statements should be removed or substantiated with real data.

Page 11328, line 10: In 2011 Digicora version 3.64 introduced a new correction for time lag and solar radiation correction for humidity measurements. These corrections can be turned off; therefore it is important to specifically mention that they were used. Furthermore, this version has an updated solar radiation correction table and algorithm for the temperature measurement, which should be pointed out.

Page 11328, lines 23-24: The main ventilation term is due to the ascent rate. This does not change in the larger payload. This statement does not hold and should be deleted.

Page 11329, line 1: The abstract states a sounding period from June 3-8 not June 3-7.

Page 11331, line 27: This statement is somewhat generic and based only on two profile comparisons shown. Since a larger number of soundings passed through clouds can the authors substantiate this statement with a statistics of all their comparisons showing that the RS41 is 'less prone' to wet-bulbing? Can the authors provide information that the hydrophobic coating has changed to explain this improvement? I am somewhat concerned that there appears to be a 250 m separation between the cloud top and the when the radiosonde sensors see the cloud top. Is that an indication that the RS41

AMTD
8, C3979–C3985, 2015

may also suffer from wet-bulbing for something like 50 s? Or can the ARM Raman Lidar confirm that the moist layer extended significantly above the cloud?

Page 11332, lines 6-9: The wind measurements appear to be basically in agreement and the differences appear not to be statistically significant. The differences may be a result of filtering and the authors should make a statement, whether the (small) differences are statistically significant or not. If they are statistically insignificant, which I believe they are, then they shouldn't be called differences.

Page 11332, lines 8-10: These statements are speculative and may not hold up to scrutiny. Despite increase radiative heating, the processing for both radiosondes uses a correction for radiative heating. Difference in the correction algorithm though may contribute to the (very small) differences in temperature. Wet-bulbing at 10 km can be excluded, since cirrus clouds don't tend to lead to sensor icing. Sensor response time may be a reason, but again, the processing does account for time response issues, which therefore should not be an issue. The differences in humidity are significant and may also be caused by sensor calibration, which should be mentioned.

Page 11332, line 20: I cannot see a 'broader peak' between 8 and 10 km. Maybe a broad peak between 5 and 13, but the correlation between general cloud occurrence and the features shown in Figure 8 is weak at best. This should either be deleted or strengthened. As is, I would not argue that the behavior in and around clouds represent the largest differences between the sondes.

Page 11333, line 19: Better write '. . . noisier as the daytime differences.'

The comparison of temperature measurements is not a true comparison of atmospheric temperature measurements, but also a comparison of the radiation correction schemes used for the RS92 and RS41. It would be very helpful, if the authors could discuss the difference in radiation correction schemes and how they relate to the observed difference in temperature.

AMTD

8, C3979–C3985, 2015

Page 11336, line 8: The authors should not only make use of the other measurements at the site, but in particular of the different RS92 processing provided by GRUAN for ARM. Since ARM SGP is formally a site within GRUAN, all data are also processed by GRUAN and these data are available to the authors. It would be most useful to make use of this processing, since this processing is more self-consistent than the operational Vaisala processing.

Page 11336: The agreement between the two radiosondes is remarkably good; however, there appears to be a systematic dry bias of the microwave observations compared to the radiosondes. This is somewhat surprising, since the Yu et al., (2015) showed much better agreement between the radiosondes and the microwave at the ARM site at Manus. The authors should use the GRUAN data at SGP, which already provide uncertainties for RH, to estimate the uncertainty for PWV of their own data instead of referencing data from a tropical site with much higher PWV. Can they comment on the apparent dry bias of the microwave?

Figure 8: Pressure difference: The pressure difference near the surface seems to be off by about 0.5 hPa on average. This should not happen, if both the Vaisala RS92 and Vaisala RS41 were initialized using the same reference pressure (see major comment above).

Figure 11: The X-axis label should probably be 'Number of soundings', where the legend explains what is meant by this.

Figure 18: Since RH over liquid at ice saturation decreases with temperature, it would be better to show the RH difference as relative difference, not as absolute difference. It would be most enlightening, if the authors would also include the GRUAN processed data in this figure.

Figure 19: It would be better to show a PWV difference as function of PWV, rather than PWV as function of sonde number. The plot should also indicate daytime and nighttime comparisons.

AMTD
8, C3979–C3985, 2015

Technical comments:

Page 11324, line 25: Some important references seem to be missing here.

Page 11325, line 19: Delete 'first'

Page 11325, lines 20-21: Delete 'with new technical solution'

Reference Yu et al. appeared 2015, not 2014.

Table 3: The resolution of the humidity observations from the RS92 can also be set to 0.1%.

Figure 3: The unwinder string maximum (not minimum) is 30 m.

Figure 7: This is not a Skew-T plot, but rather a simple T log-P plot. Please explain the dotted lines. The figure legend refers to a 'parcel path', which doesn't make sense here and shows two dew point lines. I believe the figure tries to overlay both the RS41 and RS92 data. Since the differences are very small, the authors may just show one of the two. The explanation in the text for this figure is sufficient. CAPE and CIN need not be shown as part of the figure, since they are not discussed in the text and add little to the comparison.

Figure 15: Legend refers twice to panel c.

Figure 15: The median RH value cannot be 65.2%, if only integer values are used as shown. The median RH of the data shown in table 6 is 65.0%. The median temperature of the data shown is 26.2 C (not 26.5 C).

Figure 18: Lower left panel should have units in % instead of mb.

Pressure units should be written as hPa throughout, not as mb.

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 11323, 2015.