Reply to the Editor

Thanks very much for Dr. Michael Earle to review the revised manuscript; below we give the reply to the comments.

Comments:

Title

1. Propose changing title to 'Correcting precipitation measurements from MPS TRwS204 automatic weighing gauges in the Qilian Mountains, China'

Reply:

We agree with Dr. Michael Earle's proposal and we will make changes in the next revision.

Abstract:

1.The abstract should be brief, but still needs to introduce necessary background material. The gauges included in the study must be introduced (e.g. MPS TRwS204 is an automatic weighing gauge, CSPG is the manual Chinese Standard Precipitation Gauge) and the 'existing adjustment function' should be elaborated upon. Further, it is stated that 'deriving adjustment algorithms has become a top priority,' but there is no mention of what these algorithms are adjusting for (e.g. wind - induced undercatch of precipitation). The abstract should be revised to include the above points.

Reply:

We agree with Dr. Michael Earle that the abstract needs to introduce necessary background material, and we will make modifications in combination with the proposed technical revisions.

2. When considering the results after adjustment, the following statements are made: 'It seems that the adjustment function is more appropriate to correct the snowfall measurements than rainfall and sleet measurements for this dataset.' This makes sense given the results presented, but only considers the average loss relative to the reference. What about the Root Mean Square Error? What about the Bias? It is stated that 'Overall, the results of the correction are not ideal,' but this statement is based only on the average loss, which could be impacted by a small number of events with larger losses relative to the reference. The assessment approach should be expanded, as will be discussed further in subsequent comments.

Reply:

We agree with Dr. Michael Earle that the statements were not very serious and there was no comprehensive consideration. We will expand the assessment approach in the next revision.

3. It is stated that 'so many factors seem to affect the differences between measurements,' but only two factors are noted (orifice area and wind profile). Other contributing factors should be described, or this sentence should be reformulated.

Reply:

We are going to change it to 'factors that orifice area, wind profile and random errors seem to affect the difference between measurements of or $CSPG_{SA}$ and $TRwS_{SA}$.'

4. The final sentence in the Abstract would be much stronger and more broadly applicable as 'These types of errors must be considered when correcting precipitation measurement errors for different gauge types and configurations.'

Reply:

We agree with Dr. Michael Earle, and we will change it.

A) Introduction

1. What is meant by "false" precipitation? (P2, L12)

Reply:

According to Bogdanova et al. (2002), "false" precipitation means that snow raised from the surface of the snow cover and caught by the gauge during blowing snow of blizzard.

2. It is stated that 'the transition from manual to automatic measurements was highly encouraged' by the SPICE IOC (P2, L25 - 26) – can you please elaborate on this? If I recall correctly, SPICE was organized in response to the transition to automation, not to advance or recommend this transition.

Reply:

It could be that I misunderstood, and I will delete this sentence.

3. The Introduction transitions abruptly from a discussion of errors and adjustment functions to a discussion of manual vs. automated measurements (P2, L25 - 32). This information is valuable, but seems out of place here. The biases in gauge measurements are assessed relative to reference measurements; historically (e.g. the first WMO Solid Precipitation Intercomparison), the reference measurements were manual measurements using the DFIR. I suggest that the authors revise the Introduction to first describe these biases (they are presently introduced without context), then describe known biases (e.g. for automated vs. manual measurements), and then get into the different errors/contributing factors and adjustment functions. In short, the Introduction should establish the context for interpreting the results that will be presented, and should flow logically from topic to topic.

Reply:

We agree with Dr. Michael Earle that the logic of this section is not very smooth, and we will consider the suggestion offered by Dr. Michael Earle and revised the introduction.

4. The transition from a discussion of biases between automatic and manual measurements to the statement 'Thus, intercomparisons at different sites around the world should be conducted to the test the performance of the automatic system and correct the precipitation measurements' is confusing. Why would testing at different sites around the world be helpful? It is difficult to follow the logic of this section, as currently presented.

Reply:

We agree with Dr. Michael Earle that it transit abruptly in this section, and we will revise this section. The sentence "Thus, intercomparisons at different sites around the world should be conducted to the test the performance of the automatic system and correct the precipitation measurements" will be deleted.

5. While it is true that the SPICE intercomparison sites could have their own measurement objectives, it is not necessary to state this here (P3, L1 - 2), as the Qilian Mountains site was not a formal intercomparison site.

Reply:

We agree with Dr. Michael Earle, and this sentence will be deleted.

