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Discussions

Interactive comment on "The use of personal weather station observation for improving precipitation estimation and interpolation" *by* András Bárdossy et al.

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We thank Hannes Müller-Thomy for his thoughtful remarks. Our answers to the specific comments (in blue) are as follows:

L26-28 The short periods of available radar data should be mentioned in this context as well.

We will add this aspect in the revised manuscript.

L77-79 It would be helpful if the authors are more concise regarding the number os

PWS stations finally used in the study. To enable a transfer of the applied methods the authors should provide some information, which minimum time series length was chosen for the secondary time series and how was it chosen?

The number of PWS stations varies strongly due to the increase of the network in time and due to unexpected missing records. The first filter is used to identify the locations which can be used. The number of PWS for each time step is normally slightly less than the number of stations remaining after the first filter. This depends on which stations had valid observations for this time step and if they were eliminated by the on-event filter or not. We'll include the actual number of PWS used for the maps presented in the paper. The minimum length of observations for the application of the first filter was two months. This is a reasonable choice for hourly aggregations. For longer aggregations longer series would be required. This of course leads to high uncertainties of the indicator correlations.

L85 From the first paragraph in Section 3 it sounds as only the two data quality filters will be explained. I suggest to provide a brief overview of all subsections at the beginning of Section 3 and an explanation, how they are interacting.

Referee 2 suggested a flow chart to illustrate the procedure, we will make this more clear at the beginning of the Methodology chapter.

L102 Maybe the authors should explain briefly why they consider Y as a random field.

This is a mistake and will be corrected, Y is not a stationary random field. It is the sum of precipitation (considered as random field) and a measurement error which is spatially independent, temporally dependent and has a non-zero mean.

L120-123 The chosen criterion sounds reasonable. I'm wondering if an exclusion for too high correlations has to be applied as well. Later in Fig. 5 indicator correlations of 1 are shown for interstation distances of 10 km, which is way higher than from the primary network. Maybe the authors can report if an upper limit is required or not when working with the data as a result from their data analysis.

Due to the partly very short time series the indicator correlation between the primary and secondary networks can fluctuate a lot. We did not calculate the sample size dependent confidence intervals of the correlations as this should be done for each pair individually. Instead we decided to remove the low ones - where we certainly removed a few which provide reasonable data. The correlation being 1 is mainly the consequence of small samples, and thus we did not exclude those stations.

Also, I'm struggling with the final decision if a secondary time series remains in the potential useful data set or not. As far as I understand it a time series is "flagged as suspicious" if it does not meet the criterion in Eq. 2. That means the time series will be sorted out. Since the procedure is repeated for several α and Δt, I imagine the highest exclusion rate will be found for high values of α. Is a flagging for only one of the analysed values of α enough for an exclusion of that time series? Which values of α have been applied and what was the exclusion rate?

Due to sample size we decided to apply the filter to the hourly data. The reason for taking the 99 % threshold was that we are mainly interested in heavy rainfall. Other durations and thresholds were also calculated but the decision was taken on the basis of the above aggregation and threshold. For these, the exclusion rate was about 60%.

L159-160 Does this approach introduce an upper limit for the point of interest, resulting from the maximum rainfall amount measured at the surrounding primary stations? Or are theoretical distribution functions applied and the information is missing (or I missed

it)?

There is no upper limit on the observations implied by the second filter. If the second filter is applied on the percentiles the upper limit is 1.

Fig. 3 & Table 3 From Fig. 3 it is obvious that the minimum resolution is 0.01mm for the Pluvio, while it is 0.1mm for the PWS. This makes a comparison of p0 without its consideration biased. Was the different measurement resolution taken into account for the values of p0 in Table 1? Otherwise I would recommend to either neglect values <0.1mm or to sum rainfall amounts up to a minimum of 0.1mm. The Pluvio will gain more dry time steps by doing so. It maybe has a negligible effect for hourly time steps, but for the original temporal resolution of 5min it will be critical. Hence, it should be at least communicated to the reader.

Thank you for pointing out the issue with the zeros and the resolution. This effect is indeed critical for high temporal resolution, i.e. 5 Minutes. In Fig. 3, we will consider this aspect by summing up amounts from the Pluvio to 0.1mm. The numbers in table 1 are based on 1h resolution, so this effect should be negligible, but we will check this and correct it if necessary.

Fig.3 I recommend to add x-y-lines to illustrate the perfect match since in the left figure it is not the diagonal.

We will add this.

Fig. 5 Indicator correlations with values below the minimum resulting from the primary network for similar distances are included in the right figure. From my understanding these were removed by (2)? Also, for the decision of keeping secondary stations or not indicator correlations for unknown distances resulting from the primary network have to be estimated. In general, this is done by fitting regression lines to the observations? Was it done similar in this study? If so, it could be useful for the reader to provide the type of regression line and it parameters. If not, how were values judged for unknown distances?

The indicator correlation are filtered by comparing the correlation between the pairs (1) PWS station - Primary neighbouring station and (2) Primary neighbouring station - Primary neighbouring station. This is done for the available PWS time period and varies individually. This is why, the equation was tested for each PWS and fitting a regression line cannot describe the individual behaviour between each PWS and it's neighbours.

L289 "...With increasing...as the role of the bias increases. Is the bias the only reason therefore? I guess the much higher spatial correlation for longer time steps also gives less possibility for improvements, so a frontal event with 12h duration covers some of the stations from the primary network, while this is not the case for hourly time steps (it is mentioned later, L298).

The aggregation leads to more smooth and higher correlated variables which is as the reviewer pointed out another reason for the smaller improvements for longer aggregations. This will be mentioned in the revised paper.

L335-336 Do the authors mean "event" here instead of "data"? Otherwise I'm wondering to not find the values for the RMSE in Table 5.

Table 5 contains the RMSE calculated over all events and stations, while in the text discussing the figures we used the RMSE calculated for the single event using all available primary stations. That is why the numbers are different. The word data will be replaced by event.

L75 Can the authors provide a reference for the 5°C threshold or how was it chosen?

This threshold was chosen arbitrarily, we wanted to be sure not to include any snow fall events, so this is threshold is rather strict.

The other technical corrections will be considered while preparing the revised manuscript.

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