GENERAL PEPLY

We thank the three reviewers for useful comments and suggestions to our paper manuscript. Since we found that they raised common questions, we would like to repeat our standpoint as follows in this General Reply. Though it might be already well understood by some of the reviewers, we think that it would be better to make the point clearer.

The main question commonly seen in the three reviews is about originality of our paper in comparison with the paper by Stuaffer et al. (2014). Reviewer #1 wrote, "The paper does not present much new information over and above what has already been published in the cited papers by Inai et al., 2009; Steinbrecht et al., 2008; and Stauffer et al., 2014." Reviewer #2 wrote, "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." Finally, Reviewer #3 wrote, "Major issues involve further expanding upon unique aspects of this paper and differences between this manuscript and Stauffer et al. (2014)." In addition, Reviewer #1 noted "page 2194, lines 23, 24: To me, that statement is wrong, and should be deleted. I have looked at the Stauffer et al., 2014 paper. They correctly account for the effect of pressure on both mixing ratio calculation and vertical profile shifts." Reviewer #2 noted "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)."

First, we do not argue that discussion by Stauffer et al. (2014) is incorrect, but we are proposing a better way to present the effect of pressure bias *on the vertical profile shift*. This point was already discussed by Shiotani (2013) during the interactive discussion for the paper by Stauffer et al. Unfortunately, the Stauffer et al. paper has been published without taking into account his main suggestion for further improvement. This is the motivation of our current paper manuscript, which will be described in the following.

Before going into the detail, we note here that there are two comparison methods for the measurements from two radiosondes (or ozonesondes or water vapor sondes) which were flown simultaneously with a single balloon. The first one is the "simultaneous *sensor* comparison" (i.e., at the same time during a flight or for the same air parcel) which is used, e.g., during the WMO Radiosonde Intercomparison Campaigns (e.g., Nash et al., 2011). This is appropriate for, for example, evaluation of the performance of relative humidity sensors from different radiosonde types. However, the results cannot be readily used for data homogenization and trend estimate, because this method does not inherently reveal any potential issues in association with vertical axis shifts. If one wants to carefully analyze radiosonde data for some specific period including the instrument switchover from old type to new one, the other comparison method should be used. That is the "comparison on pressure (or altitude or geopotential height) levels" by taking into account vertical axis shifts arising from the different performance in pressure sensors, as performed by, e.g., Steinbrecht et al. (2008) and Kobayashi et al. (2012). The Steinbrecht et al. paper is for evaluating temperature *profile* difference associated with switchover from Vaisala RS80 to Vaisala RS92 and the Kobayashi et al. paper for temperature and humidity *profile* differences associated with switchover from Meisei RS2-91 to Vaisala RS92 to homogenize their data records.

Bearing above in mind, to make our motivation clearer, here we divide past ozonesonde data records into the following three periods (Period A, B, and C).

Period A:
Radiosondes with a pressure sensor, but without a GPS sensor, were used. In this period, both altitude and ozone mixing ratio were calculated by using "biased" pressure data. Therefore, the mixing ratio was offset, and the ozone profile was also vertically shifted. Such a situation was common before around the year 2000.

Period B:
Radiosondes with a pressure sensor as well as a GPS sensor were used. Both we and Stauffer et al. examined observational errors using sounding data during this period.

Period C:
Radiosondes with a GPS sensor, but without a pressure sensor, are used. In this period, we can obtain "correct" pressure and "correct" altitude from the GPS sensor, and consequently "correct" ozone mixing ratio. Such a situation is common after around the year 2010.

In their Figs. 2, 3, and 10, Stauffer et al. actually showed "biased" mixing ratio of ozone plotted on "biased" altitude; however, in their Fig. 7 which is the key figure described also in their Abstract, they showed errors arising when "biased" mixing ratio is plotted on "correct" altitude, which is indicated in the labels on vertical axis of Fig. 7 as "GPS Altitude". In other words, they evaluated the ozone mixing ratio errors in their Fig. 7 by subtracting the "correct" ozone profile plotted on "correct" altitude from the "biased" ozone profile plotted on "correct" altitude at the same altitude. This error calculation is equivalent to the "simultaneous *sensor* comparison" as explained above, because the two profiles use the same vertical axis based on the same GPS measurements. This approach is only useful for correcting profiles taken during Period B, when one use a combination of "biased" pressure for the mixing ratio calculation and "correct" altitude obtained from a GPS sensor.

