

Interactive
Comment

Interactive comment on “Evaporation from weighing precipitation gauges: impacts on automated gauge measurements and quality assurance methods” by R. D. Leeper and J. Kochendorfer

Anonymous Referee #3

Received and published: 2 February 2015

General Comments:

“Evaporation from weighing precipitation gauges: impacts on automated gauge measurements and quality assurance methods” is an important paper on the field related to the issues of precipitation measurements using auto gauges. The paper is focusing on (1) the intercomparison of two identically configured weighing gauges, with or without evaporative suppressant and (2) the comparison of pairwise or weighting average algorithms. Based on a relatively short field experiment it concludes that the choice of

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Interactive Discussion

Discussion Paper



method is even more important than the use of suppressants. The impact of the evaporation on precipitation measurements was sensitive on the choice of method. The results of this paper can be analysed separating two items: the efficiency of suppressant applied in the bucket content for prevention evaporation loss of total precipitation and the applicability of two different algorithms to analyze raw data.

This paper highlighted the importance of understanding the QA methods applied in the process. Based on the presented field experiment, the conclusion is interesting but not fully convincing. The study was conducted on one site for a 3 month period, including only rain events. The result would be more general, if the field experiment can be applied for longer period and under different climate conditions.

The paper is reasonably well written, but further details, clarifications are required for full transparency and understanding. It is especially true for the methodology section. Based on the provided information, it is very difficult to determine the required level of details without seeing the “foundation” publication (Leeper et al., 2014b). My suggestion is to delay this publication until the Leeper et al, 2014b publication is available.

Specific Comments:

P 12852: line 17-20: Abstract: Please add here the names of the two important methods used later extensively in the analysis: pairwise and weighted average method.

P 12852: line 3: “lower” not clear what you mean.

P 12853: line 28: In the WMO, 2008 document the precipitation bias is given in percent, not in absolute difference. In Table 1, only the difference is provided. The precipitation events are often treated differently based on the intensity. Perhaps further conclusion could be provided using percent error due to evaporation per event.

P 12854: line 13: replace “Addition” with “Additional”

P 12855: line 11: Similarity of the Tretyakov and Geonor gauges is a subjective statement, it is more important to describe the characteristics of the gauges.

P 12855: line 21: For the transparency of the experiment the description of the suppressant (type, quantity and concentration) should also be added. Related to the gauge description, I could not find whether it is heated or non-heated gauge; the type of shield applied on the actual two Geonor T-200B gauges.

Methodology: The applied terminology of “evap” gauges is confusing to me as well (similarly to Referee #2). The term used as noun (evaporation) or as the gauge description should be separated. I like better the terminology suggested by Ref#1: Geonor-Evap and Geonor-NonEvap for the gauge type description.

Additionally, there are four different time-series discussed here: 1) Geonor_Evap_pairwise; 2) Geonor_Evap_wAvg; 3) Geonor_NonEvap_pairwise; 4) Geonor_NonEvap_wAvg. To avoid any misunderstanding, I suggest spelling out at each of the occurrences.

P 12856: line 14: Further details are required in the description of USCRN QA system regarding “aggregated” one-minute gauge depth to 5 minutes.

P 12856: line 16-27: Two types of quality assurance (QA) algorithms were applied on the raw data from the experiment, namely the pairwise and the weighted average wAvg method. The definitions of the equations are missing from the paper, only referring to another submitted (accepted?) paper. Since the difference from these two methods is an important finding of the present discussion paper, detailed and precise description of the equations should be added here.

P 12857 line 23-24: The average hourly loss and the associated range should be defined better, the meaning is not clear in the context. Also, the same precision should be used on the base and interval.

P 12858 line 7: “regardless of wind speed” – based on figure 4d, this statement is not true.

P 12858 line 9: false precipitation due to “condensation buildup” – need to be studied

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and explained further.

P 12858 line 15: Figure 5 – the minimum reached at 19 not 18

P 12858 line 17: albeit = even though

P 12858 line 13-22: In this resolution all the consideration regarding the results is not necessary true before the actual processing algorithm and signal noise is not well defined. The analysis of the magnitude of possible errors would add to the credibility of the conclusions.

P 12859 lines 3-4: It is actually Evap – NonEvap in Table 1, please correct. Also, the sign of difference is not consistent in case of pairwise algorithm either.

P 12859 lines 10-18: some details of the missing QA pairwise and wAvg descriptions are included here (the use of reference depth), even more highlighting the need for the accurate description of the algorithms earlier in the methodology section!

P 12861 lines 11-14: The conclusion that “suppressants and evaporative adjustments . . . may not be required” is too strong, cannot be concluded from this field extremely limited experiment (one location, 3 summer months, 29 rain events). This sentence should be modified by adding the circumstances under the experiment were performed.

Table 1: It is a very important part of the paper. Additionally to the “Event Id”, the Event duration would also be useful for full understanding. Also, would it be possible to add another digit – increase the precision from 0.1mm to 0.01mm? It is particularly important in the difference series, where several identical values are found. Some of the results later in the paper are anyhow defined in higher precision. This would provide a consistency through the paper.

Table 1 and Figure 6 are seemed to be the mirror of each other (-3 in Table 1 and +3 in Figure 6. . . . Can it be change to be consistent?

Figure 3-5: Title should contain the reference to the type of events it includes (definition

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Interactive Discussion

Discussion Paper



of dry condition – or all?)

Figure 3-5: The terminology “wire-noise” first appears in the conclusion and the “wire” in figure titles. Please add the definition, its meaning to the description part.

Figure 6: Since this figure contains only results from evap gauges, different choice of colors would be better compared to Figure 5 where both type of gauges were present.

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 12851, 2014.

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