6. Which existing adjustment function? (P3, L7 - 8)

Reply:

It refers to the adjustment function from Kochendorfer et al. (2017). We will revise this sentence.

B) Materials and Methods

1. Is the DFAR configuration (with TRwS204 gauge) at the Qilian Mountains site used in the analysis? If not – which I believe to be the case – the DFAR configuration does not need to be introduced and discussed on P3, L23 - 27 (i.e. these two sentences can be deleted).

Reply:

The DFAR configuration (with TRwS204 gauge) at the Qilian station was not used in this study, and we agree with Dr. Michael Earle to delete these two sentences.

2. In Figure 1, it appears that the single - Alter shield slats on the $TRwS_{SA}$ (Figure 1b) are installed differently than those on the $CSPG_{SA}$ (Figure 1c). Those on the $TRwS_{SA}$ are oriented with the flat side of the slat toward the gauge (correct), while those on the $CSPG_{SA}$ are oriented with the flat side of the slat away from the gauge (incorrect). The two shield configurations are therefore not identical. The location of the centre of mass and distribution of the slat surface area will be different in each case, impacting how the slats respond to a given wind speed. The shields being identical is an important assumption in the assessment, so this difference should be noted in the manuscript.

Reply:

First, we must admit that such mistake did occur during installation. As far as I know, there was no professional guidance on early installation so that we didn't notice this problem timely. The right single-Alter shield slats have been replaced in June 12, 2016. This difference will be noted in the manuscript.

3. How were the $TRwS_{SA}$ measurements adjusted to match the diameter of the CSPG? You indicate that the manufacturer changed a setting (P4, L10), but additional details would be helpful.

Reply:

The transfer function for TRwS to match the diameter of the CSPG is:

$P2 = (P1/200cm^2)*314\ cm^2$,
where P2 refers to the converted precipitation of TRwS and P1 refers to the original precipitation of TRwS. This work was done by the instrument company.

4. In the manual quality control and filtering process, are you referring to the mean 30 min humidity? The details should be provided to guide those who may want to use a similar procedure. Also, it makes sense to remove precipitation during clear sky periods (sunshine duration = 0.5 h), but the threshold duration value is not very strict. For example, if the sunshine duration was 0.49 h, would the precipitation data be included in the analysis?

Reply:

We agree with Dr. Michael Earle and also feel sorry that we didn't make this section clear. We will change theses sentences to 'First, 30-min precipitation data were removed if the corresponding 30-min mean humidity was less than 50%. Second, 30-min precipitation data were removed if the corresponding 30-min sunshine duration was strictly equal to 0.5 h.'

5. It is stated that $TRwS_{SA}$ precipitation 'was compared with the reference precipitation to investigate the performance of the $TRwS_{SA}$ and correct its measurements.' This makes it sound like the comparison with the reference corrects the $TRwS_{SA}$ measurements, which is not the case. Perhaps it would be clearer to state simply that the $TRwS_{SA}$ measurements were assessed relative to reference precipitation measurements from the $CSPG_{DFIR}$? I don't know if it is necessary to mention the adjusted/corrected measurements and their assessment at this point.

Reply:

We agree with Dr. Michael Earle that this sentence is of some thoughtlessness, we will change it to 'Precipitation recorded by the $TRwS_{SA}$ was assessed relative to reference precipitation from the $CSPG_{DFIR}$.'

6. Again, no information is provided to indicate what is meant by "false" precipitation (P4, L30).

Reply:

We will change it to 'no "false" precipitation caused by blowing snow flux into the gauge (Bogdanova et al., 2002)'.

7. The reasons for not deriving transfer/adjustment functions from the experimental dataset and motivation for using the transfer function developed by Kochendorfer et al. are not clearly articulated. It is stated that 'it seems difficult to derive a valid and robust transfer function for $TRwS_{SA}$ using the dataset at this site during the experimental period.' Why is this difficult? What are the limitations of the dataset? Several important points were raised during the previous review stage and discussion, which should be reflected in the manuscript.

Reply:

We agree with Dr. Michael Earle that the reason for deriving adjustment functions from the experimental dataset and motivation for using the transfer function developed by Kochendorfer et al. are not clearly articulated. We will revise this section as 'However, because of rather small precipitation data set (especially for sleet and snow events) and limited meteorological conditions (lower mean wind speed) at this site during the experiment period, it is difficult to derive a valid and robust transfer function for $TRwS_{SA}$ using this data set.'