In contrast, we are trying to evaluate observational errors for conventional soundings for Period A and to consider artificial offsets in the meteorological records associated with the transition from Period A to Period C. This has to be done by the "comparison on pressure (actually geopotential height in our study) levels" as described above.

The "comparison on geopotential height levels" approach is useful for data homogenization and trend estimate, but we admit that our original discussion paper had a problem as pointed out as follows. Reviewer #2 wrote, "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." Also Reviewer #3 wrote, "Analyses and comparisons between existing trends papers and trends calculated using reprocessed data should be expanded upon in the text and figures. These results would be of great interest to the ozone and TTL water vapor trends community."

According to these comments, we will show an attempt to correct sounding errors without GPS information in the revised manuscript. Of course, we can not perfectly evaluate sounding errors and correct them when an ozonesonde and a water vapor sonde are launched without GPS sensors. Still, we can correct them if we assume that a statistical feature of radiosonde pressure bias in historical datasets would not differ from that in this study. On the basis of this assumption, we will make an additional analysis where the errors of the SOWER sounding data will be corrected without GPS information. In this way, we will propose an appropriate approach how to deal with historical datasets affected by the pressure sensor biases.

References:

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Stauffer, R. M., Morris, G. A., Thompson, A. M., Joseph, E., Coetzee, G. J. R., and Nalli, N. R.: Propagation of radiosonde pressure sensor errors to ozonesonde measurements, Atmos. Meas. Tech., 7, 65–79, doi:10.5194/amt-7-65-2014, 2014.

Steinbrecht, W., Claude, H., Schönenborn, F., Leiterer, U., Dier, H., and Lanzinger, E.: Pressure and temperature differences between Vaisala RS80 and RS92 radiosonde systems, J. Atmos. Ocean. Tech., 25, 909–927, 2008.

Kobayashi, E., Noto, Y., Wakino, S., Yoshii, H., Ohyoshi, T., Saito, S., and Baba, Y.: Comparison of Meisei RS2-91 rawinsondes and Vaisala RS92-SGP radiosondes at Tateno for the data continuity for climatic data analysis, J. Meteor. Soc. Japan, 90, 923-945, doi:10.2151/jmsj.2012-605, 2012.

REPLY TO COMMENTS BY REVIEWER #1

We are grateful to your thorough reading and constructive comments on our manuscript. We believe we have incorporated all aspects pointed out. For your major comments, we have prepared "General Reply" as a separate document. Please read it first. The detailed description on the revisions follows:
(Authors' comments and responses in red.)

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 #1
Received and published: 3 April 2015

The paper by Inai et al., reports on pressure errors, up to 0.4 hPa, for RS80 radiosondes. Pressure errors were determined using GPS data as a reference. Results come from SOWER radiosoundings over the 2003 to 2010 period in the tropical western Pacific. The pressure errors result in vertical shifts for temperature, water vapor and ozone profiles. The paper describes that the errors arising in these quantities from the vertical shifts can reach up to 1 K in temperature, one or two percent in water vapor and several percent in ozone. Errors are usually small to negligible in the troposphere, and become substantial in the stratosphere. Overall this is an important topic, relevant for atmospheric observations, and especially relevant for long-term trends from radiosonde data.
The paper does not present much new information over and above what has already been published in the cited papers by Inai et al., 2009; Steinbrecht et al., 2008; and Stauffer et al., 2014. Still, the manuscript does provide another independent piece of information on the apparent pressure bias of Vaisala RS80 radiosondes. To me, this makes it acceptable for ACP.

1 Major comments

In order to put the Inai et al., results into better perspective, and to make it easier for readers to get the full picture, I request that the authors include the biases reported in Steinbrecht et al., 2008 and Stauffer et al., 2014 into their plots (Figs. 1,2,4,6).

It is not easy to include their results into our figures, but we will describe the difference between our result and their results in our text.

In many places, the paper should be made shorter and more concise. Focus should be much more on the new / specific aspects.

• Abstract and Introduction could easily be shortened by 50%.

We will shorten Abstract and Introduction but will add new descriptions to convey our motivation more clearly following the reviewer's suggestion.

