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Hydrology and Earth System Sciences
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Discussions

Interactive comment on "ESOLIP – estimate of solid and liquid precipitation at sub-daily time resolution by combining snow height and rain gauge measurements" *by* E. Mair et al.

Anonymous Referee #3

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This paper tackles the very important and interesting issue of sub-daily precipitation measurements in mountainous terrain. The approach that has been chosen relies on a combination of various measurements in order to reconstruct precipitation time series. This could prove quite useful, unfortunately this paper fails in several aspects.

First, the choice of cumulated snow fall as a validation parameter for some of the validation periods is not appropriate. Because such measurements highly depend on the measurement protocol (bringing in the effects of snow settling), they do not provide any kind of a reference. The authors acknowledge that a large uncertainty is expected and guess a large correction factor, making all validation pointless. Instead, since Snow

Water Equivalent measurements are available, all validation should have been performed on SWE and estimating the impact of filtering should have been performed on SWE instead of the cumulated snow fall.

Then, the measured snow height is at the very core of the method. Unfortunately, these measurements need to be filtered in order to be usable and some very important aspects of the filtering are not exposed. Did the authors had to remove some obvious outliers, and how? The filtering relies on a Window Moving Average, but such a filtering introduces a phase (ie the signal is shifted in time). How was this phase removed? What was the centering of the time window? A centered window would not introduce any phase but would not handle data points too close to data gaps. The authors state that a five hours window gives the best results, but don't discuss the smoothing that happens on the signal, further reducing the time correlation between the filtered snow height signal and other measurements. Also, snow settling seems not to be taken into account, since in this model, only increases of snow height produce precipitation.

Finally, the paper lacks clarity and should be further proof-read and reorganized: the data pre-processing is spread over numerous sections, making it hard to follow what really happens. Quite a few sentences should be rephrased and the overall length should be reduced while some keys elements should be more detailed: as previously mentioned, the Window Moving Average should be better explained, the snow settling should be covered as well as rain on snow events. A few elements are also surprising: when the authors mention on page 8697, line 17 that using a variable density in their model performs better than using a snow pillow for estimating SWE or when in table 1 using a more stringent criteria leads to less points being filtered out. Some citations also appear as not really necessary and should be removed.

Overall, I recommend this paper to be rejected and encourage the authors to address the identified issues, making sure that their claims are backed by robust measurements, before eventually resubmitting.

Detailed comments:

* Abstract, line 1: measuring precipitation is demanding in itself, not only for hydrological applications!

* abstract, lines 10-11: please rephrase

* abstract, lines 17-18: please rephrase

* page 8686, lines 5-6: it might be better to be more specific: "especially in small, steep catchments"

* page 8688, line 20: "at a distance of 3km"

* page 8690, lines 20-21: where is the additional data coming from (ie how far from the original station)

* page 8690, lines 23-24: please rephrase

* page 8690, lines 25-26: it is not clear what these 1% refer to. Is it compared to a reference precipitation measurement or the original data set?

* page 8691, lines 21-23: what happens for points passing one criteria and not passing the second one?

* page 8692, line 20: such events will be underestimated!

* page 8693, line 13: I am uneasy about the usage of "metadata" since the authors actually talk about extra meteorological data

* page 8693, line 19: extra comma to be removed

* page 8695, line 3: the table 3 should be fully redone. Using an unreliable measurement for validation is not right!

* page 8695, line 16: "underestimates snowfall by 18-21%"

* page 8695, line 21: "calculated with the four methods"

* page 8696, lines 17-24: please rephrase

* page 8697, line 1: please rephrase

* page 8697, line 1: the fresh snow density parametrizations that have been rejected show very reasonable values. It is not clear why they got rejected (see also line 7), or based on which criteria

* page 8697, lines 22-23: please rephrase

* page 8698, line 20: "of both rain gauge and model is likely"

* page 8699, lines 8-12: please rephrase

* page 8700, lines 2-3: please rephrase

* page 8707, table 4: is there any difference between SWE and total precipitation at the chosen test sites?

* page 8711, figure 4: consider using initials as labels instead of numbers. Moreover, identifying on the plot which parametrizations will be used afterward would be a plus. Finally, it is not clear why the first two parametrizations have been selected, since they show a significant number of outliers events (if I understand correctly the meaning of the red crosses)

* page 8714, figure 7: The explanations for the right panel are unclear. Is adding rainfall from the unheated rain gauge to snow pillow estimates not double-counting precipitation? And for the heated rain gauge, does it simply means that delayed melt is accounted for?

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 8683, 2013.