

**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 Rodica Nitu (referee)**

Dear Rodica Nitu,

We would like to thank you for your positive and helpful review of our paper. We are happy to hear that we managed to explain the used mathematical method in a sound and understanding way and that you evaluate the presented concept as innovative and a substantial contribution to the scientific communities dealing with precipitation and water resources.

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

The conclusions are reflective of the experiments conducted by authors, at the Haukelisetter site. The broader application of methods proposed should be validated with datasets from other sites and other climate conditions, thus confirming the transferability of proposed methodologies. The manuscript indicates that SPICE datasets could be used in this sense.

Additionally, it is recommended that the manuscript includes an evaluation (preferably quantitative) of the improvement in the reported accumulation based on the gauge measurements from this site, following the application of the proposed adjustment methods, over the entire observation period, or subsets of it.

*At the time of preparing the paper, we only had very few processed data beyond those which were already used as input for the derivation of the adjustment function. Evaluating the validity of the adjustment function as well as the connected improvement by its application with these data would not have been an independent test. Furthermore, a thorough evaluation and quantification would preferably require a detailed study of very different individual events, longer periods of various lengths and also includes data from other sites, easily filling another full-size manuscript. We do indeed hope that part of this work can be done within the WMO SPICE effort, which gives the brilliant opportunity of numerous similar equipped sites all around the world.*

*Now, one completed winter further, we did some preliminary checks on the newest data to see the effect of the application of the transfer function to independent data. We will include the results of two checked events of March 2014, representing a snow and a mixed precipitation event, respectively. Please, see the table below, which also contains the results of application of the transfer function on already analyzed data (thus not independent data) from longer periods in March 2011 and March 2012.*

*It is important to note, that these results are not from a thorough analysis and can't be used to exactly quantify the improvement connected to the application of the presented adjustment function, but they might give an indication of its effect. In all four cases a significant improvement could be achieved. Differences between the adjusted precipitation amount and the precipitation measured inside the DFIR are both positive and negative, which might indicate that the remaining differences are actually representing the noise/uncertainty of the method. For the two cases where the original difference was 32%, the adjusted precipitation differed less than +/-10% from the DFIR measurement.*

The remaining differences after adjustment of the two cases with the larger original differences (52% and 74%) are 20% and 16%, respectively.

Period	Temp (hourly averages)	Wind (hourly averages)	Observed accum. DFIR	Observed accum.	Corrected accum	Difference before	Difference after	Improvement
03/2011 30 days	-25°C- +5°C	On average 5-15 m/s, >20 m/s for some events	78.8	53.2 (X1)	80.5	25.6 (32%)	-1.7 (-2%)	30%
03/2012 20 days	-10°C- +7°C	5 – 25 m/s	29.3	14.0 (X1)	23.6	15.3 (52%)	5.7 (20%)	32%
19.-20.3.2014 37 hours	-2°C- +3°C	6-13 m/s	20.7	14.0 (X2)	19.2	6.7 (32%)	1.5 (7%)	25%
21.-22.3.2014 27 hours	<-2°C	8-15 m/s	14.6	3.8 (X2)	17.0	10.8 (74%)	-2.4 (-16%)	57%

The authors have outlined well the experiments conducted, giving consideration to the particular conditions (e.g. wind direction, proximity of gauges) and their impact on the quality of datasets. To fully enable other fellow scientists to replicate the work, the site description, Section 2, should include references to all instruments that are contributing to the creation of the precipitation events datasets; specifically, the precipitation detector, present weather sensor and/or disdrometer. The data of these instruments are referenced in Section 3.1.1, p. 10051, ln 17, and on page 10055, ln 4 to 8.

*In connection with the addition of further sensor descriptions (temperature and wind sensors) described in the reply to John Kochendorfer, we will add the description of these sensors in Section 2. The paragraph will be extended by the following sentences:*

*[...]to allow measurements undisturbed by the precipitation sensor installations, see section 3.1.2. (last sentence in additional paragraph described in reply to John Kochendorfer)  
Several optical precipitation detectors (Thies modelname) are placed at the two 10 m masts at the site. In the described even selection one of these sensors (selected because of its stability over the course of the experiment) was used for the event selection, see section 3.1.1. Furthermore, one forward scatter instrument (Vaisala PWD 21) and two disdrometer type instruments (Thies LPM and Ott Parsivel) were installed at the meteorological mast close to the DFIR to give additional information on the precipitation type, see section 3.3.3. A complete list of the instruments and an evaluation of the homogeneity of the test-site can be found in Wolff et al. (2010, 2013).*

The manuscript presents the context of the work, with appropriate credit given to related work and clearly identifying their new work, following a logical structure. The details of Section 1, Introduction, and Section 2, Measurement Site, should be restructured and streamlined (reduce in length and details), allowing for an increased focus on the methodology proposed and the results.

*We will keep that advice in mind when revising the manuscript and try to shorten wherever unnecessary detail is given. We would like to point out, that the impression of too many or too little details is rather subjective. This study is aiming at different user groups, i.e. climatologists and hydrologists and we tried to give enough information and background for all readers.*

The mathematical formulae and symbols are well described and the development of concepts is presented in a clear and logical sequence, in spite of their complexity. The approach in developing

the new concepts using Bayesian statistics is clearly outlined in the manuscript, thus lending credibility to the proposed method.

