

Interactive comment on “Derivation of a new continuous adjustment function for correcting wind-induced loss of solid precipitation: results of a Norwegian field study” by M. A. Wolff et al.

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This publication contains very valuable work and is of highest interest for the hydrological community. The Bayesian approach for finding the optimal correction function provides a sound and retraceable justification for the selected functions and their parameters.

I fully agree with chapter 1 which underlines the importance of this experiment. The site description in chapter 2 is also sufficient. Both chapters need some minor lingual corrections (e.g. word order).

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In chapter 3 on p. 10051 it is stated that “no significant differences occurred” in the qualitative analysis of 10 min and 60 min events. Is there any proof of this statement that could be shown? Theoretically there could be a difference, especially when temperature and wind conditions are changing or highly variable (see also p. 10066, line 13 ff.). A 60 min event represents an average over this time period. The relation between catching ratio and wind speed or temperature should be influenced if this relation is non-linear. If this effect is negligible in reality it should be explained why (small non-linearity? Small variations in temperature or wind speed at this site?).

In section 3.2 the description of fig. 3, panel d) and e) has to be checked. There is a discrepancy between the text and the caption. From my understanding of the caption, panel f and h show all points for temperatures below -2°C , but in panel h the temperature and wind speed filter was not applied. If this is correct, it could be mentioned to support the argument why none of the filters were finally used.

Lingual correction in Section 3.3.2 at line 16: data was *divided* into ... classes. The statistical approach and the description are well described.

The plots in figure 8 are very small and might be hard to look at when printed on paper. I suggest plotting only one larger graph with the resulting functions for some different temperature ranges in different colors, similar to fig. 4. If this graph tends to be overloaded, I would suggest reducing the depicted temperature classes. Despite the discussion of the residuals later on I was wondering if some explanations could be given, why the scatter of data is so different between certain temperature ranges. I also wonder, why the data points for the same temperature classes are different in fig. 8 a) and b). Shouldn't they be the same?

As the article is about the correction of measured precipitation data I was looking for a graph and/ or table where the resulting improvement after correction could be seen. Eventually this could be a contour plot like fig. 10 showing the deviations from the reference with applied correction. Maybe a “before” and “after” plot in this contour style

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could give a good impression of where the problem is and how well it is solved.

On p. 10067, line 25 it is said that the uncertainty of the correction cannot be properly derived. An unknown distribution of residuals and a missing specification of regression noise are mentioned as the reason. Could eventually GUM provide a solution, because in this framework uncertainties can be derived without knowing the actual distribution of noise (by using a rectangular distribution)?

Chapter 5 and 6 need more detailed lingual corrections by a native speaker (which I am not).

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