Response to E. Lanzinger

General Comments:

Large measuring uncertainties for solid precipitation measurements are a major issue in hydrology.

The paper „Errors and adjustments for single-Alter shielded and unshielded weighing gauge precipitation measurements from WMO-SPICE“ by John Kochendorfer et al. is clearly a substantial contribution to improved solid precipitation measurements.

Besides improved, robust correction formulea there is also an estimate of the remaining uncertaitiy given. Outliers in the results are clearly explained and hints for improvement are given.

The text is well stuctured and language, explanations, graphs and tables are very clear and don‘t need any change. I have only minor questions in some parts That I address below.

The intention of this paper is to provide a simple and practical method for correcting solid precipitation data by ancillary data that is available at most of the meteorological stations world wide.

The scientific significance, quality, and the presentation quality are therefore excellent.

Authors' response: Thank you very much!

Specific Comments:

P. 1, lines 23/24: Choose a more appropriate word for „differences“. What you mean here are „errors“ or „measuring uncertainties“. The word „difference“ is too neutral, i.e. different amounts of precipitation could indeed occur in different regions without being an error.

Authors' response: Thank you; "differences" has been replaced with "measurement biases" in the revised manuscript.

P. 2, line 4: In „...changes in the velocity of the air around the gauge…“ velocity and direction of the airflow could be added, as the air flow is bended around the gauge which is also leading to an uplift of light particles over the gauge orifice.

Authors' response: The word 'velocity' actually includes both speed and direction, but it has been replaced with "speed and direction" for clarity, and the word "airflow" has been included.

P. 2, line 19/20: Could you eventually find the original citations that served as a rationale for the WMO decision of 2010? Your citation is of 2012. you could use

Authors' response: This is a good point. Rasmussen et al. (2012) has been replaced with these: (Førland and Hanssen-Bauer, 2000; Goodison et al., 1998; Sevruk et al., 2009).

P. 2, lines 27-29: It is not understandable for everybody, what the term „WMO-SPICE weighing precipitation gauges“ means. I suggest two sentences: The focus of the work described below is on unshielded and single-Alter-shielded weighing precipitation gauges. Based on results of a previous CIMO survey (Nitu and Wong, 2010), WMO-SPICE selcted two weighing gauges which represent the two most ubiquitous configurations used in national networks for the measurement of solid precipitation.

Authors' response: The text has been changed accordingly.

P. 2, line 32: DFIR needs a citation.

Authors' response: An appropriate citation has been added (Goodison et al., 1998).

P. 3, lines 7-8: „Some of this variability is driven by differences in ice crystal shape (habit), mean hydrometeor fall velocity, and hydrometeor size...“. Actually hydrometeor fall velocity is a function of its size, shape and mass (or mass density). Therefore it should not be mentioned side by side with the other variables. Hydrometeor shape and density are very difficult to measure for each particle, and are therefore generally not available. But some disdrometers provide hydrometeor size and fall velocity. By using fall velocity you implicitely take into account crystal shape and mass density. Please check again the given citation (Thériault et al. 2012) for details.

Authors' response: This list of attributes may admittedly seem redundant, but the mass and surface area of a hydrometeor affect not only its fall velocity, but also its inertial tendencies. Two hydrometeors with the same fall velocity may respond differently to the flow disturbance around a gauge due to differences in their mass. A larger hydrometeor will more readily cross local streamlines around the inlet due to its increased inertia. This effect is described by Colli et al. (2015).

P. 9, line 3: „mixed precipitation was defined as...“

I wonder whether „mixed precipitation“ is a good term here, because you would expect a mixture of rain an snow for each of the events. I guess most of the events were snow only? Isn‘t the temperature used to distinguish between wet and dry snow? I would suggest „wet snow/mixed precipitation“ and for the colder regime eventually „dry snow.“

Authors' response: In this case temperature is being used to distinguish between precipitation types, because at monitoring sites where precipitation type is not measured, air temperature is the only available proxy for precipitation type observations. No differentiation between wet snow and dry snow is made in this manuscript, because below -2 °C the catch efficiency was relatively insensitive to air temperature (e.g. Figure B1 in the response to Helfricht's comment). The temperature region defined as mixed (2 °C ≥ T_{air} ≥ -2 °C) contains measurements that cannot with any degree of confidence be defined as either solid or liquid. It is indeed true that many of the individual 30-min events are not mixed precipitation, and are dominated by either rain or snow (eg. Kochendorfer et al., 2017; Wolff et al., 2015), but the set of 30-min measurements this temperature regime can justifiably be described as 'mixed' because it includes snow, mixed, and liquid precipitation. Wet snow/mixed precipitation would not describe it well, as it does include a significant amount of rain.

