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Atmospheric Measurement Techniques Discussions

Interactive comment on "Altitude misestimation caused by the Vaisala RS80 pressure bias and its impact on meteorological profiles" *by* Y. Inai et al.

Anonymous Referee #2

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General Comments:

This paper presents a study of the impact of the pressure registration error present in radiosonde instruments on the profiles of temperature, ozone, and water vapor as gathered in the Soundings of Ozone and Water in the Equatorial Region (SOWER) campaigns from December 2003 – January 2010. The pressure registration error problem has been identified in the literature previously (including in another work by the lead author published in the Japanese journal SOLA in 2009, and in Steinbrecht et al., 2008) and discussed extensively for its influence on the ozone profiles in the recent paper by Stauffer et al. (2014).

What's new in this paper is the application of the pressure offset to investigate the influence on temperature and water vapor profiles, although both of these impacts are natural incremental steps that build upon the pressure offset error work previously published. Also new is the careful analysis of the SOWER data itself and the discussion of the impacts of the pressure offset error on analysis of that data. Most important in this work is the discussion of the impact of the pressure registration error (and connection to altitude error) on studies that examine trends of temperature and/or ozone in the UT/LS and stratosphere. In fact, this observation is critical to the modeling and trend community and needs further exploration.

That said, the only real difference in the technique in estimating the errors resulting from the pressure offset correction as applied in this paper versus that discussed in Stauffer et al. (2014) appears to be the choice of independent variable (as discussed in the detailed comments below). This paper simply reproduces and confirms the findings of the Stauffer et al. paper – the conclusions are compatible and nearly identical. In the case of the former paper, it seems the authors used as their independent variable the GPS altitude (which I argue below is equivalent to time, the true independent variable in the sonde measurements) while the current authors use altitude, albeit a variable that changes between their PTU and corrected values. From the standpoint of the data user, in fact, the authors are correct in the value of knowing the errors that result from shifting from the uncorrected PTU altitude to the corrected PTU altitude (or GPS altitude). For those sondes that have had GPS, however, the user community could always have chosen the GPS altitude over the PTU altitude as the vertical coordinate, which would have resulted precisely in the errors described by Stauffer et al. (2014). The shift described in the present work arises only in the case that the user always referred to the PTU altitude (corrected or uncorrected) as the vertical coordinate, which undoubtedly is the case for many analyses, and which therefore is an important point to raise in the larger data user community.

As a final general comment, the authors seem to imply some insufficiency in the Stuaffer et al. (2014) study. It is not clear to this reviewer what that insufficiency is. As described above (and in the detailed comments below), the studies basically reach the same conclusions about the problems resulting from the pressure registration error. In that sense, this paper does not present something novel. Again, the discussion section in the present work is important – the problems in profile shapes and offsets (whether temperature, water vapor, or ozone) are important and have real implications for the trend community. I am not sure that such an observation, however, is worthy in and of itself for publication. An explicit example of the changes in trends resulting from the corrections described in this work would be beneficial to the community. Furthermore, the paper also could make a valuable contribution to the literature if it presented an approach that would allow for correction of profiles without GPS units on board (which was the case for most of the older, historic radiosonde data in the archives). In its present state, however, this paper does not provide such an approach, and it is not clear how the community should account for these errors in the historic data record – a critical problem for the trend community.

While the paper does point out important implications in trends resulting from the pressure offset errors, and while it points out the influences not only on ozone but also on temperature and water vapor (a step beyond the Stauffer et al., 2014 work), it is not clear in its present form that it contains enough novel to warrant publication. Detailed comments are below.

