**Atmospheric
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Interactive comment on "Comparisons of temperature, pressure and humidity measurements by balloon-borne radiosondes and frost point hygrometers during MOHAVE 2009" *by* D. F. Hurst et al.

Anonymous Referee #1

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General Comments

The thrust of the paper, Comparisons of temperature, pressure, and relative humidity measurements by balloon-borne radiosondes and frost point hygrometers during MO-HAVE 09 by D. F. Hurst et al, only is to determine statistical evaluations of measurement uncertainties for the Vaisala RS92 and Intermet IMET-1-RSB radiosondes. There has been a long-term interest in radiosonde measurements and accuracy. This paper contributes to that interest while improving the understanding of the measurement ca-

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pability of two radiosondes in particular. The overall presentation is well structured, addressing each parameter separately allowing an orderly hierarchy of measurement discussion, i.e., temperature, pressure, relative humidity, etc. The authors look at paired measurements and separate, what they term, anomalous and non-anomalous profiles for a detailed analysis. It is not unusual for bad data to be mixed with good data. The reality would be to remove outliers. Conclusions are not given, but the paper's summary provides a reasonable explanation of the analysis. However, the presentation is not always clear.

The clarity of the paper needs improvement. A large number of sentences are written in a convoluted style that made reading the paper difficult, forced re-reading, sometimes more then once. The paper contains a number of mistakes, wordy sentences, non-sequiturs, and redundancy. The authors did a good job of identifying uncertainty limits but the detail provided in the text is extreme and often difficult to grasp; Table 2 was useful in this regard. The figures showing multiple profiles might be modified to include only the anomalous profiles discussed in the text, if the authors decide to retain them. The usage of subjective terms such as, more than, majority of, most of, etc., does not contribute to understanding how bad or how good are the results. The use of specific values or numbers would aid the reader. This paper requires significant clarification before being accepted for publication.

The authors include anomalous profiles in their determination of uncertainties; when trying to obtain information about the level of instrument quality and uncertainty shouldn't the best available data be used. Is it the basic premise of uncertainty analysis to examine homogeneous objects? The paper assumes the objects analyzed are alike. Only the RS92 instruments are the same, i.e., same configuration, same sensors, and same software. IMET sensors are very different than the RS92, the instrument is a different size and configuration, and the processing used two different software packages, not the manufacturers software. The authors should consider justifying why the uncertainty analysis between RS92 and IMET radiosondes is valid in view of the extremely

different character of these radiosondes. The discussion concentrates on differences, but RS92 temperature measurements are corrected while IMET does not apply corrections. Wouldn't results using corrected and uncorrected temperature differences be bogus.

Eight of 26 profiles stood out that the authors considered being anomalous. What purpose is achieved by including poor data? Justification for including anomalous data is not found in the Introduction of elsewhere in the text. The authors should justify why including the anomalous profiles is important. Eight profiles are 30 percent of all profiles, a relatively large portion.

The unfortunate lack of exact timing regarding the release of RS92 radiosondes (2-second time hacks) and the IMET radiosonde (1-3 second non-uniform time hacks) makes the fidelity of the match up procedure suspect. The authors depended on the software to obtained release times that provided ill-defined precision. Were he actual times of instrument release visually noted? These would be the basis for using elapsed times. Simultaneous comparison between two and more instruments depends on precise time match up which is not apparent from reading section 2.3. From the discussion given, time stamping as described does not seem to have been very successful and may have led to relatively poor profile synchronization. The paper notes the RS92-SGP and IMET radiosondes have GPS capability. GPS times would have been a more precise method with which to align the profiles. Although the RS92-K radiosonde did not have GPS capability better match ups probably could have been accomplished. A short explanation why the available GPS times were not utilized would be useful, otherwise why mention the GPS capability. Neither were the number of K-type instruments not mentioned in the text or tables.