*Thank you, we are glad to hear that.*

It is recommended that Section 3.1.1, Precipitation events, is reviewed, to more clearly describe the derivation of the event datasets. Specifically, Ln 10-12, p 10051, reference is made to the derivation of 10 min events, while the analysis was conducted on 10 min and 60 min events. The authors should clarify whether the same thresholds have been applied for both event intervals.

*The selection process for precipitation events was only conducted on 10 minute periods. For each 10 minute interval, it was decided if it was a precipitation period or not. The 60 min events are simply 6 consecutive 10 minute precipitation periods, no different thresholds were used. We will clarify that in the description in section 3.1.1.*

Ln 23-24, p 10051, indicate that qualitative analysis was performed for 10 min, as well as 60 min events, with no significant differences. It would be helpful to graphically illustrate these results, to strengthen the statements made.

*We attached a figure to the reply to Eckhard Lanzinger, showing all 10 minute events. It is comparable to Figure 4 with catch ratio for different wind speeds during the 2011-12 winter. Note that this figure has slightly different temperature classes, and a forward scatter instrument (type Vaisala PWD 21) was used for the classification rain, sleet and snow (see section 3.3.3 (Precipitation type) in the manuscript for information and discussion). In addition, no filtering was used, comparable to Figure 3a.*

*Overall, the same pattern appears as in Figure 4: clear differences are seen for precipitation classified as “cold” snow (snow below -1), mixed precipitation (e.g. snow above -1 and sleet) and rain. The under-catch has a pronounced relation to temperature and a non-linear relation to wind speed. For solid precipitation the slope of the catch ratio subsides remarkably and stabilizes at ca. 20 % at 7 - 8 ms<sup>-1</sup>. Thus the differences between 10 min and 60 min events seem to be very small. Because of not delivering any additional information, we decided to not overload the manuscript with this figure.*

The plots in figures 3, 7, 8 present valuable results, and are critical to the understanding of the concepts presented. For this reason, it is recommended that they are presented in a larger size, or structured in a manner that would allow to more thoroughly understanding the results presented.

*We agree that the final presentation of the figures is not optimal. We will re-arrange the panels or otherwise try to retrieve a better readability of the figures. In the reply to Eckhard Lanzinger we suggested to divide the two divide figure 8 (a and b) into two figures and allow them to be printed on two pages.*

In section 3.4.1, p 10057, Ln 1-5, and section 3.4.2, p 10058, Ln 24-26, reference is made to the potential impact of intensity on the catch ratio. Ln 26, p 10058 indicates that “As this study focusses on winter precipitation only, intensity is assumed to be negligible.” Additional clarification should be provided to support this statement.

*We will change the explanation about the impact of intensity in section 3.4.1 in the following way: [...] Especially during summer, when mainly liquid precipitation occurs, can precipitation events be of very different nature, varying in drop size distribution and length. Precipitation with larger and heavier drops is expected to be less pronounced to wind induced loss than precipitation with smaller*

*and lighter drops. That let expect that intensity as an indirect measure of drop size may also affect the catch ratio.*

It is recommended that Section 6, Conclusions, is simplified and streamlined, avoiding repetition.

Overall, the language is fluent. It is recommended that a thorough linguistic and grammar review is undertaken, to improve the clarity of the text. Additionally, simplicity of expression and avoiding frequent qualifiers would further help in communicating the scientific message.

*Before sending the revised manuscript, we will ask a native-speaker for a complete language-review to help improving the language. We will also check for unnecessary repetitions and remove them.*

Section 1, Introduction, p 10045, In 2: “water and the availability to water” is recommended to be changed to “water and the access to water”

*That will be changed in the revised manuscript.*

Section 1, Introduction, In 2, p 10046, In 12/13, “Temperatures are significantly rising in the Arctic and already today falls a larger fraction of the annual precipitation as rain than earlier.” Recommend to change to “More recently, temperatures are significantly rising in the Arctic, and an increasing proportion of the annual precipitation falls as rain, rather than snow.” If available, a reference source for the statement would be recommended.

*The paragraph is referring to the analysis and results of Fjørland and Hansen-Bauer (2000) which is cited twice in the beginning and the end of that paragraph. We'll reword the sentence as suggested.*

Section 3.1.1 p 10051, In 1, “. . .to guarantee an objective and comparable method”, is recommended to be changed to “to guarantee a consistent method”

*That will be changed in the revised manuscript.*

Section 3.1.2 Wind measurements in 10m height and gauge height, p 10052, In 8, states that “Wind directions between 0 and 240 [degrees] were affected”.

Section 3.2, Data filtering, p 10053, In 6, states that “ Geonor X2 will be mostly affected by shadowing for wind directions between 355 and 55 [degrees]” It is recommended that the information in the two statements is correlated.-

*The first statement is related to the measured effect on wind measurements due to the installation directly at the precipitation sensor construction, very close to the single Alter wind shield.*

*The second statement is describing a larger sector, which additionally includes the geometrical determined area of the wind-shadow of the DFIR-construction. We will clarify these differences in the revised manuscript.*

Best regards,

Mareile Wolff and Co-authors.