C) Results and Discussion

1. Numerous (20) 12 - h precipitation events were noted in which the $TRwS_{SA}$ did not report precipitation, but the $CSPG_{DFIR}$ reported precipitation. Four events were rain and sleet, during which the conditions were 'nothing special'; can you please reword and elaborate on this? 16 events were snowfall events, which were evidently characterized by lower temperatures. Do you have a theory to explain why this may have been the case? What were the characteristic wind speeds? For example, if the precipitation was light and the wind speeds were higher, it would not be surprising if the single Alter shielded gauge missed the event. Did the $CSPG_{SA}$ report precipitation during these events?

Reply:

This sentence "For rainfall and sleet events, …… were nothing special" should be revised or be deleted, because few rainfall and sleet events were missed by $TRwS_{SA}$, it is hard to find features of environmental conditions. The theory to explain why these 16 snowfall events were evidently characterized by lower temperatures was investigated in the next paragraph. 12-h mean wind speeds at gauge height varied from 0.4 to 1.7 m s^{-1} for these 16 snowfall events, and the distribution was uniform. As presented in P5, L29 that "During the experiment, 304 precipitation events (12-h scale) were measured by $CSPG_{DFIR}$ (also by $CSPG_{SA}$)", the $CSPG_{SA}$ reported precipitation during these events.

Another important concern is whether the 12 - h conditions are representative of the conditions during which precipitation actually occurred during a given 12 - h period. There's not necessarily a better way that you could have addressed the conditions, but the representativeness of conditions is an important point that must be noted.

Reply:

In fact, in the section "Losses of $TRwS_{SA}$ Relative to Reference Precipitation", we included 304 precipitation events (12-h scale) which contain these 20 events missed by $TRwS_{SA}$. Because of no precipitation recorded by $TRwS_{SA}$ for these 20 events, it was hard to know specific precipitation time. Therefore, mean wind speed during precipitation for these 20 precipitation events can not be calculated. In this section, 12-h mean wind speeds were used for unity. In the sections "Adjustment for $TRwS_{SA}$ Measurements" and "Discussion on Specific Errors of $TRwS_{SA}$ Measurements", mean wind speeds at gauge height during precipitation at 12-h scale were used.

2. When discussing precipitation losses (e.g. P6, L9), it is important to specify what the losses are relative to (i.e. the reference configuration).

Reply:

We agree with Dr. Michael Earle, it may be changed to 'the losses of the recording electronic weight precipitation gauge relative to the standard non-recording Hellmann gauge.'

3. Snow accumulating on the orifice and sublimating is proposed as a loss mechanism for the $TRwS_{SA}$. Were any incidents observed during the experimental period in which snow accumulated on the orifice, or is this just a theory? Any accumulated snow could also prevent incident snowfall from entering the orifice and being measured – that is, capping of the gauge may occur – which would influence the assessment.

I would assume that the CSPG is not heated, either. Did you see any snow accumulation on the

CSPG? If so, is it included in the manual measurement, or is it removed prior to the measurement? Any differences in how precipitation accumulated on the gauge orifice is dealt with for the different gauge types could potentially impact the assessment.

Is there any other reason why accumulation/sublimation would be an issue for the $TRwS_{SA}$ and not the CSPG gauges? It is stated that accumulation/sublimation 'may explain most of the situations in which the $TRwS_{SA}$ recorded values of 0 for snowfall events,' but the reasons why this does not occur for the CSPG gauges are not discussed.

Reply:

Snow accumulating on the orifice and sublimating is just a theory. However, when I consulted the observer, he said that almost no accumulated snow on the orifice of TRwS and CSPG was observed in winter at this site. Obviously, I made a false theory. I will revise this part. Additionally, the manual CSPG is not heated.

4. Is data loss considered to be another precipitation loss mechanism for the TRwS gauge? If the gauge reports are based on the weight of accumulated precipitation, won't the reports following any periods of data loss include any precipitation accumulated in the bucket during the loss period?

Reply:

Yes, data loss was considered to be another precipitation loss mechanism for the TRwS gauge in this version of manuscript. The gauge reports were based on the weight of accumulated precipitation, however, the output of depth failed to work while the output of weight still worked for these 20 precipitation events. Because the output of weight worked normally, it can't be the problem of electricity shortage. We finally thought this may be the internal data processing problem.

5. The title of Table 1 indicates a comparison of events on a 12 - h scale; however, the notes indicate daily (24 - h) mean wind speeds and temperatures are presented. Is this correct? As noted above, the conditions during 12 - h periods are not necessarily representative of the conditions during which precipitation occurred. This issue will be more significant if conditions over 24 - h periods are used.

