Hydrology and Earth System Sciences

Discussions

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comment

Interactive comment on "Testing and Development of Transfer Functions for Weighing Precipitation Gauges in WMO-SPICE" *by* John Kochendorfer et al.

Anonymous Referee #1

Received and published: 6 July 2017

The manuscript "Testing and Development of transfer functions for weighing precipitation gauges in WMO-SPICE" deals with the interesting and relevant topic of how to correct undercatch especially of snowfall for different sensors. This paper is basically an extension of the Kochendorfer et al. 2017 (HESS) that again is based on the (Kochendorfer et al. 2016) study. Here the apply the previously developed correction functions to different sensors and locations and found that the previously developed, more general correction function of Kochendorfer et al. 2017 (HESS) is always recommended to use. In general the paper is mostly very written and I found no crucial error or mistake. My major concern is more related to presentation and the structure of the manuscript. Both section "Methods" and "Results (& Discussion)" are written very detailed, techni-

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Discussion paper

cally and lengthy, giving each sensor, its location, and treatment of the data its place. Figures 4 - 11 display in detail the (mostly non-existing) differences between correction functions. Partly, I found it hard to follow the overall structure/story of the manuscript. In my opinion, this structure would have been justified if the authors would have found several different best-performing equations that need to be presented alongside each sensor and in comparison with the general correction function. But, given that in most cases the general (somehow site and sensor unspecific) correction function is as well performing and recommended by the authors, a more summarizing structure would have been much more feasible and would have cause less redundancies. In its current version, the manuscript unfortunately reads quite lengthy, especially given the final outcome. Many of the interpretations named in the abstract and conclusion are a bit out of blue and should be much more elaborated. Hence, instead of the detailed description, I would recommend to show some summarizing, and comparing analyses/graphics on sensor performance.

Based on this basic concern, and the following further smaller concerns, I recommend to provide either a revised, restructured version that strongly condense the specific sensor part and put more focus on the comparison analysis, or to show the transferability of the Kochendorfer et al. 2017a Equation 3 to other sensors in a HESS-technical note. I am sorry to be that harsh, but I really do think that the level of detail and length of the manuscript does not match your findings.

Further general concerns:

1. Several time differences are named significant or non-significant, but I somehow missed the section describing how a significance test was performed.

2. Given that both the biases of the corrected values and the differences between the corrected functions are mostly rather small (Figure 4- 11, RMSE < 0.5 mm, biases « 0.5 mm, differences far less), I wonder how and if at one can and should interpret these differences, given the measurement accuracy of each sensor.

3. The paper needs to be more independent from Kochendorfer et al. 2017 (HESS). At least the essential equation 3 should also be given in this paper.

Further specific comments:

page 2, line 19-22: I do not understand this sentence. Please check language and try to avoid too long sentences.

page 3ff: I would recomment to use Table 1 much more often to guide the reader through the different configurations.

page 3, A paragraph that outlines the design of this study would help the reader to follow your following descriptions

page 3, line 9: typo: "a either a"

page 3, line 31: CARE, acronym not yet introduced

page 4, line 4, "have" missing in and a lower porosity

page 4, line 31: 6 s and 1 min data correspond to with sensor / site. Maybe update Table 1

page 4, line 32: "realistic" and "operational" limits, please define

page 5, Maybe I missed it, but where do you define that DFAR is the reference, and please discuss briefly its quality and its deviation from the "truth"

page 5, line 13ff: How were temperature thresholds defined. If possible refer to citations

page 5, line 18: Please provide these equation also here to be more independent from the Kochendorfer et al. (2007a) paper

page 7, line 13ff: I do not understand this interpretation. If significant site biases exists, doesn't that mean that you have to develop site-specific empirical correction functions?

page 10, line 31: "1500mm" Geonor ? cp. with page 11, line 1: please be consistent, see also page 11, line 17

page 11, line 6: typo: "attributed"

page 11, line 20: does the noise of the Geonor 1500 mm stem from the fact that they have a different sensitivity than the 500 mm ?

page 11, line 20 ff: Are these interpretation valid, given the some deviation and the measurement accuracy?

page 11, line 26, CARE and Marshall *test sites* !

page 17, Table 1: please add time period of measurements considered

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2017-228, 2017.