Atmos. Meas. Tech. Discuss., 7, C700–C708, 2014
www.atmos-meas-tech-discuss.net/7/C700/2014/

Atmospheric Measurement Techniques
Discussions

Interactive comment on "Reference quality upper-air measurements: GRUAN data processing for the Vaisala RS92 radiosonde" *by* R. J. Dirksen et al.

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Received and published: 28 April 2014

GENERAL COMMENT:

Typical radiosonde data are not suitable for long-term trend monitoring or other climate-related or satellite validation purposes. This paper describes the GRUAN program's specialized method of processing raw data from Vaisala RS92 radiosondes to make it sufficiently accurate and well-characterized to serve these purposes, mainly by applying corrections that minimize known systematic measurement errors and by rigorously estimating the uncertainty in the corrected data on a point-by-point basis. Good atten-

tion was paid to thoroughly considering uncertainties and traceability. The approach that was used allows for reprocessing of the raw data if/when superior corrections or uncertainty estimates are derived. Suggestions for improving the paper are relatively minor.

SPECIFIC COMMENTS:

- sec 3: Consider including a picture of an RS92, especially the sensor boom (or reference a picture in the 2011 WMO Intercomparison report).

- p3733, line 16: Miloshevich et al. (2009) is a better calibration error reference than the 2006 paper.

- p3734, line 10-11: This sentence needs some editing for clarity. Also, you are suggesting that the reconditioning portion of the ground check procedure is important to eliminate a 1-5% RH dry bias, which I agree is important but also implies that ALL ground check elements in section 3.1 are being recommended...is that true (including the RH recalibration adjustment based on the desiccant)? Also, it might help readers to better understand the purpose of the reconditioning by mentioning in Sect 3.1 that the contaminants produce a dry bias in the RH measurements.

same paragraph: It is a little confusing that both the GC and the additional GC are discussed in this same short paragraph, which makes it unclear as to what is being recommended.

line 12: "budget" –> "estimate"?

same paragraph: Can a correction based on SHC measurements at 100% RH really ever be made? Wouldn't it require knowing a lot of proprietary information about the Vaisala calibration function, which is NOT simply a linear function of RH that could be scaled based on a measurement at one point?

line 21: remove "and". "include" –> "including" p3735, line 9: It might be more clear to readers if it was mentioned in Sect. 3.1 that the RH GC correction is not applied for GRUAN soundings due to its very large uncertainty. Is it always necessary to reverse the RH GC correction, or is it possible to use Digicora settings to simply not apply it in the first place?

It's very important that readers be clear that it IS recommended to apply the sensor reconditioning (heating) but NOT the RH GC correction; otherwise some users might skip the reconditioning.

Would it be advisable to inform readers specifically how the Digicora settings must be adjusted from their default settings in order to not apply the Vaisala corrections in a manner consistent with GRUAN soundings?

p3736, line 19: Please indicate that this refers to "Vaisala-corrected" T measurements.

line 21: The wording of this sentence suggests that the satellite measurements are "correct" and the RS92 measurements have a 1 K bias. 1 K is a lot, and is inconsistent with the Luers, Vaisala, and WMO findings, and I would guess that the discrepancy could mostly be attributed to the satellite approach. Perhaps the sentence should be reworded, and if possible add some info about uncertainty in the satellite approach.

Fig. 5: suggest minor tick marks at 0.1 K intervals (rather than 0.125 K).

Sec 5.2.1: It would be helpful to see a photograph of this chamber.

p3738, line 18: "extend" –> "extent" (also p3744, line 6)

p3738, line 21: It appears from Fig. 2 that 20 s is most but not all of the way to the ultimate (relatively stable) T difference. Does this choice of 20 s represent a component of uncertainty? For example, why not use the average over a time period once the T has stabilized? Is the idea here that a time is chosen that fully includes the rapid rise but does not include much of the slow rise due to uncertainty over its relevance to actual operational conditions? More importantly, please make clear whether this selection of 20 s varies with P (presumably it is longer at lower P).