Reply:

I am sorry that I forgot to change "daily (24-h)" to "12-h", and the data in Table 1 are all at 12-h scale. Because 304 precipitation events (containing 20 events which missing precipitation data for $TRwS_{SA}$) were included in this section, we used 12-h mean meteorological variables for unity.

6. I would consider removing the statement 'It is obvious that the $CSPG_{SA}$ performed better than the $TRwS_{SA}$ during the experiment at this site' (P6, L32). This is a subjective statement. Alternatively, it could be changed to something like, 'It is evident that the $CSPG_{SA}$ collected more precipitation relative to the $TRwS_{SA}$ during the experiment at this site.'

Reply:

We agree with Dr. Michael Earle that this statement is subjective, we will change it.

At the low mean wind speeds experienced at the site over the experiment (Table 1), the differences between measurements from a single - Alter shielded gauge and those from a

DFIR - shielded gauge may not be significant. In this case, the $CSPG_{SA}$ and $CSPG_{DFIR}$ measurements would be expected to be very similar, and any differences relative to the $TRwS_{SA}$ could be attributed to systematic differences in the sampling, principle of operation, aerodynamic profile, etc. between the gauge types. These systematic differences are addressed in the $TRwS_{SA}$/$CSPG_{SA}$ comparison.

Reply:

We will add the analysis of the difference between $CSPG_{SA}$ and $TRwS_{SA}$ measurements. As Dr. Michael Earle stated, the aerodynamic differences between $CSPG_{SA}$ and $TRwS_{SA}$ would be expected to be small in such a small wind speed range. The difference between $CSPG_{SA}$ and $TRwS_{SA}$ measurements in this study may be attributed to difference in sampling, operating principle, etc. between the gauge types.

7. It is stated that 'the performance of the $CSPG_{SA}$ was more stable than that of the $TRwS_{SA}$' (P7, L13 - 14), but this comparison does not acknowledge that the CSPG is a manual gauge (how can it be unstable?), or define what is meant by 'stable.' Is this statement made in reference to the lower standard deviation for the $CSPG_{SA}$? If so, when considering measurements over such long time scales (12 - h), does the standard deviation really say anything about noise or signal variability?

Reply:

We agree with Dr. Michael Earle that this statement "the performance of the $CSPG_{SA}$ was more stable than that of the $TRwS_{SA}$" is subjective. Here, I did make this statement in reference to the lower standard deviation for the $CSPG_{SA}$. As Dr. Michael Earle mentioned, measurements over such long tome scales (12-h) can not say anything about the noise or signal variability. I will delete this statement.

8. Aside from wind speed and temperature, which other 'meteorological variables have relationships with the catch ratio' (P7, L17)? Also, when noting that air temperature and wind speed are 'typically' applied in adjustment functions, it would be valuable to include supporting references.

Reply:

Aside from wind speed and temperature, humidity also has relationship with the catch ratio. I think it may be more better to change "Although most meteorological variables have…" to "Although several meteorological variables have…". We agree with Dr. Michael Earle that it would be valuable to include supporting references when noting that air temperature and wind speed are typically applied in adjustment function. It will be added in the next revised version.

9. The trends described for the catch ratio as a function of wind speed and temperature in Figure 3 (text on P7, L22 - 24) are difficult to observe in the plots. Binning the results for each precipitation type (rain, sleet, snow) and plotting as box and whisker plots would provide a much clearer representation of the data trends, and are recommended to complement the scatter plots provided in Figure 3.

Reply:

We agree with Dr. Michael Earle's suggestion, and we will add this kind of pots in the next revised manuscript.

10. RMSE values are computed for observed catch ratios relative to adjusted values. It would be valuable to also include RMSE values for observed $TRwS_{SA}$ accumulation values relative to the adjusted values.

Reply:

We agree with Dr. Michael Earle's suggestion, and we will compute the RMSE values for observed $TRwS_{SA}$ accumulation values relative to the adjusted values in the next revised manuscript.

11. On page 8, the performance of the adjustment functions is assessed in terms of average precipitation losses in each precipitation type. This assessment should be complemented by RMSE and bias values for the observed $TRwS_{SA}$ accumulation values relative to the adjusted values. In the work of Kochendorfer et al., it was found that the adjustments improved the bias in value relative to the reference, and had less of an impact on the RMSE; it would be interesting to see if similar trends are observed using this experimental dataset.

