Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2018-447-RC1, 2018

Hydrology and Earth System Sciences
Open Access
Discussions

Interactive comment on "Using Snowfall Intensity to Improve the Correction of Wind-Induced Undercatch in Solid Precipitation Measurements" *by* Matteo Colli et al.

Bocchiola (Referee)

daniele.bocchiola@polimi.it

Received and published: 14 December 2018

The manuscript is generally speaking of interest, given the importance of snowfall, and solid precipitation measurements for a number of purposes.

However, the manuscript as is requires large revision in my opinion, as follows

1) The text is not always clear, and at times awkward or even unintelligible. I report large suggestions for re-editing in the pdf fils attached.

2) The basic assumptions seem arbitrary. Why undercatch must only depend upon either on temperature or intensity. Can it depend on both ? More generally, when

a correlation/regression analysis is to be taken, a larger array of variables need be explored, and those influencing need be retained, with indication of the explanation power. The authors start with the idea that wind and intensity are the only affecting variables, but this should be better documented, and if other variables (e.g. temperature) provide more explanative power they should be included. Technically use of RSME does not indicate goodness of fit, which is best givwn by r2, the authors mix the two, so it is difficult to make comparisons.

3) Also, undercatch as defined seemingly depend by construction upon intensity...was this considered ? Am I wrong ?

4) The CFD part is utmost unclear...the authors refer heavily to other studies, but so doing thuis part does no clarify or add anything visible. This CFD part needs heavily to be tightened.

5) There is no clear explanation of what is "solid precipitation"...is this snowfall, hail, or else ? Here more clarity is needed. Also, standard PSD against diameter makes sense for snowfall, which comes in dendritic shape ?

6) The authors do not measure number of particles, and (equivalent?) diameter, so their complex reasoning covering distribution and shape is partly limp. Also lambda parameter is not clearly defined in its meaning, and use...do the authors have measured avalues of it ? Is it only conjectural ?

7) Some assumptions seem arbitrary, and ad hoc, e.g. cutoff of intensity (apparently 0.25 mm/30 min, i.e. 0.5 mmh-1, or so), and temperature (+2 to -2 °C)...isn't exactly in such borderline situations when meausing solid precipitation becomes uncertain ?

8) It is not clear whether the authors suggest use of equations as per intensity ranges (Figure 3), or only one equation (Table 1). Also, this holds for different cumulation times (Table 2).

9) On top of it....clearly undercatch is expected to depend upon (also) wind and in-

tensity..so the authors should in my opinion clear that their approach does not provide innovative, unexpected results, but simply fine tune some already existing approaches. For instance they might have tried to use othe functional approaches beyond Eq. (3)...and introduce other variables, which might have given more generality to the work.

In conclusion, I suggest large modifications based on these reasonings, to lift the manuscript level for publication.

Please also note the supplement to this comment:
https://www.hydrol-earth-syst-sci-discuss.net/hess-2018-447/hess-2018-447-RC1-supplement.pdf

---

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2018-447, 2018.