Eq (1) and related text: Just a curiosity question...is there any advantage in accuracy to be gained by instead characterizing dT in terms of several (I/P) relationships for different v values, and interpolating between the v curves? I'm wondering if each curve would have a tighter grouping of points in Fig. 3.

p3739: It is not entirely clear whether consideration is made for the difference between the solar spectrum at ground level in Lindenberg where the experiments were done and the modeled spectrum as a function of height and geographic location (assuming that the T sensor is sensitive to the spectrum). Also, what is the spectral response of the filter (does grey mean that it affects all wavelengths equally)?

p3742, sec 5.2.4: Can the magnitude of the nighttime cooling correction be characterized, and can anything be said about how the Vaisala correction was determined (pure modeling?)?

p3747, sec 5.7 and fig 9: Is it correct that there is statistical uncertainty in the mean differences (red curve) due to the sample size, given by the mean divided by sqrt(N)? If so, consider adding a red shaded region to Fig. 9 indicating this uncertainty. Also, why is only one sounding used to represent the modeled uncertainty, rather than the average of the actual GRUAN uncertainty estimate for the 29 soundings?

p3747, line 25: Consider adding "about" before -40C, because there is also time-lag error at warmer temperatures that may not be totally negligible (e.g., at -35C when the humidity gradient is very steep).

p3748, line 4: Regarding the convention to express humidity as %RH over liquid water, the Associate Editor specifically asked me to comment on its appropriateness. It is indeed appropriate, as you know, but I recommend adding a sentence or two for clarity, and possibly a reference to Miloshevich et al. (2006, Appendix A) where this issue is discussed at length. Some may be confused that RHw has no physical meaning below the homogeneous ice nucleation temperature of -35 to -40C where even tiny droplets freeze spontaneously. Nonetheless, there are many expressions for RHw, and the key point to make in the paper is that accurate conversion to other water vapor units such as vapor pressure, mixing ratio, or RH with respect to ice requires using the same saturation vapor pressure formulations as assumed by Vaisala in determining their RH calibration function, which are Wexler (1976) and Hyland and Wexler (1983). Errors from using other common SVP expressions are shown in Fig. A1 of the above reference. The recommendation to use Wexler (1976) was also made in the 2010 WMO Intercomparison report (Nash et al. 2011).

p3748, Sec 6.2: The choice to not apply the Vaisala GC correction in GRUAN processing is important and well-described here. The accuracy of RS92 soundings globally is fundamentally tied to how much care operators take in changing the desiccant, and this paper could do a great service to the community by strongly advising all RS92 users to either abandon applying the GC correction or at least use a correction of 1% RH as a criterion for changing the desiccant. I have seen tropical soundings where the correction was allowed to reach 7% RH. GC corrections of even 2-3% RH are obviously in error because they lead to negative RH values in the stratosphere.

Also, it might be advisable in order to be clear, to repeat in this section that the other part of the launch prep, the heating of the sensor in the GC25, is necessary in order to drive off contaminants.

Also, if fresh desiccant generally indicates a GC correction of about 0.0% RH, apparently the Vaisala calibration is fundamentally based on the assumption that fresh desiccant really does produce an environment of 0.0% RH. However, laboratory desiccant experiments, plus private communication with Vaisala, suggest that in reality the environment above fresh desiccant is 0.3-0.5% RH. This suggests a possible systematic calibration bias of this magnitude (at 0% RH and relative warm T).

p3749, Sec 6.3: This section would be more robust if the profile data upon which Table 3 is based were presented, for example nighttime profiles of RS92/CFH differences (perhaps 1 km vertical averages), after correcting for the time-lag error.

This correction may require more justification because it is empirical (not physically-based like the other corrections), and as such it may include other sources of error such as a constant calibration bias at 0% RH that is not well-represented linearly as a scale factor, or CFH-related considerations such as its suspected bias above -20C or so when there is liquid condensate on the mirror, or mismatched time responses between CFH and RS92 at low T. Such complexities may be responsible for a strong RH-dependence in the "calibration bias" from CFH/RS92 comparisons found by Miloshevich et al. (2009, Fig. 9a), even though as stated the calibration error should theoretically be just a function of T. This is also a less reliable correction because it will be in error if Vaisala ever changes their calibration function, since such changes by Vaisala affect even the raw data used by GRUAN. I suggest that the paper reflect more caution about how well understood and reliable this correction is. It is appropriate that the stated uncertainty is large in that it is roughly equal to the correction itself.

p3749, Sec 6.4, line 18 or elsewhere: It may strengthen the case to also reference the radiation error found by Miloshevich et al. (2009, Fig. 9b), which was derived from a different CFH/RS92 dataset and showed a similar height-dependent bias.

p3750, line 1: This could be clarified a little, for example: "dT is THE RADIATIVE HEATING OF THE TEMPERATURE SENSOR calculated from...where f is an empirical scale factor that accounts...".

p3750, line 12: The radiation error for post-2009 sondes has been reduced by half from pre-2006 sondes? Since that is substantial, can the data be shown from which the post-2009 values in Table 4 were derived?

p3751, line 9: Since the reference is to a website (non-permanent), the authors are invited to also include the figure and caption in their paper if they wish (or alternatively, add a sentence of explanation along the lines of the figure caption that the fit is based on the same time-constant data referred to earlier).

p3752, line 20: I'm a little confused as to why the GC25 readings (terms 2 and 3 in Eq.