Reply:

We agree with Dr. Michael Earle's suggestion, and we will add the content of analysis of RMSE and bias values for the observed $TRwS_{SA}$ accumulation values relative to the adjusted values.

12. Can you please elaborate on the 'other possible errors' that may contribute to differences between $CSPG_{DFIR}$ and $TRwS_{SA}$ measurements (P9, L4 - 5)?

Reply:

Errors caused by different aerodynamic profiles and orifice areas of TRwS and CSPG, and random errors.

13. Do you think that the similarity of the mean absolute differences between the $CSPG_{SA}$ and $TRwS_{SA}$ in all precip types (P9, L10) may indicate a systematic difference in measurements between these gauge configurations? That is, does the combined influence of errors specific to the $TRwS_{SA}$ result in a systematic offset in measurements relative to the CSPG?

Reply:

We agree with Dr. Michael Earle that the similarity of the absolute differences between the $CSPG_{SA}$ and $TRwS_{SA}$ in all precipitation types may indicate the difference in measurements between these gauge configurations. It may also relate to the most low mean wind speed during precipitation. Because the corrected $CSPG_{DFIR}$ measurements were regarded as reference precipitation, we prefer to believe that the combined influence of errors specific to $TRwS_{SA}$ mainly result in these differences.

14. In Figure 5, the cumulative sums of precipitation accumulations are plotted as a function of event number – this is effectively a time series of the total accumulated precipitation for each gauge at the end of each event. It would be far more instructive to plot the individual event accumulations (accumulation at end of 12 h period minus the accumulation at the start of that 12 h period) for the $TRwS_{SA}$ vs. those for the $CSPG_{SA}$ (or vice versa) as a scatter plot. That way, each event can be compared independently, irrespective of the precipitation accumulated in previous events. A 1:1 line (indicating perfect agreement between the gauges) can be added to illustrate the accumulation trends.

Reply:

We agree with Dr. Michael Earle, and I had some misconceptions here before. I will change this plot to scatter plots of $TRwS_{SA}$ vs. $CSPG_{SA}$ event measurements.

15. As presented, the results in Figure 6 are difficult to interpret. I recommend generating histograms of the differences between the $CSPG_{SA}$ and $TRwS_{SA}$ measurements for each precipitation type to more clearly illustrate the magnitude of differences between the gauges, and how those differences are distributed. Alternatively, the $TRwS_{SA}$ vs. $CSPG_{SA}$ scatter plots (see comment above) could be modified to include only points events with mean wind speeds < 1 m/s.

Reply:

We agree with Dr. Michael Earle that it is a little hard to see the mean absolute difference for each precipitation type since I combined all the precipitation events for gauge-height wind speed below 1 m s^{-1}. We will plot them separately in the next version.

16. As noted above, the $TRwS_{SA}$ and $CSPG_{SA}$ technically have the same shields, but the orientation of the slats is different (Fig. 1). This potentially complicating factor should be noted in the relevant discussion in Section 3.3.

Reply:

We agree with Dr. Michael Earle, and this issue will be elaborated in the relevant discussion in Section 3.3.

17. It is interesting that limiting the $CSPG_{SA}$/$TRwS_{SA}$ comparison to wind speeds below 1 m/s results in almost the same mean absolute differences between the measured values as observed for the full wind speed range. I wonder if this is a reflection of the full wind speed range being low (at least in terms of the mean wind speeds in Table 1), a reflection of the mean wind speeds for 12 - h periods not being representative, or an indication of differences/errors not related to the aerodynamic profile, as proposed.

Reply:

Here, we need to restate that the gauge-height mean wind speeds during precipitation at 12-h scale were used in section 3.2 and 3.3. As shown in Fig.3, the full mean wind speed during precipitation is about in the range of 0 to 3 m s^{-1}. However, the most wind speeds were low. In my opinion, such phenomena may be a reflection of the full wind speed range being low as Dr. Michael Earle stated. In this way, the difference/errors caused by aerodynamic effect can be small from the results for these two wind speed ranges. This should be stated in the manuscript, and we will elaborate on it.

18. The trends indicated in Figure 7 do not appear to be conclusive for sleet and snow, given the significant scatter and small numbers of events relative to rain (I expect the R^2 correlation values would be low). This caveat should be noted when making the statement, 'it may be inferred that the amount of precipitation mainly affects the specific errors of $TRwS_{SA}$ during the experiment at this site.'

Reply:

We will pay attention to this problem considering small numbers of sleet and snow events, and this kind of statement will be revised.

