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Interactive comment

Interactive comment on "Using Snowfall Intensity to Improve the Correction of Wind-Induced Undercatch in Solid Precipitation Measurements" *by* Matteo Colli et al.

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As suggested by reviewer # 1, the new version of the paper has been strongly revised in order to improve the general organization of the discussion developed by the authors, the consistency of the terminology throughout the various sections of the manuscript and to provide answer to the various clarifications pointed out by reviewer # 1.

In the following, we provide specific replies with the italic font.

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Discussion paper

Response to reviewer # 1 (D. Bocchiola)

The manuscript is generally speaking of interest, given the importance of snowfall, and solid precipitation measurements for a number of purposes.

However, the manuscript as is requires large revision in my opinion, as follows

1) The text is not always clear, and at times awkward or even unintelligible. I report large suggestions for re-editing in the pdf files attached.

We agree that the text could be largely improved, and we revised many parts. However, reviewer n. 2 finds that "the paper is well written and clear" , so we are a bit puzzled with this comment. Various aspects of our work were deeply misunderstood by reviewer n. 1, and may be this has some basis in this comment, so we did our best to make our arguments clearer and intelligible even to the unexperienced reader.

The reported editing suggestions have been duly considered in the revised version.

2) The basic assumptions seem arbitrary. Why undercatch must only depend upon either on temperature or intensity. Can it depend on both? More generally, when a correlation/regression analysis is to be taken, a larger array of variables need be explored, and those influencing need be retained, with indication of the explanation power. The authors start with the idea that wind and intensity are the only affecting variables, but this should be better documented, and if other variables (e.g. temperature) provide more explanative power they should be included.

It is not clear which basic assumptions are those mentioned by the reviewer. The use of precipitation intensity as the major explanatory variable, beyond wind, is not an assumption but rather the thesis of this work. This is documented in the paper using *data analysis from field tests and demonstrated using numerical simulation.*

The reviewer seems to neglect the physical basis of the wind exposure problem and a large bulk of literature (widely referenced in the paper) aiming to derive collection efficiency curves from field experiments and/or numerical simulation. Wind is generally recognized as the main source of bias, and temperature was recently suggested as an additional explanatory variable, since it is related to the type and density of hydrometeors (Thériault et al., 2012; Colli et al., 2015; Wolff et al., 2015; Kochendorfer et al., 2017a). However, while the wind speed or wind speed and temperature approach is generally effective at reducing the measurement bias, it does not significantly reduce the Root Mean Square Error (RMSE) of the residuals, possibly implying that part of the variance is still unexplained. In this study, we show that using precipitation intensity as an explanatory variable significantly reduces the scatter of the residuals.

Indeed, considering only snowfall events with T less than -2° C, we show in Figure 1 that the collection efficiency depends on the snowfall intensity (bottom panel), while no significant correlation is evident with temperature (upper panel). This visual impression is further investigated in the paper, and Figures 5, 6, 7 show that after correction based on CE(Uw, T) curves (bottom panels) an evident dependency of the residuals on SI and an high RMSE remain, while using the CE(Uw, SI) curves the remaining information associated with T is lower and appears quite random.

Furthermore, the "idea" of using precipitation intensity as an explanatory variable has a physical basis (also demonstrated in the paper using CFD simulations) since it provides information about the particle size distribution, and this was shown in previous works to affect the collection efficiency of snow gauges (Thériault et al., 2012; Colli et al., 2015).

Technically use of RSME does not indicate goodness of fit, which is best given by r2, the authors mix the two, so it is difficult to make comparisons.

No mix of the two parameters is made in the paper. We used r^2 to indicate the goodness of fit of the proposed interpolation functions with respect to the experimental dataset, as reported in Tables 1 and 2. We used the RMSE value as an indicator of the scatter of the residuals after correction is applied using different CE curves (based on SI or T), i.e. of the amount of the stochastic variance still present in the data. Note that $RMSE = \sigma \cdot \sqrt{1 - r^2}$. We checked that the information is provided consistently throughout the paper in the revised version.

3) Also, undercatch as defined seemingly depend by construction upon intensity...was this considered? Am I wrong?