Specific Comments:

2194-15: As I begin this paper, I am not sure that I understand the problem that the authors are citing with the Stauffer et al. (2014) study. That study characterized the pressure offsets of a large collection of RS-80, iMet, and RS-92 radiosondes. All of the sondes in that study also had GPS instruments on board, allowing for the validation of the pressure-derived altitude. If the authors are saying that the Stauffer et al. (2014) approach cannot be used to correct sondes without any GPS instruments on board (which would be the case for large fraction of RS80 launches worldwide), then indeed, the Stauffer et al. (2014) approach is insufficient, as it provides no method to correct

such profiles. That said, this paper does not provide a clear way to accomplish that task either (although that would be valuable to the community). The authors should clarify the cited "insufficiency" of the Stauffer et al. (2014) paper.

2196-7: When solving this equation in the stratosphere, what is the contribution of the second term in the denominator (the one involving the saturation water vapor pressure)? My experience suggests that RS80 radiosondes often report RH values in the stratosphere at values higher than feasible. What would be the impact on the calculation of this second term being non-zero? How much uncertainty is introduced into the calculation by using the RH data in the stratosphere? What is the maximum altitude at which the Humicap sensor is reliable? The authors should communicate this information in the text so that the readers can evaluate the reliability of the altitude formulation the authors are using.

2196-21: The differences between the blue and red curves in Figure 1 are very small. The authors note that in the troposphere, they amount to $\sim$20 m but grow to $\sim$240 m at 30 km. The authors do not communicate the uncertainty associated with their altitude and pressure altitude calculations. My experience with sondes suggests that 240 m will fall below the inherent uncertainty associated with the data contributing to their calculations using equations (1) and/or (2), but the authors should compute the uncertainties associated with each calculation using the manufacturer's specifications for uncertainty in T and RH, which are as follows (from the Vaisala RS80 information sheet): pressure accuracy: 0.5 hPa temperature: 0.20C below 50 hPa; 0.30C from 50 – 15 hPa, 0.40C above 15 hPa. Relative humidity: <3%, repeatability of 2% for calibration I am curious to know whether the extra effort to which they have gone has produced a result statistically significantly better than the Stauffer et al. (2014) approach.

2197-24: The reference to Shiotani (2013) should identify that work as a comment on the Stauffer et al. (2014) publication in the text. As it stands now, the reader may misinterpret that reference as a peer-reviewed publication critiquing the Stauffer et al. study.

Furthermore, the Stauffer et al. (2014) study most certainly discusses the altitude offset caused by the pressure bias (see their Section 2.4 and discussion of their Figure 2 as well as their Figure 10, which clearly shows that offset as well). The discussion regarding the altitude offset also appeared in the original discussion paper (doi:10.5194/amtd-6-7771-2013), and the authors addressed thoroughly the comments of Shiotani in response to their original discussion paper, although they took a slightly different approach than recommended by Shiotani, one that produces results not very different than his proposal. Therefore, the comment in the current manuscript that "The authors discussed the influence of pressure bias on the ozone mixing ratio, but they did not take into account an altitude offset caused by the pressure bias as pointed out by Shiotani (2013)" seems inaccurate. I recommend this sentence be stricken or altered.

2198-8: This paragraph is difficult to follow, particularly starting in line 11. Perhaps the authors could attempt to clarify this text.

2198-18: The temperature bias of -1.2K for an altitude offset of just 230 m suggests a large vertical gradient in temperature at this altitude, as shown in the left-hand panel of Figure 3. Are such temperature gradients common at these altitudes in the SOWER record? What is the mechanism responsible for such a large temperature gradient? Would it be helpful to show the associated ozone profile at the same time? Do the authors believe the gradient is real or an artifact of the measurement?

2198-23: If I understand the authors' argument correctly, this temperature "bias" is simply the result of the altitude offset due the pressure registration error rather than a bias in the temperature measurement. If my interpretation is correct, I would prefer to describe this as a temperature profile offset resulting from the pressure registration error than a temperature bias. In my mind, using "bias" infers that the temperature measurement itself was biased, which is not what the authors are arguing. I note that the mean temperature offset above 20 km is negative, as expected with a mean pressure bias that is negative (i.e., the radiosonde reports lower pressure/higher altitude than it should, shifting the profile upward, and in the context of a roughly monotonically

increasing variable like temperature, that results in, at a given "altitude," a temperature that is too low in the uncorrected data.