• Eqs. 1 and 2 are equivalent. One of them should be omitted, the discussion should be shortened substantially, since the cited references or a textbook give the information.

Eq. 2 will be omitted and the description will be shortened.

page 2194, lines 23, 24: To me, that statement is wrong, and should be deleted. I have looked at the Stauffer et al., 2014 paper. They correctly account for the effect of pressure on both mixing ratio calculation and vertical profile shifts. Note that this depends on the vertical coordinate system, and on the ozone quantity considered.

As described in General Reply, while Stauffer et al. (2014) showed the vertical shift as in Figs. 2, 3, and 10 in their paper, they defined the ozone mixing ratio error without considering *vertical profile shift* when they calculated the error shown in Fig. 7. They used only zGPS as vertical coordinates for biased profile and for corrected profile, and then they calculated the difference of the biased profile from the corrected profile at the same altitude. Such a procedure is equivalent to the comparison of two sensors at the same time or for the same air parcel, but not equivalent to the comparison of two profiles.

There are three possible vertical coordinates:

1. Time: In that case, the ozone partial pressures are the same with or without pressure error. Ozone mixing ratios will be different, however, due to division by different pressures (at the same time).

That is correct.

2. Altitude: In that case, the ozone partial pressures with or without pressure error will differ, due to vertical profile shift. Ozone mixing ratios will differ further, due to additional division by different pressures (at the same altitude level).

The first sentence is correct.

The second sentence, however, needs clarification. It is correct that ozone mixing ratios will differ further due to additional division by different pressures, but the amount of difference is very small. If temperature and humidity are uniform in vertical, pressure exactly stay unchanged according to the Equation (2). Therefore, in this case, the % error of mixing ratio exactly corresponds to the % error of partial pressure. In reality, temperature and humidity are not constant, so these values also differ due to the altitude shift. However, the differences are very small. For example, 0.5 K of temperature offset in the stratosphere (e.g., 30 km) where temperature is 230 K produces only 0.2% pressure difference (i.e., 0.5 K/230 K). This is much smaller than the ozone partial pressure offsets estimated in Section 4.2 (3% at 30 km). The difference in mixing ratio between the biased profile and the corrected profile is almost same as that in partial pressure with an uncertainty of 0.2%. We will note this in the revised manuscript.

3. Pressure: In that case, the ozone partial pressures with or without pressure error will differ, due to vertical profile shift. Ozone mixing ratios will not differ further, because division is by the same pressure (at the same pressure level).

This is correct.

I think the manuscript currently is not presenting these error sources correctly, e.g., in Figs. 5, 6 and 7. At least, I find the current presentation confusing or misleading. I think this needs to be corrected/ improved. As usual, shorter would probably be better.

As described above, the error for mixing ratio is almost same as that for partial pressure with an uncertainty of about 0.2%. Hence we have presented only the error for partial pressure representing the both errors in Figs. 5 – 9.

2 Minor comments

Page 2196, lines 9, 10: Note that Stauffer et al., 2014 use a slightly different value for the specific gas constant for dry air (287.05 J kg-1 K-1 versus 287.10 = 8314.51 / 28.96 here).

We will recalculate our results using the same value as Stauffer et al. (2014).

Page 2197, last paragraph: As mentioned, the pressure biases from Steinbrecht et al., 2008; and Stauffer et al., 2014 should be included in Fig. 2, and could be compared briefly here.

As already described at the response to your first major comment, we will include the discussion in our text.

Page 2199, lines 10 to 19: As I said before (last major comment), Stauffer et al., include both profile shift and conversion to mixing ratio correctly, as they plot things as a function of altitude. They do not plot things as a function of time, or pressure, and they give the overall effect for the altitude profile. See their Figs. 2 and 3, which show both the profile shift and the effect of conversion from partial pressure to mixing ratio. Please correct and shorten.

This comment is correct for Figs. 2, 3, and 10 in their paper, but is *not* correct for their estimation for ozone mixing ratio error (their Fig. 7) as explained in General Reply. They evaluated the ozone error by subtracting the biased profile on zGPS from the corrected profile on zGPS at the same altitude; thus, it is equivalent to the "simultaneous *sensor* comparison" but not to the "comparison on pressure (or altitude or geopotential height) levels." In order to make the point clearer, the above point will be noted in the text of Introduction, and a statement, "when they evaluated the ozone mixing ratio error (their Fig. 7)" will be inserted into P.2197 L.24 and P.2199 L. 17.