Mistakes and typos should have been noted and removed by the authors before submitting the paper. One example is found on line 18 of section 1, Introduction. The date given for MOHAVE 09 should have been 2009, not 2011. Another significant mistake is found on line 13 of section 2.1 Radiosonde. Vaisala's pressure sensor is not piezo

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resistive, it is a silicone capacitive sensor.

SPECIFIC COMMENTS

Page 4360 Line 17. Mistakes. Campaign date October 2011 s/b October 2009 and the reference to Leblanc et al, 2011 is missing from reference list on page 4380.

Page 4361 Line 12. Mistake. RS92 does not use a piezo-resistive pressure sensor but uses a Silicone Capacitive Sensor. Page 4361 Lines 19-21. Wordy. 'two types' and . . . study: total measurement uncertainties and measurement reproducibilities.

Page 4362 Line3. Wordy. Why not use . . . thermal lag error . . . . Page 4362 Line 21. 'accuracy limits' is redundant. Page 4362 Line 23. The authors noted that the IMET radiosonde did not have temperature corrections but should also have mentioned the RS92 radiosonde has temperature corrections, although its nighttime corrections are small. The authors might mention whether they took into account the possible influence of these errors on the RS92-IMET differences?

Page 4363 Data Matching section. A clearer description of the time stamp method is needed. Data profile matching is the key ingredient for a valid analysis and should be clearly described, including positive and negative issues. The authors claim the time stamps to be' most reliable' (line 23) but continue the discussion with an explanation of the offsets encountered. After carrying out the match up process, average time differences of 2.5±6.0 seconds remain. This much difference compromises the match up fidelity and constitutes a flaw in the analysis. The authors should justify how were these differences actually were handled. Matching profiles using correlation seems to be a good idea but did not recover 100 percent of the profiles. A secondary method (page 4364 lines 11-17) was used to match profiles, but use of this method contradicts the statement made earlier (page 4363 lines 23-24). There is a lack of match up consistency that points to arbitrary profile alignment. The authors need to mention why GPS was not used? Page 4363 Line 25. Wordy. Remove 'measurement timestamps as'. Page 4365 Line 19 to end of paragraph. The method used to identify some of the

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profiles as being anomalous seems arbitrary. If some (number needed) profiles are indeed anomalous the authors should explain why they were not totally excluded from the analysis or for that matter, included? Furthermore, eight anomalous profiles is a fairly large portion of the total measurements, thus a further reason to remove them.

Page 4366 Line 2. Wordy. What does the word 'repeatedly' indicate? Remove. First paragraph. The authors use median differences, which reduces the data ensemble to one point per kilometer per radiosonde pair, while arguing the median is better than using averages because it removes random differences. Were the median points tested for randomness? It is true using the medians reduce calculations to a minimum while eliminating the effect of extreme differences but the question is whether or not extreme differences should be removed or allowed to remain. How was this decision reached? Were the data normally distributed and were outliers removed before selecting the median? Page 4366 Lines 16-25. The authors need to be clear when discussing uncertainty, reproducibility, and accuracy. In fact, definitions would help. Furthermore, RS92-RS92 uncertainty differences are quoted for pressure regimes 1080-100 hPa and 100-3 hPa and reproducibility is given for three layers: 1080-100 hPa; 100-20 hPa; and 20-3 hPa. The authors have mistakenly transformed the manufacturer-quoted reproducibility pressure boundaries to 400 hPa and 100 hPa. Why 400 hPa when it is not a boundary given earlier in the text or by Vaisala? See Vaisala RS92 Tech. Data Sheet [Ref. B210358En-E].

