General comments

Part II of, “Unshielded precipitation gauge collection efficiency with wind speed and hydrometeor fall velocity” is the experimental companion to the Part I paper, which describes a modelling experiment. Part II tests the transfer function created in Part I, and it goes further to modify this transfer function based on the experimental results. It demonstrates that hydrometeor fall velocity can be used in a practical way to improve the adjustment of unshielded precipitation measurements. These improvements are impressive and significant.

Like Part I, the manuscript is well-written and easy to follow, and it is definitely worth publishing.

Specific comments

Ln. 65 – 67. This is a misinterpretation of those results. In addition to the uncertainty of the adjustment, it overlooks the fact that adjusted measurements increase the magnitude of errors multiplicatively. For example, if the gauge measurement has an inherent uncertainty of 0.1 mm, with CE = 0.5, after adjustment the uncertainty will be doubled along with the measurement. Two single Alter gauges agreeing with each other with an uncertainty of 0.09 mm does not imply that they can be adjusted without increasing the uncertainty. I accept that there is significant room for improvement in our transfer functions, but I find it very difficult to believe that adjusted unshielded measurements will ever be as accurate as well-shielded measurements. I am afraid that someone reading between the lines here might take that to be the suggestion.

Ln. 112. Change, “using similar methodology” to, “using *a* similar methodology” or, “using similar *methods*.”

Ln. 172 and Eq (2). Why was *h* chosen for precipitation, instead of *P*?

Ln. 269 – 270. This makes me wonder about the details and physics of the POSS averaging. How is the hydrometeor fall velocity calculated by the POSS when there is mixed precipitation, and/or when there is significant variability in the types of hydrometeors simultaneously present? I am guessing that for the purposes of transfer functions, ideally the fall velocity would be representative of the total mass of water falling, but perhaps it is actually weighted towards the average by volume?

Ln. 289. I apologize in advance, because I hate it when reviewers ask me these types of questions, but how was the threshold fall velocity of 1.93 m s^{-1} selected?

Equation 7b. Given my comments on Part I this should come as no surprise, but I think that defining CE = 0.0 any under conditions is problematic.

Ln. 299. Clarify that CE_{HE2} decreases linearly with wind speed *at a given/fixed hydrometeor fall velocity*.

Ln. 299 – 300. Explain how this works in practice. How were measurements that occurred when fall velocity was defined as zero treated? Were they simply removed from the analysis? How is the user of these functions supposed to adjust such measurements?

Ln. 314 – 315, Figure 4 caption. Typo. I believe that the three occurrences of "u_p" in,"fall velocity u_p categories…" should be replaced with "u_f".

Ln. 352. Why wasn't the same temperature threshold technique used for $K_{Universal}$? At the risk of personifying a, "get off my lawn" attitude, I wonder how much of the improved performance of the K_{CARE} adjusted measurements were caused by large errors in measurements that were over-adjusted using $K_{Universal}$ above this temperature threshold? The largest improvement in RMSE includes some of these measurements, when T is between positive and negative 2 deg C (Table 8), and I am guessing that at least some of the very poorly measurements were warmer, larger events (Fig. 5b).

Ln. 504. A realistic vertical wind profile, with a zero-slip boundary condition at the Earth's surface, may be important for larger wind shields.

Ln. 507 – 509. I agree that it is difficult to accurately adjust measurements at windy sites, but the 'limitation' described here is entirely avoidable. The collection efficiency was defined as zero above 7.19 m s^{-1} by choice, not by necessity.