D) Conclusions

1. As noted above, I don't think that the standard deviation of losses is a representation of measurement stability; the losses are larger for the $TRwS_{SA}$, so the standard deviation of loss values will also be larger.

Reply:

We agree with Dr. Michael Earle that it may be not appropriate to compare the stability of TRwS and CSPG because of different operational methods. Additionally, as Dr. Michael Earle pointed, measurements over such long tome scales (12-h) can not say anything about the noise or signal variability. I have different opinions about the statement that 'the losses are larger for the $TRwS_{SA}$, so the standard deviation of loss values will also be larger'. When the differences were small between the losses for each event, the standard deviation of loss values can be small even if the losses are larger.

2. How is it 'clear' that the 'worse correction for $TRwS_{SA}$ measurements should not be attributed to the limited meteorological conditions and precipitation data during the experiment'? I don't believe that these points were addressed in the manuscript; an earlier comment requested more details in this regard.

Reply:

This has been addressed in P8, L20. In the third paragraph in P8, to verify whether the limited meteorological conditions would contribute to the poor correction for $TRwS_{SA}$ measurements, the same adjustment function was used to $CSPG_{SA}$ measurements. From the result, different correction effects occurred. Because the $TRwS_{SA}$ and $CSPG_{SA}$ experienced the same meteorological conditions, the presented correction effect should not be attributed to the limited meteorological conditions. We will revise this part and add more details.

3. I don't know if you are in a position to say that the adjustments are 'better' or 'worse' when the changes in loss values after adjustment for both gauges are within 0.2 mm for all precip types. You indicate that the measurement uncertainty is ignored (P10, L27), but what is the estimated uncertainty for each gauge type? Perhaps more important, you are using wind speeds over 12 - h periods for the adjustment that do not necessarily reflect the conditions during which precipitation actually occurred, which will impart significant uncertainty on the adjusted values. I think it is OK to state the results obtained, but any broader application of these results should be treated with extreme caution.

Reply:

Maybe the content about correction for $TRwS_{SA}$ and $CSPG_{SA}$ which put in the conclusion is not very appropriate. As presented in P8, the change of measurements of $TRwS_{SA}$ for all precipitation types after correction is -33.3 mm (in L10), while the change of measurements of $CSPG_{SA}$ for all precipitation types after correction is 20.2 mm (in L19). The correction for $CSPG_{SA}$ measurements seems to be better than correction for $TRwS_{SA}$ measurements. We will also combine the RMSE and bias values to discuss it. We agree with Dr. Michael Earle that any broader application of these results should be treated with extreme caution. We will check our wording again. Additionally, we agree with Dr. Michael Earle that the estimated uncertainty may be from the unrepresentative mean wind speed during precipitation at 12-h scale. I think that the sample size will also cause the estimated uncertainty.

4. The last two sentences on page 11 are very difficult to follow. Can you please rephrase these, including relevant results from the study, if possible, to demonstrate your points?

Reply:

In the work of Kochendorfer et al. (2017), the adjustment function Eq. (3) was derived for correcting wind-induced loss for single Alter shielded Geonor gauge, and its reference precipitation was the measurements of DFAR shielded Geonor gauge. In this study, the same adjustment function was used for $TRwS_{SA}$ measurements, but the reference precipitation was the corrected measurements from DFIR shielded manual gauge (CSPG). Because in addition to the wind-induced loss needing correction, the specific errors for $TRwS_{SA}$ were thought may also require to be corrected in this study. After applying the adjustment Eq. (3), we can find that it was no completely calibrated for $TRwS_{SA}$ measurements. This result seems to be consistent with the assumption we made before, so it can be reasonable.

References:

Bogdanova, E. G., Ilyin, B. M., and Dragomilova, I. V.: Application of a comprehensive bias-correction model to precipitation measured at Russian North Pole drifting stations, J. Hydrometeorol., 3, 700–713, doi: 10.1175/1525-7541(2002)003<0700: AOACBC>2.0.CO; 2, 2002.

Kochendorfer, J., Rasmussen, R., Wolff, M., Baker, B., Hall, M. E., Meyers, T., Landolt, S., Jachcik, A., Isaksen, K., Brækkan, R., Leeper, R.: The quantification and correction of wind-induced precipitation measurement errors, Hydrol. Earth Syst. Sci., 21, 1973–1989, doi: 10.5194/hess-21-1973-2017, 2017.

Finally, thanks again for Dr. Michael Earle spending his valuable time pointing out the problems and offering suggestions for this manuscript.