The undercatch is the normalized difference between the measured and the real precipitation, therefore equal to CE -1. This depends by construction on the precipitation amount, and therefore on the average precipitation intensity over any aggregation interval Δt. Our point is that the undercatch depends on the instantaneous precipitation intensity (in the limit when Δt tends to 0), which is directly linked to the particle size distribution. The use of the average intensity over a given aggregation interval (e.g. 30 minutes) is needed for practical calculation of the observed undercatch from experimental data. In the CFD simulations, the instantaneous intensity is used and calculation of the undercatch is possible at a much finer aggregation, therefore we say that it demonstrate our point beyond the variability that is typical of field measurements.

4) The CFD part is utmost unclear...the authors refer heavily to other studies, but so doing this part does no clarify or add anything visible. This CFD part needs heavily to be tightened.

We agree that the CFD part could be reorganized in the manuscript to better focus on the results that are supportive to our main point. This has been done in the revised manuscript by moving everything related to the CFD into a single section in the second part of the paper.

However, the CFD part is central to our thesis since it clearly demonstrate that precipitation intensity can be used to introduce the particle size distribution (PSD) information into the collection efficiency curves. Using CFD simulations, different PSDs could be tested and the collection efficiency evaluated based on the associated precipitation intensity. This allowed testing the proposed hypothesis in a simplified environment where the noise that is typical of experimental datasets is avoided. The results show a good agreement of the CE values with field data and a clear dependency on the SI. In particular, Figure 11 – that is central to our work – shows that the curves obtained from the CFD analysis (panel a) are able to describe the deviation from linearity (constant CE) in the SISA-SIREF graph for different wind classes, as observed in the field data (panels b, c, d, e).

5) There is no clear explanation of what is "solid precipitation"...is this snowfall, hail, or else? Here more clarity is needed. Also, standard PSD against diameter makes sense for snowfall, which comes in dendritic shape?

In the revised manuscript, we avoid the use of the term "solid precipitation" and only snowfall is used. The standard form of the PSD is generally used for snow as well, while the density of the snowflake is used to account for the type and shape according to power law relationships as a function of the particle diameter (Rasmussen et al., *1999).*

6) The authors do not measure number of particles, and (equivalent?) diameter, so their complex reasoning covering distribution and shape is partly limp. Also lambda parameter is not clearly defined in its meaning, and use do the authors have measured a values of it? Is it only conjectural?

The reasoning concerning the particle distribution and shape reports the results from previous studies and we use it to introduce the way we had simulated snowfall event in the CFD approach. The meaning of the lambda parameter is well described in Marshall and Palmer (1948) and used for snowfall (Brandes et al., 2006), and we used observed values published in previous studies. It represents the slope of the PSD and relates to the snowfall intensity. Higher values of the lambda parameter correspond to events with a larger number of small size particles, and therefore light precipitation intensity and vice versa. CFD simulations confirm that light precipitation events, characterized by higher values of lambda and a large number of small size particles, are more sensitive to wind effects, and the CE decreases at low wind speed, due to the loss of many small particles. A higher number of large particles, which are less affected by wind, characterizes severe precipitation events resulting in a higher CE.

7) Some assumptions seem arbitrary, and ad hoc, e.g. cutoff of intensity (apparently 0.25 mm/30 min, i.e. 0.5 mm h^{-1}, or so), and temperature (+2 to -2^{o}C)...isn't exactly in such borderline situations when measuring solid precipitation becomes uncertain?

The choice to consider the minimum accumulation of 0.25 mm over a period of 30 min is commonly used in previous works (Kochendorfer et al. 2017a ;Wolff et al., 2015) and was used here to avoid considering false precipitation events in the data processing. These standard values are also suggested by WMO in the final report of the SPICE project (WMO 2018).

We concentrate on the range of temperature below -2° C. Temperature is often used to identify the type of precipitation. Below -2° C it is assumed that only snow occurs, above the limit of 2° C the majority of precipitation events are liquid precipitation, between -2° C and 2° C the precipitation type is defined as "mixed" and both solid and liquid precipitation may occur (Kochendorfer et al. 2017a;Wolff et al., 2015). In case events with temperature above this threshold should be considered, a measure of precipitation type would be needed to select only snowfall precipitation events, but this information is not available for the field data used.

8) It is not clear whether the authors suggest use of equations as per intensity ranges (Figure 3), or only one equation (Table 1). Also, this holds for different cumulation times (Table 2).

We used various intensity ranges for optimal identification of the dependency of the CE curves on precipitation intensity rather than temperature. This was needed to eliminate the effect of the experimental variability on the results. Once this dependency is demonstrated, we use a single curve that includes the dependency on precipitation intensity as a continuous variable rather than based on intensity ranges.