2200-18: Looking at Fig. 7 in Stauffer et al. (2014) and using the RS-80 data, the 2, 5, and 10% figures cited in the present work appear to refer to the dashed line representing the 90th percentile rather than the solid black line representing the median. Using the median produces values closer to 0.5, 2.5, and 7% at 20, 25, and 30 km respectively.

Regarding the change in sign, it appears that the assumption made in Stauffer et al. (2014) was that the independent variable for comparison was the GPS altitude. In reality, the truly independent variable for all of the sonde measurements is time. Given the relationship between GPS altitude and time, however, perhaps their use of GPS altitude as the independent variable was not such a bad one. That said, in the case of their Figure 7, if you plot the ozone profile vs. GPS altitude both before and after the correction, and if on average, the pressures of the RS-80 radiosondes were registering too low by $\sim$ 1.0 hPa, then the expected correction should increase exponentially with GPS altitude.

The approach of Inai et al. (2015) appears to instead use the altitude (uncorrected pressure altitude vs. corrected pressure altitude = GPS altitude) as the independent variable. Thus, the vertical coordinate in their Fig. 5 changes depending on whether or not the correction has been applied. Personally, I do not like the idea of the "independent variable" changing. That said, data users indeed want to know the error that they are making as a result of using the uncorrected data, and the right-hand panel in Figure 5 as well as Figure 6 of the present work communicate those errors well. As expected, the sign of the error changes with altitude at the peak of the ozone partial pressure. This result, however, also appears in Stauffer et al. (2014) Figure 10B, although the ozone error cited in the present work is not so clearly described. Thus, the differences that the present authors point out are not a real differences, but rather ones that results from a different choice of the vertical coordinate. Both papers are correct.

2201-1-2: In fact, the summary recommendations in Stauffer et al. (2014) suggest only launching ozonesondes with a GPS unit on board. Is this paper suggesting here that its approach can be generalized to those ozonesonde flights without GPS units? If so, that would be a welcome addition to the literature. However, in its present version, I find no clear path to implement corrections to ozonesonde flights without GPS units.

2201-2202: The authors should probably communicate clearly that the entire water vapor discussion is dependent upon a sensor with better sensitivity than the standard radiosonde can provide. Use of a frost-point hygrometer (FPH) or cryogenic frost point hygrometer (CFH) is likely necessary for these results to have meaning. Furthermore, the altitude offsets near the cold-point tropopause are typically small and may well be within the errors of the pressure sensor/GPS unit. The authors also do not compare their estimated offset errors due to the pressure registration problem with the inherent uncertainties of the water vapor measurement using the FPH or CFH measurements. Such a comparison would be useful to evaluate the importance of this error in studies of UT/LS water vapor.

2202 and following: The discussion of the implications for trend studies of temperature and ozone in the stratosphere is important. The community should examine the instruments used at each site included in trend studies. A switch from RS80 to iMet might not lead to an artificial trend, as the pressure offset errors between the two instruments appear similar (see Stauffer et al., 2014). However, a switch from either of those instruments to RS-92 might introduce into the data record an artificial trend. Such an error should appear as a discontinuity in the data record at a given site and should, therefore, be easily identifiable. An important question for the community is how to handle historic data from sites without GPS instruments, for which determining the appropriate correction may be more difficult.

2206 – Appendix A: This discussion might explain the reason that the current paper found a somewhat lower average pressure offset than did Stauffer et al. (2014). What would the average offset have been if the authors did not use a cutoff of 1.5 hPa to

exclude 5 of their profiles?

2214-Fig. 3: The choice of 0.7 K units for dT is odd. It would appear that the differences (shown in the right figure) are roughly at or below the estimated uncertainty in the Vaisala RS80 manufacturer's specifications below 28 km. This example for a single profile is not so instructive and must be considered in the context of an ensemble of such observations to reveal any consistent offset or bias.

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 2191, 2015.