11) are included in the uncertainty estimate, when these same readings are deemed unreliable for applying as a correction due to the varying desiccant contamination. Isn't this essentially adding the amount of desiccant contamination as RH measurement uncertainty?

p3755, line 21: Why is the Vaisala radiation correction not applied to the FLEDT data when it IS applied to the GRUAN data? Doesn't that make Fig. 14 an apples-to-oranges comparison when it doesn't need to be?

p3756, line 2: So do the differences represent only differences in the processing of the raw data (mainly GRUAN spike removal)?

p3756, line 10: Rather than "due to the time-lag correction" should this be "due to differences between the GRUAN and Vaisala time-lag corrections"?

p3757, line 11: This would seem to be an offset at -40C that is attributable to the reference instrument. Can any possible explanation be given as to why the FPH would exhibit a behavior below -40C that the CFH doesn't exhibit? This is potentially troubling as it calls into question the reliability of the reference instrumentation. Upon reflection, the different behavior of the FPH may be the portion that is warmer than -40C rather than colder. Another possible explanation would be differences in the typical RH values and hence the corrections in the lower levels of the Boulder soundings relative to the other sites.

p3757, line 20: It is implied here that there may be an underestimate in the calibration correction in a portion of the temperature range (though this correction appropriately has a large uncertainty). Note that the calibration correction reported by Vomel et al. (2007b) is smaller than the RH and T-dependent calibration correction reported by Miloshevich et al. (2009). The inclusion of an RH dependence in the latter correction is one possible explanation, namely that it is not purely a T-dependent bias because it is empirical and includes other small sources of error, and has an RH-dependence that explains differences between sites that have different typical RH profiles. This is just

speculation.

p3761, line 4: Could there be a problem with using the sonde temperature measurement right at launch? The temperature may jump at launch because there is no ventilation so T will be too high for a couple seconds.

p3766, sec 10.1: I suggest several changes regarding the RS92 launch preparations discussed in the second and last items in this list, first by moving the last item to the third item since they are related. You are recommending two inconsistent things: following the manufacturer-recommended launch prep procedure, but not applying the desiccant-based GC correction. It should be clarified that it is important for users to apply the recommended sensor reconditioning (heating) to drive out contaminants that give a dry bias, but users should not apply the GC correction (especially if it is >1% RH).

While GRUAN reverses the GC correction when reprocessing from raw data, everyone else uses the Vaisala-processed data product, and since this section is about recommendations to others for making reference-quality soundings, the recommendation should be to not apply the GC correction in the first place.

Another related recommendation to mention in this section is for users to select the FLEDT output file rather than the default EDT output file, because EDT has only integer values for RH, and this low precision results in poor accuracy for low RH values as well as difficulties and inaccuracies if a time-lag correction is applied. It might even be helpful to describe how users select FLEDT output files with the Digicora.

f3767, line 16: Is this really possible? Perhaps Vaisala should be asked about this possibility. The RH calibration function is not linear; I think it is a non-linear fit through several RH calibration values and therefore a linear scaling would be inappropriate (except possibly above the highest RH calibration point). At a minimum the statement should be hedged by beginning the sentence with something like "Investigate using..."

p3767, last sentence: curiosity question: changing the dT correction value would also affect the RH radiation correction...would the factor f need to be rederived?

Aside to authors: Just FYI, not for the paper, the RS92 heating cycle for the dual sensors effectively reduces sensor icing but does not entirely eliminate it in two situations. One is well-known (below -60C for ice-supersaturated conditions), but the second recently became apparent when someone was exploring curious results just above low-level cloud tops in Greenland that turned out to be instrumental artifacts (this interpretation was confirmed by Vaisala). For a portion of a heating cycle, the measurement sensor may be somewhat iced after emerging from cloud top and lead to elevated RH measurements and inconsistency of the RH and T measurements until the heated sensor becomes the measurement sensor. It might be worth trying to detect this condition from a relatively large jump in RH between the sensors when they switch dominance, at least for liquid water clouds when cloud top can be approximately determined within a heating cycle, and add a flag or increase the uncertainty.

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 3727, 2014.