Page 2199, line 10 to page 2200 line 8: I find this lengthy and confusing. I would shorten it and omit equations 4 to 6. See my last major comment. Since the authors are using altitude as vertical coordinate in their plots, there should be two curves in the right panel of Fig. 5, one for ozone partial pressure differences, and one for ozone mixing ratio differences. See also the different curves in the left and middle panel. The red line in the right panel cannot be the partial pressure difference. This difference would not be zero due to vertical profile shifts (see left panel).

As described above, the difference between error for ozone partial pressure and that for ozone mixing ratio is quite small, e.g., by only about 0.2%. This part is very important, describing the reason why the error for mixing ratio is almost the same as that for partial pressure, and thus cannot be omitted.

Figure 6: Again, there should be two different curves, one for partial pressure difference, and one for mixing ratio difference. The corresponding result from Stauffer et al., should be plotted in here as well.

Please see above. The corresponding result from Stauffer et al., i.e., Fig. 7 in their paper, has been described in text from P.2200 L.15 to P.2200 L.22 of the original manuscript.

Page 2200 line 15 to page 2201 line 4: I think those paragraphs should be omitted. I find them more confusing than helpful.

As explained in General Reply, our approach differs from that of Stauffer et al. (2014). This part describes how results differ according to the different approaches. We will make necessary revisions to make this point clearer.

Page 2201, line 12 to 21: Some comments as for ozone. Again, I think there should be two lines in the right panel of Fig. 7, one for the partial pressure difference in the right panel, and one for the mixing ratio difference in the middle panel.

Same as for ozone, the difference between error for water vapor partial pressure and that for water vapor mixing ratio is quite small, e.g., by only about 0.2%.

Pages 2202, line 21 to page 2205 line 2: I find this discussion much too long, and quite far removed from the actual (new) findings of the paper. I do not see the need for reporting all these trend papers in lengthy detail. It would be enough/ better to compare the magnitude of the pressure error effects with the magnitude of observed/ reported trends. I suggest to shorten this discussion by about 50%.

We will shorten the discussion for ozone trend, but as in General Reply, trend estimation issues are one of our important motivations.

Page 2205: I am missing a reference to Stauffer et al., 2014, and I am missing a statement, how your pressure difference and consequent results for temperature and ozone differences compare with the results reported in these studies. Please add, maybe also in the abstract. Since the present study does not report really new findings, it is key to put results into the perspective of existing literature.

As explained in General Reply, we show how we should evaluate sounding errors due to pressure biases of radiosondes in conventional measurements. The motivation of our study and the difference from Stauffer's study will be described clearly in the summary.

In addition to above revisions, we will replace "geometric altitude" with "geopotential height" for vertical coordinate to accommodate our results to conventional data sets.

REPLY TO COMMENTS BY REVIEWER #2

We are grateful to your thorough reading and constructive comments on our manuscript. We believe we have incorporated all aspects pointed out. For your major comments, we have prepared "General Reply" as a separate document. Please read it first. The detailed description on the revisions follows:
(Authors' comments and responses in red.)

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
Received and published: 13 April 2015

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.

As described in General Reply, our motivation of this study is to propose how we should evaluate sounding error in conventional measurements with a biased pressure sensor and without a GPS sensor. We point out that the approach of Stauffer et al. (2014) is not appropriate for such a purpose because they evaluated ozone mixing ratio error with respect to GPS-measured altitude only. That said, the incorrect vertical shifts of profiles due to pressure biases were not accounted for. We recognize, however, that these points were not very clear in our original manuscript. We will revise Introduction so that the points in General Reply are fully covered.

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.

The Humicap sensor is reliable only up to about 12 km, and the RH values reported by RS80 in the stratosphere is always higher than the actual values. The error of RH sensors in the stratosphere, however, has little influence on our analysis. For example, if we calculate zPTU with RH = 0 above the 200 hPa level, the difference from original zPTU (with reported RH) becomes only 3 cm at 30 km. So we used RH values reported by the RS80 radiosonde also in the stratosphere for altitude calculation in this analysis. We will add a note on this in the revised version.

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 ~20 m but grow to ~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.