Page 4367 Line 3. If the TF028 and TB028b profiles are well inside the uncertainty limits the authors should explain why they are considered anomalous? In fact, agreement between these profiles is the best of the ensemble. Page 4367 Line 6 beginning with…All but two … etc. Sentence is confusing. Has 'excessive differences' been identified earlier? What are the excessive difference limits? Page 4367 Line 9. Wordy. Page 4367 Line 11. Wordy. Non sequitur. Explain or unravel 'small' vs 'similarly anomalous'? Page 4367 Line 12. TF028 in Figure 3 is not identified. Page 4367 Line 16. Profile TF025 should be identified in Figure 2. Page 4367 Line 22. Where can the

'specified temperature differences' be found? These appear to refer to specific values but Figure 4 does not show specified temperature differences.

Page 4368 Line 6. Subjective. What is the 'majority of profiles', i.e., how many? Page 4368 Line 18. If it is clear from Figure 3 that the two anomalous profiles will skew the statistics negatively, why were they included in the analysis?

Page 4369 Paragraph beginning with line 6. The anomalous differences near the surface suggest time differences between the instrument profiles may be the reason for such large separations (See previous comment page 4363). The radiosonde operator should have noted pre-release discrepancies and followed the manufacturers' instructions by rejecting the radiosondes. Page 4369 Line 15. I may have missed a definition for 'conforming' and 'non conforming' earlier in the paper. These are subjective terms, please define or include limits, if any. Page 4369 Line 29. The term 'near-normal distribution' is difficult to comprehend. If near-normal in the Guassian sense, were the data skewed or have an unusual kurtosis? Page 4370 Paragraph starting at line 9. The authors may want to reconsider use of the term 'smallest' on line 15. At 16 km ($\sim$100 hPa) $\pm$0.78 hPa is an uncertainty of approximately 0.8 percent while at 32 km ($\sim$10 hPa) this uncertainty limit is $\sim$8.0 percent, in the first instance a calculated height error is $\sim$50 meters, the second instance the height error is about $\sim$530 meters. Page 4370 Line 27. This seems to be another instance when the radiosonde operator should have noted large differences prior to balloon release. If acceptance/release criteria were not met prior to balloon release should those profiles that include large surface anomalous data records be removed from the data set? Additionally, TF025 and TF025b are not labeled in Figure 7.

Page 4371 Line 5. Correct units from C to hPa.

Page 4372 Line 11. Wordy. . . . RH, and each belongs . . . . Not clear to what 'each' refers.

Page 4373 Line 6. 'well documented algorithms.' References should be included. Page

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4373 Line 11. Redundant. Remove word 'here' and end of sentence, 'for temperature differences (Fig. 2).' Page 4373 Lines 22 through 27. The second sentence contradicts the first. Sentence beginning with 'A big temperature difference . . . is ok, but the following sentence does not follow. Unless the atmospheric temperatures were unusual wouldn't the temperature at 4-7 km be higher (warmer) than temperature near 10-12 km?

Page 4377 Paragraph at line 19. I disagree with this statement and the next. Considering each level has a different pressure and/or temperature bias it is possible the total error would be small or none at all. However, a biased pressure measurement at the surface of 1 hPa (i.e., an offset) results in approximately 670-meter error at 10 hPa given the temperature measurement is accurate. Therefore, two instruments with different pressure offsets at the surface would not have a constant difference with altitude.

Page 4378 Line 22. What are the authors' descriptions of non-conforming vs conforming?

Page 4379 Line 5. I disagree with the use of 'typical'. The small sample size precludes assuming anything is normal or typical. Page 4379 Line 16. The result of 'dividing' is to obtain a 'smaller' quantity! Would the word 'combining' be better usage? Page 4379 Line 22. Remove extra words.

Page 4380 References. Add reference to LeBlanc

Page 4383 Caption. Remove sentence beginning with 'Astericks' . Insert asterick symbol in the notes below the figure.

Page 4384. Figure 1. Explain subjective phrase: (non-conforming to the majority ...)

Page 4386. Figure 3. Redundant. 1-km median not needed in sentence. Last sentence is not clear.

Page 4387. Figure text too detailed. Some explanations not needed. The figure is

obvious.

Interactive comment on Atmos. Meas. Tech. Discuss., 4, 4357, 2011.

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