9) On top of it clearly undercatch is expected to depend upon (also) wind and intensity so the authors should in my opinion clear that their approach does not provide innovative, unexpected results, but simply fine tune some already existing approaches. For instance they might have tried to use other functional approaches beyond Eq. (3)...and introduce other variables, which might have given more generality to the work.

Actually, both the latest large international project published in December 2018 by the World Meteorological Organization (WMO) on the subject of snow measurements (SPICE project) and recent specific studies (Kochendorfer et al. 2017a ;Wolff et al., 2015) ignore the dependency of the undercatch on the precipitation intensity. They only *focus on wind and temperature as the explanatory variables upon which the transfer function is empirically tuned to match the wide dataset of field results.*

Therefore, no fine-tuning of "already existing approaches" could be done in this work, because the current state-of-the-art simply ignores this approach. May be the reviewer is right that the result could be somehow expected, but for sure it is innovative in the sense that it was not covered by the existing knowledge (quite comprehensively summarized by WMO as a result of the SPICE project).

Scientifically speaking, indeed, using the snowfall intensity to explain the residual variance after correcting for wind is quite innovative because it relates to (is a proxy of) the particle size distribution whereas the dependency on temperature accounts for the type of precipitation (rain vs snow). The conclusion states that the particle size distribution has a major role in affecting snowfall measurement biases compared to the temperature that can't be used to diagnose the type of snowflakes. Rime particle, falling faster can occur at cold temperatures that are usually associated with dry snow, falling slower. Since both of them are difficult to measure in an operational framework, proxies are generally used, and the snowfall intensity is a suitable proxy for the most relevant variable (PSD). Furthermore, the SI variable does not require any additional measurements to perform the correction.

Given the innovation provided by our approach with respect to the state-of-the-art, the paper focuses on demonstrating that snowfall intensity is a better proxy than temperature in resolving the wide scatter observed in the residuals. We agree with the reviewer that further efforts could be devoted to understand the remaining unexplained variance (about 1-2‰) – if needed, while we concentrated on showing that this cannot be solved in terms of temperature. Other functional approaches could also be addressed, e.g. to account for local climatological features, in case the regression coefficient obtained (0,75-0,9) are deemed insufficient, but this is far beyond the scope of the present paper

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(and already addressed e.g. by Kochendorfer et al., 2017a).

In conclusion, I suggest large modifications based on these reasonings, to lift the manuscript level for publication.

Please also note the supplement to this comment: https://www.hydrol-earth-syst-sci-discuss.net/hess-2018-447/hess-2018-447-RC1- supplement.pdf

Detailed comments as provided on the supplement document have been addressed where appropriate.

References

Brandes, E. A., Ikeda, K., Zhang, G., Schönhuber, M., & Rasmussen, R. M.: A statistical and physical description of hydrometeor distributions in Colorado snowstorms using a video disdrometer. Journal of applied meteorology and climatology, 46(5), 634-650, 2007.

Colli, M., R. Rasmussen, J.M. Thériault, L.G. Lanza, C.B. Baker, and Kochendorfer J.: An Improved Trajectory Model to Evaluate the Collection Performance of Snow Gauges. J. Appl. Meteor. Climatol., 54, 1826–1836, 2015.

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

Marshall, J.S. and W.M. Palmer: The distribution of raindrops with size. J. Meteor., 5, 165–166, 1948.

Rasmussen, R.M., J. Vivekanandan, J. Cole, B. Myers, and Masters C.: The Estimation of Snowfall Rate Using Visibility. J. Appl. Meteor., 38, 1542–1563, 1999.

Thériault, J., Rasmussen, R., Ikeda, K., Landolt, S.: Dependence of snow gauge collection efficiency on snowflake characteristics. Journal of Applied Meteorology and Climatology 51, 745–762, 2012.

WMO. WMO Solid Precipitation Measurement Intercomparison. Final Report. IOM Report- No. 67 (1998)

WMO. WMO Solid Precipitation Intercomparison Experiment (SPICE) (2012 - 2015).

Final Report. IOM Report- No. 131 (2018)

Wolff, M. A., Isaksen, K., Petersen-Øverleir, A., Ødemark, K., Reitan, T., and Brækkan, R.: Derivation of a new continuous adjustment function for correcting wind-induced loss of solid precipitation: results of a Norwegian field study, Hydrol. Earth Syst. Sci., 19, 951–967, https://doi.org/10.5194/hess-19-951-2015, (2015).