We examined the uncertainty of our results coming from temperature and RH uncertainties corresponding to the RS80 manufacturer's specifications on our calculation of zPTU. As a result, temperature uncertainty of 0.20^{O}C below the 50 hPa level, 0.30^{O}C from 50 – 15 hPa, 0.40^{O}C above the 15 hPa level and RH uncertainty of 3% produce 34 m of zPTU uncertainty at 30 km. Since this is a third as much as one standard deviation shown in Fig. 1, namely, 90 m at 30 km, we believe the temperature and RH uncertainties do not have significant influence on our analysis. We will add a note on this in the revised version. With the help of our QC procedure (in Appendix A), the statistically significance of our results must become better than that of the results in the Stauffer et al.'s study.

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.

"Shiotani (2013)" will be replaced by "Shiotani (2013) as a comment on the discussion paper by Stauffer et al. (2014)".
As explained in General Reply, the Reviewer's comment is correct for Figs. 2, 3, and 10 in their paper, but not correct for their evaluation of ozone mixing ratio error (i.e., their Fig. 7). They evaluated the ozone error by subtracting the biased profile on zGPS from the corrected profile on the same zGPS. This calculation procedure gives the "simultaneous *sensor* comparison" but not the "comparison on pressure (or altitude or geopotential height)." In order to make the point clearer, Introduction will be revised, and a statement, "when they evaluate the ozone mixing ratio error (in their Fig. 7)" will be inserted into P.2197 L.24 and P.2199 L. 17.

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

As explained in General Reply, what we intended to do is to evaluate the error by the "comparison on pressure (or altitude or geopotential height) levels", not the one by the "simultaneous *sensor* comparison." This explanation will be added.

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?

This magnitude of large vertical gradient is often seen in the tropical lower stratosphere. The mean temperature gradient is about 3 – 3.5 K km^{-1} in the tropical stratosphere, but the profiles often have fluctuations due to large-amplitude gravity waves.

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.

Your interpretation is correct. According to your suggestion, the terminology "bias" will be replaced by "offset" throughout the paper.

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.

We suppose that the reviewer referred to Stauffer et al.'s, AMTD discussion paper, but we referred to Fig. 7 of the Stauffer et al.'s final AMT paper. We took a closer look at the figure and found that the values were 2%, 4%, and 10% at 20, 25, and 30 km, respectively. We will revise those values.

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 ~ 1.0 hPa, then the expected correction should increase exponentially with GPS altitude.

While the explanation of the reviewer is correct, as described in General Reply, the result shown in Fig. 7 of the Stauffer et al. paper was obtained from the "simultaneous *sensor* comparison" approach. What we intended to do is to evaluate the error due to the RS80 pressure bias arising from conventional measurements without a GPS sensor. Such an error evaluation should be done by the "comparison on pressure (or altitude or geopotential height) levels," not by the "simultaneous *sensor* comparison". Namely, the errors should be evaluated by subtracting the biased profile on biased zPTU from the corrected profile on "correct" zGPS.

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.

As discussed in General Reply, there are two comparison methods for two simultaneous radiosonde sounding data, i.e., "simultaneous *sensor* comparison" and "comparison on pressure levels." Indeed, Stauffer et al. (2014) showed the vertical shift of the profile in their Fig. 10. However, Stauffer et al. used the "simultaneous *sensor* comparison" method for the evaluation of ozone mixing ratio errors (in their Fig. 7). Our purpose is to evaluate sounding error associated with RS80 pressure biases in conventional sounding data, and the approach of Stauffer et al. (2014) is not appropriate for this purpose. The "comparison on pressure levels" method is useful for the researchers who are considering to make corrections and data homogenization of conventional sounding data without GPS sensors.

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.

The recommendation by Stauffer et al. (2014) is good for the future soundings.
As described in General Reply, our purpose is to evaluate sounding error associated with RS80 pressure biases in conventional sounding data launched without GPS sensors. Such an evaluation can be done if we assume that the statistical characteristics of radiosonde pressure biases in historical datasets do not change over time and location. Of course, we understand that it may be difficult to confirm that the statistical characteristics were same for all soundings. However, what we intend to do is to show the importance of the profile shift due to pressure biases. To show this point more clearly, we will also apply our correction method to one of the SOWER sounding data obtained without GPS sensors. The results will be added in the revised manuscript.

