Manuscript: "Derivation of a new continuous adjustment function for correcting wind-induced loss of solid precipitation: results of a Norwegian field study"

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Reply to Eva Mekis (referee)

Dear Eva Mekis,

We would like to thank you for your review. We appreciate that you call our study important and evaluate the presented method as a general approach for adjusting precipitation measurements for wind loss. You pointed out a couple of limitations, which we will discuss in our answer, hoping to raise the clarity of our study.

Please find the detailed replies to your general and specific comments below

[…] All that written, it has some limitation not really discussed in the paper. The suggested new equation was tested only in Norway under very unique circumstances (the rain gauge installed higher than usual, at 4.5m). Therefore the actual parameters obtained in the calibration process probably cannot be applied under different circumstances without further testing.

We agree that the results from the study at Haukeliseter are not just transferable to any other station without further testing for several reasons. We discussed this from a more general point of view in the last two paragraphs of section 5.1 (Discussion and Outlook: Representativeness).
The special case of wind measurements at the non-standard gauge height and its possible effects on the obtained parameters are mentioned earlier in the same section (5.1, page 10065, line 4ff). We concluded that using the parameters derived for the analysis with the 10 m wind data cannot just be applied everywhere else, because a larger distance between gauge and wind sensor might change these parameters. We recommended therefore favoring wind measurements at gauge height wherever possible.
The unusual gauge height was chosen to limit the events affected by blowing snow events as stated in Section 2.2 (Measurement site: Climate) and simply prevent the gauge being buried under a possible snow height of 2-3 m at the end of the season. Consequently, all compared precipitation measurements and gauge height wind measurements were performed at the same height. We could show a direct and significant link between wind speed at the orifice height and precipitation loss and do not see any obvious reason why that relation should change depending on the installation height. The higher construction of the DFIR, however, might have different characteristics depending on its height, as the size of the clearing under the fence varies, which could change the overall wind pattern. We would have expected to see any effects of this when comparing data from different times during the winter seasons, as the height of the sensors are becoming more "normal" with the raising snow depth. No obvious influence could be observed, which is indirectly mentioned in the manuscript in section 4.1. (Results: Analysis of residuals), where we state that no misspecifications (dependencies on not considered parameters as for example the time during the season) could be seen.

The question, in which manner the installation height of the DFIR affects its efficiency would, in our opinion, best be addressed with some numerical studies, and are beyond the scope of this work. How far wind adjustments are possible with wind measurements not performed at gauge height, a limitation mentioned in the present manuscript, will be closer looked into during the WMO-SPICE effort.

General comment on small precipitation amounts:

Another limitation of the equation is that it was developed and tested only for greater than 0.1 mm events, trace precipitation was complete neglected. In Canada (which has similar climate to Norway) we found, that trace precipitation may add up to 20% to the observed precipitation on the Arctic (Mekis and Vincent, 2011). Given the frequency and importance of trace precipitation, it cannot be neglected. The general form of equation described in the paper can be applied under different circumstances. But in order to improve the confidence and applicability of the resulted parameters under different climate, further verifications would be required on different locations and longer time-series.

Specific comments on small precipitation amounts:

Sec. 3.1.1. For curve development the trace precipitation (less than the minimum measurable amount but more than zero) is excluded. But trace observation is important over the Arctic, where precipitation amounts are very low and many trace events are recorded. Under these conditions, the sum of all trace amounts becomes a significant portion of the total precipitation. The authors should consider to include the small precipitation amounts and discuss the applicability of their equation in case of trace precipitation.

Sec. 6. It is important to state explicitly the applicability and limitations of the equation. The equation was not tested on small (< 0.1 mm) events.

We agree that trace precipitation plays indeed a non-neglectable role in the total precipitation amount of a site. During the course of our study about 5% of the recorded events (compared to measurements inside the DF) at Haukeliseter are showing no significant accumulation in the standard gauges outside the DF. The accumulated sum of these events adds up to 10% of the observed precipitation at Haukeliseter.

These values depend highly on the local climate and are probably very site specific. In Norway, automatic weather stations have in contrast to manual stations no means to record trace precipitation. As these trace precipitations are adding up to a considerable sum, a thorough analysis of this lost amount and how it could be adjusted for is an interesting topic for further studies.

However, we do believe that an analysis beyond mentioning the existence and importance of trace precipitation is outside the scope of our paper. The focus of this study was to develop a mathematical adjustment method for the wind loss of measurable precipitation. The presented adjustment function does naturally require a precipitation larger zero for being applied. There is no reason to believe that very small accumulations cannot be adjusted as well as larger accumulations, as long as they can be considered significant, which might be limited by the uncertainty of the instrument. The preliminary testing of the adjustment functions are actually done on all detected precipitation larger than 0 and was not limited to events larger than 0.1 mm. The tentative results of these tests are included in the replies to E. Lanzinger and R. Nitu and will also be added to the revised manuscript.