P.9, line 5: I suggest to delete the phrase „and the negligible magnitude of the liquid precipitation adjustment“, because for drizzle the CE in windy conditions is negatively affected and needs correction. What you might have in mind is that the corrections for snow are generally larger. But as the fall speed of small droplets and small snow particles are very close it is clear that both will be affected by wind in a comparable way.

Authors' response: It is indeed true that drizzle measurements can be underestimated due to wind, but within these WMO-SPICE measurements there was no significant undercatch of liquid precipitation. This is demonstrated by the similarity of the measurements adjusted using Eq. 3 and Eq. 4, as the liquid precipitation measurements were corrected using Eq. 3, and were uncorrected using Eq. 4. This has been clarified by re-writing as, "…the negligible magnitude of the liquid precipitation adjustment derived from these measurements".

P. 13, line 1: PE was improved by the application of transfer functions for all sites, except one: Sodankylä. It is really a very very small difference, but it could be mentioned that in a site that is well shielded by trees and where there are generally low wind speeds, a SA shield is already sufficient and no further correction is needed. As you mentioned, the correction curves are not precise at the low and high wind speed end. Could this be a reason, why the Sondakylä results with SA shield are getting a bit worse after correction? With the same argument, that you require to keep the correction constant for wind speeds higher than 7.2 m/s you could think about keeping the correction constant to 1 (or some value close to 1) for wind speeds lower than a threshold. By these two means you get closer to the sigmoid curves published elsewhere.

Authors' response: The following text has been added to the single-Alter results section, "At Sodankylä however, the single-Alter shielded measurements were not significantly improved by the adjustments, and the $PE_{0.1\ mm}$ values were actually slightly lower after adjustment. This indicates that at a field site such as Sodankylä, which was well sheltered from the wind in a forest clearing, such adjustments may not be necessary." It may indeed be possible that the Sodankylä results were affected by inaccuracies in the adjustments at low wind speeds. Our hope was that the inclusion of the many low wind speed measurements from Sodankylä would minimize this problem in the resultant transfer function. Indeed the value of the transfer function could be fixed at 1.0 near $U = 0$. The sigmoid function and the present Eq. 3 have been compared previously using a separate dataset from two sites, with no significant improvement to the sigmoid function detected (Kochendorfer et al., 2017). The two equations will not perform the same in all cases, and any equation is of course only as good as the data it has been fit to. However in the present case, based on the negligible differences between the adjusted and unadjusted measurements from Sodankylä, it does not appear that the added complexity of a lower wind speed threshold is warranted. In addition, without performing a detailed analysis, comparison of the Eq. 3 and Eq. 4 adjustments shown in Fig. 6, with larger predicted CE for the Eq. 4 curve at low wind speeds, does not indicate that differences in the low wind speed adjustments had a significant effect on the Sodankylä $PE_{0.1\ mm}$ values (Fig. 7 d).

Typos and linguistic comments:

P. 5 line 11: rationale

Authors' response: A definition of 'rationale' is available here: https://www.merriam-webster.com/dictionary/rationale

P. 5, line 15/16: „false precipitation error" sounds a bit like „false error". I suggest „false accumulation" instead.

Authors' response: Thank you very much; "precipitation error" will be changed to, "accumulation".

P. 6, line 2: „10 m wind speed". I suggest „wind speed at 10 m height"

Authors' response: Thank you very much; "10 m wind speed" will be changed to, "10 m height wind speed".

P. 8, line 4: threshold of 0.1 mm in 30 min was chosen… the 30 min could be added.

Authors' response: Thank you very much; this will be changed accordingly.

P. 17, line 1: ...reduce the horizontal wind speed impacting...

Authors' response: Thank you. This phrase has been reworded as, "which is to reduce the horizontal wind speed inside the shield".

References

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