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.

We will add a description, “and such a high-performance hygrometer is necessary for this analysis.” into the first paragraph of Section 4.3. Also we will add the following notes into the last paragraph of Section 4.3, “It should be noted that the results are based on approximately 20 m of the altitude offset near the cold-point tropopause. This value is comparable to the uncertainty of the geopotential height and also to the uncertainty of the GPS sensor. Also, the uncertainty of CFH measurements is estimated to be 9% or smaller in the UT/LS (Vömel et al., 2007). This value is larger than water vapor offset due to the pressure biases of RS80."

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.

As described in General Reply, this can be done if we assume that the statistical characteristics of the radiosonde pressure biases in historical datasets do not change over time and location. We believe that this study will make an important contribution to improve historical data without GPS sensors.

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?

We examined how the pressure bias would change if we do not use a cutoff of 1.5 hPa to exclude 5 soundings data. As a result, the estimated pressure bias is -0.4 +/- 0.2, -0.4 +/- 0.1, and -0.4 +/- 0.2 hPa at 20, 25, and 30 km, respectively. Thus these values do not differ significantly from our previous results. These values are also still significantly different from the result by Stauffer et al. (2014), which are about 1 hPa.

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.

The differences (dT) depend on the vertical shift and the temperature gradient, the latter of which is largely controlled by gravity waves. The difference of this example is incidentally small below 28 km and it is comparable or smaller than uncertainty in the RS80 manufacturer's specifications. The profile of dT of each sounding is quite different for different soundings, but the mean difference for all soundings has been shown in Fig. 4.

In addition to above revisions, we will replace "geometric altitude" with "geopotential height" for vertical coordinate to accommodate our results to conventional data sets.

REPLY TO COMMENTS BY REVIEWER #3

We are grateful to your thorough reading and constructive comments on our manuscript. We believe we have incorporated all aspects pointed out. For your major comments, we have prepared "General Reply" as a separate document. Please read it first. The detailed description on the revisions follows:
(Authors' comments and responses in red.)

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 #3
Received and published: 21 April 2015

Review for Inai et al. (2015), "Altitude misestimation caused by the Vaisala RS80 pressure bias and its impact on meteorological profiles"

Synopsis:
This manuscript by Inai and coauthors takes an approach similar to that of Stauffer et al. (2014) to radiosonde pressure biases and errors, and how they affect dependent atmospheric variables such as geopotential altitude and O3 mixing ratio. Inai et al. analyzed 30+ soundings of SOWER radiosonde, ozonesonde, and cryogenic frost point hygrometer data and discovered RS80 radiosonde pressure biases of -0.4 ± 0.2 hPa in the stratosphere. The pressure biases led to errors in geometric altitude of hundreds of meters, and errors of a few percent in O3 mixing ratio and water vapor mixing ratio. Temperature errors of about -0.2 to -0.3 K were found in the stratosphere resulting from the profile shift caused by the pressure bias.

Summary of Recommendation:
Much space is spent discussing the methodology, which already exists in published literature (e.g. Inai et al., 2009; Stauffer et al., 2014), for correcting radiosonde biases using GPS data. Thus, there not much new information presented here. The one unique aspect of this paper appears to be the choice of the vertical coordinate when reporting measurement errors. Inai et al.

attempt to put the results of their study in the context of reported trends in O3, temperature, and water vapor in existing literature, but do not carry out potential applications of their results. These applications would need to be executed to make this work publishable. The pressure bias of RS80 radiosondes has been quantified in several recent publications. The Inai et al. manuscript provides additional motivation to the radiosonde/ozonesonde community for reprocessing of data, and analysis of stratospheric water vapor measurements from frost point hygrometers. However, other than Inai and coauthors' choice of vertical coordinate, this paper offers no real unique contributions to the study of radiosonde pressure biases. This paper in its current form requires substantial additions to the text and analyses
if it is to be published. This manuscript is not yet suitable for AMT given the lack of unique contributions to this area of study. Major issues involve further expanding upon unique aspects of this paper and differences between this manuscript and Stauffer et al. (2014), execution of potential analyses implied by the Discussion section of this paper, and explicit recommendations to the radiosonde/ozonesonde community to reprocess data sets.