In fact, for the derivation of the transfer function, we raised the minimum value of significant accumulation within an hour to 0.1 mm, a value significantly above the noise level of the used measurement system. That step was important, because smaller and noise affected values would have produced a number of very unrealistic catch ratios, which would have seriously hampered the successful derivation of the adjustment function.

We will add a paragraph about the importance of trace precipitations for the total precipitation amount and quantify the amount for Haukeliseter. Furthermore, we will clarify more the rationale for not including trace-events when deriving the adjustment function.

Since the project has very strong tie to the SPICE experiment and the Haukeliseter location is one of the 20 sites selected worldwide, the SPICE internet site should be added as reference: SPICE: WMO Solid Precipitation Intercomparison Experiment (2012-2014) http://www.wmo.int/pages/prog/www/IMOP/intercomparisons/SPICE/SPICE.html

We think it is a good idea to not only mention and describe the SPICE project, but also refer the reader directly further to more information about the project. We will follow your suggestion on adding a reference to the SPICE web site in the revised manuscript

Figure 3 - references and description has to be corrected. Panel d is missing completely from the discussion.

We realize that both the text and caption was not made totally clear and we will change this in the revised version. At P10053, L9-10 we will write: "Panels g and h are based on the same data as in panel c, but are filtered for temperature < 0 _C and temperature < −2 _C, respectively." And for L12-16 the correct references to panels are d and e, respectively (not e and f). In addition we include a new sentence for panel f in the text: "The same filters are applied in panel f as for panel e, however only events with mean temperature below −2 C are shown."

Figure 4 – unit is missing on the classes (oC).

Thanks for pointing out the missing unit. We will add "Temp. [°C]" at the bottom of the legend.

Table 1 and 2 - should be combined into one table.

We will follow your suggestion and combine the tables into only one table.

Sec 5. The use of bush-gauge as further reference / validation would be still desirable for more general applicability of the equation with the introduction of another filter for events smaller than 9 m/s wind speed.

We feel that even for wind speeds below 9 m/s the transfer-function between bush-gauge and DFIR is based on a very limited data set. Data from only one site are used and the gauge in the DFIR was installed at a different height than the bush-gauge. Furthermore, the transfer function was derived by a very different method.

Therefore, we were and still are very hesitant to combining the presented transfer function in this study with the bush-gauge transfer function, and by this introducing an additional uncertainty factor. We also think, that given the fact that about 20 stations are equipped with a DFIR compared to only two with a bush gauge on an international scale, a direct comparison of DFIR-measurement is more suitable to identify site-specific issues.

It is, however, possible to first apply the transfer function to adjust to measurements performed inside a DFIR and after that further adjusting the "calculated" DFIR-measurements to a bush-gauge. Applying this two-step approach will also make it easier to adapt when only one of the transfer functions changes in future analyses.

Yang et al. (2014) describes the difference between the DFIR and bushgauge based on his results as follows: "This relationship suggests that the DFIR catch is very close to true snowfall for the low winds, it however measures, on average, about 93% of "true snowfall" for wind speeds up to 6-7 m/s." We will add this reference to quantify the effect of a further adjustment to the bushgauge.

Sec. 5.1 ln 6-7. Please describe the quality control steps performed on the wind speed observations. In the same context, the quality control of the temperature is important, should be mentioned.

We agree that the performed quality control of the data should be mentioned in the manuscript. We will add a paragraph about the general quality control (human quality control) performed on all measurement parameters (including temperature) in the beginning of chapter 3 (Data and methods). In case of the wind measurements a more detailed analysis was performed which revealed disturbances by the nearby installations, followed by the exclusion of selected wind sectors as described in section 3.1.2. (Wind measurements in 10m height and gauge height).

Sec. 5.2. I assume the precipitation gauge in the center is different now from the Førland 1996 version. Can you please describe the possible effect of this difference on the results?

The reference used during the former WMO solid precipitation intercomparison is a manual Tretyakov gauge inside a DF. The aerodynamical characteristics of a Tretyakov are surely different from those of a Geonor-Altershield-configuration as used in this study. In our opinion, the effect of the double fence around these different gauges will dominate the overall aerodynamics of the reference system. Therefore, we do not expect large deviations. However, the possible effect of this difference will be quantified during SPICE with data from those sites, equipped with both references. We will include a comment on this topic in the revised manuscript.

Best regards,

Mareile Wolff and co-authors.