Major Comments:

1) One of the major goals of Stauffer et al. (2014) is to motivate the reprocessing of ozonesonde data when GPS measurements are available. Stauffer et al. (2014) demonstrate improvements from recalculating dependent variables based on pressure derived from GPS altitude. To reprocess ozone and radiosonde data in Stauffer et al. (2014), coincident measurements are considered and a recalculation of variables in the data files themselves is performed. Stauffer et al. (2014) then present biases as a function of GPS altitude, which undergoes no changes during reprocessing. Conversely, in the present study, Inai and coauthors map data from the GPS and radiosonde to the same altitudes to derive biases. The methods here and in Stauffer et al. (2014) are nearly identical as stated by the authors in Section 3, except when the unique aspect of the Inai et al. study – the mapping of GPS and radiosonde measurements to the same altitude – is introduced. This altitude mapping apparently leads to most of the differences in reported biases between Stauffer et al. (2014) and the present study. Regardless of how the biases in radiosonde data are reported, the goal of motivating reprocessing radiosonde data sets with GPS data must be clearly and explicitly stated in this paper.

As described in General Reply in details, there are two comparison methods for two simultaneous radiosonde sounding data, i.e., the "simultaneous *sensor* comparison" and the “comparison on pressure (or altitude or geopotential height) levels.” While Stauffer et al. used the former method for the evaluation of ozone mixing ratio errors (i.e., their Fig. 7), the latter method is useful for the researchers who are considering to make corrections and data homogenization of conventional sounding data without GPS sensors. Our purpose is to evaluate sounding errors associated with the RS80 pressure biases in conventional sounding data. Based on the General Reply, we will revise Introduction to convey our motivation more clearly.

2) Section 3 should be shortened given that the pressure recalculation techniques are already found in multiple publications. Section 2 and 3 could also be grouped into a combined “Data/Methods” section.

Eq. 2 will be omitted, and sections 2 and 3 will be combined.

3) There are figures in the Inai et al. paper that resemble those published in Stauffer et al. (2014). Figure 2 in this study is similar to the RS80 panel of Figure 4 in Stauffer et al. (2014). Figure 5 in this study is a replication of the style of Figure 10 in Stauffer et al. (2014). However, mapping of GPS and radiosonde measurements to the same altitude makes these unique (e.g. Inai et al. Figure 5, right panel) and acceptable for eventual publication.

Thank you for the comment. In their Fig. 7 which is the key figure described in their Abstract, they showed errors arising when “biased” mixing ratio is plotted on “correct” altitude. This error calculation is equivalent to the “simultaneous *sensor* comparison”. In contrast, we are trying to evaluate observational errors for conventional soundings, which has to be done by the “comparison on pressure levels” as explained in General Reply.

4) The stratospheric pressure bias of the RS80 is reported as approximately -1.0 hPa in Stauffer et al. (2014) (Figure 4, RS80 panel), which is cited in the current study and is much larger than the pressure bias reported in this study (approximately -0.4 hPa). The authors of this study quality checked the data, and eliminated 5 outlier profiles from analysis before presenting it here. That would contribute to differences in reported pressure biases compared to Stauffer et al. (2014). The authors compare results with the Stauffer et al. (2014) study which applied no initial corrections. This difference should be described.

We have examined how the pressure biases change if we do not use a cutoff of 1.5 hPa to exclude 5 soundings data and if we do not make our QC. As a result, the estimated pressure biases are -0.3 +/- 1.0, -0.3 +/- 1.1, and -0.4 +/- 1.0 hPa at 20, 25, and 30 km, respectively. Thus the values do not change significantly from our previous result. They are also still significantly different from the result in Stauffer et al. (2014), which is about 1 hPa. This information will be added to the text.

5) The authors should make clear the RS80 pressure biases are not constant with altitude, particularly in the troposphere. This is implied when the authors apply a constant pressure correction as described in the Appendix, which does not eliminate pressure biases. This shows that one cannot simply input a surface pressure correction prior to launch that will fix the entire profile for RS80 radiosondes. This should be discussed in the paper.

We will add a statement about a surface pressure correction as: "we may not be able to eliminate the RS80 pressure biases using a constant pressure correction because the RS80 pressure biases may not be constant with altitude particularly in the troposphere" in the text.

6) The discussion in Section 5 provides the motivation for the analyses necessary to make this paper impactful. At present, Section 5 mostly reviews recent trends papers, and includes one concluding sentence that briefly mentions the subject of data reprocessing/ correction. Analyses and comparisons between existing trends papers and trends calculated using reprocessed data should be expanded upon in the text and figures. These results would be of great interest to the ozone and TTL water vapor trends community.

We agree with this comment that further effort to include trend analyses using reprocessed data makes this paper more impactful, but it is not easy. Instead, one example of correction for sounding data without a GPS sensor will be added in Section 5. Also, the following recommendation to the radiosonde/ozonesonde community will be added: "In this study, we have proposed a useful method to correct the sounding data without GPS sensors. We recommend for the radiosonde/ozonesonde/water vapor sonde communities to apply this method to historical datasets to improve our understanding of the stratospheric temperature, ozone, and water vapor trends."

7) These data are of interest to the ozonesonde community, particularly since this study presents evidence of necessary reprocessing. Are these data publically available? The SOWER website does not appear to make the data available (http://sower.ees.hokudai.ac.jp/data.html).

Currently, the SOWER campaign data during 2010 to 2014 are available upon request. All SOWER data will be publicly available without request in the near future.

Minor Comments/Corrections:

Is it correct to say that because of the altitude mapping, and the fact that the radiosonde pressure altitude is almost always higher than GPS in the stratosphere, that there are radiosonde observations near the top of the profile with which there are no GPS data to compare?

Yes, it is. Our comparison method can be applied up to the top of the GPS altitude.

Page 2194, Line 19-23: Are you suggesting an attempt to correct data without GPS information? If so, how might this be accomplished?

As described in General Reply, this can be done by the "comparison on pressure levels" approach if we assume that the statistical characteristics of the radiosonde pressure biases in historical datasets would not differ from those in this study. We will actually correct a sounding data obtained without GPS sensors and the results will be added into revised manuscript.

Page 2195, Line 21-22: This sentence belongs in the introduction section.

We will move this sentence to Introduction.

Page 2197, Line 8-10: The 1 minute data smoothing is another distinguishing characteristic from the Stauffer et al. (2014) study that should be pointed out. Stauffer et al. (2014) presented much of the results in 1 km median values due to noisy data; the 1 minute smoothing employed here appears to produce a similar effect.

Following this comment, we will add the following notes, "The +/-1 minute smoothing is different procedure from Stauffer et al. (2014) who used 1 km median value due to noisy data.

If we assume that the typical ascending speed of soundings is 5 m s^{-1}, the vertical resolution of our smoothing is about 600 m which is somewhat smaller than that of Stauffer et al."

Page 2197, Line 16-20: The RS80s in Stauffer et al. (2014) were launched from 2005- 2011, with no bias dependency on launch year (used as a proxy for production date) detected. In that work, the RS80 pressure biases were almost universally negative in the stratosphere.

Thank you for the information. We will confirm this information with Dr. Stauffer and will add this to the text.

Page 2198, Line 18-21: The relationship between the altitude shift, the change in the variable of interest, and the local gradient in the variable of interest makes sense, and is a good point that should be retained.

Thank you for your comment.

Page 2200, Line 20-22: The method with which one calculates O3 mixing ratio or any of the errors reported here is ultimately somewhat subjective and reflects the intended use of the results. Again, motivation should be the reprocessing of data, which requires consideration of coincident measurements.

We agree with this comment. As described in General Reply, our motivations include the reprocessing of historical data. We will describe this in Introduction and Summary.

Page 2201, Line 15 (Figure 7): What was the altitude difference at balloon burst for BI048? It looks almost negligible. Are there other cases with chilled mirror water vapor measurements that have a larger bias that might be more suitable for this figure?

This is because the frostpoint temperature data are not good above 26.5 km. We think that this case is appropriate to explain the altitude shift up to around this height.

Page 2206 Section 6 (Summary): This is essentially a copy of the abstract. Some parts of Sections 5 and 6 could be combined to provide better discussion and motivation for reprocessing ozone and radiosonde data.

We will revise our manuscript as suggested. We will shorten the Abstract and add our motivation and the different point from the Stauffer et al.'s approach more clearly in Summary.

In addition to above revisions, we will replace "geometric altitude" with "geopotential height" for vertical coordinate to accommodate our results to conventional data sets.