

Interactive comment on “Improving the precipitation accumulation analysis using radar-, gauge- and lightning measurements” by E. Gregow et al.

S. Ochoa Rodriguez (Referee)

s.ochoa-rodriguez@imperial.ac.uk

Received and published: 18 April 2016

This paper explores the use of cloud-to-ground lightening data for improving radar-based (and raingauge-adjusted radar-based) precipitation estimation, with a focus on high intensity, convective storms. The study includes an evaluation of resulting rainfall estimates at different temporal accumulations.

The topic of the paper is interesting and the results are potentially useful. However, the paper has a number of major flaws which should be addressed before it can be published.

General comments / major flaws:

C1

- The structure of the article is rather unorganised and the description of data, methods and results are often unclear. The description of the way in which lightening and radar data are merged is very unclear (and bits and pieces of the methodology are spread throughout several sections of the paper), descriptions of the methods are included in the data and results sections, amongst other things (more details are provided below).

- The fact that only 7 independent rain gauges are used for evaluation renders the results statistically weak. This issue is so critical that in one of the cases (Table 2), there are simply no data available for evaluation from any of the independent gauges. Why out of 472 available rain gauges would you only select 7 for independent evaluation? I am aware that you also present the results of performance statistics at non-independent rain gauge locations; however, I truly believe that more interesting results could be achieved if either more independent rain gauges were used or if a cross-validation approach were implemented.

- The results, as currently presented, are too vague and far from the objectives initially set in the abstract and introduction. As stated in the title of the paper, you aim at “improving precipitation accumulation analysis using radar, gauge and lightening measurements”. Throughout the paper, the focus is mostly on the added value of lightening data, which does not really lead to significant improvements in radar-based QPEs, let alone gauge-based adjusted radar QPEs. One option would be to change the focus (and consequently the title) of the paper to “the added value of lightening measurements” for generation of QPEs. The added value could be more clearly assessed under different scenarios, including with and without radar data available, with / without rain gauge data. From the results you present, the real advantage of lightening data appears to be in cases when radar data are not available (which can occur either because there simply are no weather radars, due to malfunctioning of the radar or to issues such as beam blockage and/or attenuation); this is very valuable and should be made clearer (and, from my point of view, justifies changing the focus of the paper/changing the way in which objectives and results are described).

C2

Detailed comments:

- Introduction: work by the co-authors of the paper is often cited and a proper review of relevant literature on the actual topic of the paper is lacking. The introduction should be extended to include:

- o A review on the use of lightening data for QPE generation

- o A more in depth and critical discussion of adjustment of radar QPEs. The only comment so far is that “the use of monthly adjustment factors leads to less than optimal results”. Several studies have been carried out which have shown a clear advantage of adjusting radar QPEs based on rain gauge data and other sources of information, with corrections implemented at significantly shorter time scales (as compared to the monthly one that you mention). Also, since the focus of your study is mostly on convective storms, it may be worth discussing the performance of merging techniques for convective storms, which is still a problematic issue (see Jewell & Gaussiat, 2015; Wang et al., 2015) and which could be a case in which lightening information could be useful.

- o A review and discussion on the topic of the impact of temporal aggregation on precipitation products. The impact of the temporal scale at which adjustments are performed is a key part of this study. This issue has been discussed in a number of papers which should be reviewed and in the light of which the results of the present study should be analysed (e.g. you should discuss how the optimal temporal resolution that you found (1h) compares to that found in previous studies for the case of convective storms). See for example Berndt et al. 2014.

- o I would suggest to remove irrelevant information, such as L26-27 (projected annual precipitation in Northern EU) and keep the introduction focused on the topic of the paper.

- Data Section:

C3

- o L47-50 include description of methods and should be removed from the data section. I would suggest that you simply say that three data sources are employed in this study and then go on to explain them (without starting to describe the LAPS, which should be done in the methods section).

- o Section 2.1 – I would change title to “Ground rain gauges” or would at least include the word “rain gauge” in it.

- o Section 2.1, L57-58 is a repetition of L52; try merging these sentences.

- o Section 2.3: A bit more details about lightening sensors would be desirable. It would help the reader understand how is it that lightening measurements can be translated into vertical ‘radar’ rainfall profiles and so on. I am aware that this information can be found in other papers, but I think it would be helpful and interesting for the reader to find a brief description of the sensors here. Just in the same way that you provide a brief overview of radar QPE generation.

- o While all but one of the Finnish radars are dual-pol, it appears that dual-pol parameters are not being used for QPE generation. The authors mention that a single Z-R relationship is used, which implies simplification and assumptions about variable drop-size distribution and the like. Such simplification could clearly be avoided were dual-pol parameters used. It should be made clear (in Section 2.2) that dual-pol parameters are not being used at all and the implications of this should be discussed (I reckon that the use of dual-pol parameters would lead to much larger improvements in the quality of radar QPEs than the use lightening information).

- Methods Section: this section is rather unorganised and a thorough re-structuring would be desirable. Some specific comments/suggestions are the following:

- o Section 3.1, L97: “. . . where a dense observational input, from several sources, are fitted to a coarser background model first-guess field”. Please indicate which data sources are used.

C4

o Section 3.1, L102: you indicate the spatial resolution of the FMI-LAPS output, but not the temporal resolution. From subsequent sections I gather that the temporal resolution is 5 min, but it should be clearly indicated in Section 3.1.

o Sections 3.2 and 3.3: these two sections present overlapping information and a clear and integrated description of the integration of lightening data with radar data is missing. I suggest merging these two sections and producing a new and clearer description of the merging method.

- Results section:

o Why using coefficient of determination AND Pearson's correlation coefficient? Their only difference is that the correlation coefficient has a sign, so it may be useful to include both in cases where you expect the correlation coefficient to take negative values. Since this is not the case here, the two performance measures provide very similar information. I would suggest to use only one of the two.

o A description of the log STDEV is missing and a justification for using a log-STDEV instead of the non-log STDEV should be included.

o As mentioned above, this section is unorganised and includes a great deal of description of methods, which should be transferred to Section 3. Also, results should be presented in a more concise and assertive manner (comments such as "neutral to positive impact" should be removed). As suggested above, a shift in the focus of the paper could make it easier to describe the results in a more assertive / critical manner.

o A scale bar should be included in Figure 1.

o Why using log scales in figures 4, 5, 7 and 8? I think a normal (linear) scale would be better. A linear scale is normally used in papers on this topic, so readers are used to it. I do not see any added value in using a log scale and I do think that it hinders interpretation of results.

o I suggest using mm to indicate rainfall accumulations (accompanied by the temporal

C5

aggregation scale), instead of using mm/h (which is the unit normally used for intensities).

- Other comments

o Why not work with sub-hourly temporal accumulations? The lifetime of convective cells is often < 1 h. Since the focus of the study is on convective storms and lightening and radar data are available at high temporal resolution (5 min), it would make sense to evaluate sub-hourly scales.

o Misuse of semicolons throughout the paper. The semicolon should be either removed or replaced by a colon. E.g.:

L16: "such as;" (remove semicolon)

L133: "resulting from;" (either remove or change to colon)

L218: "analysis time;" (change to colon) Many others! Please check.

REFERENCES:

Berndt, C., Rabiei, E. & Haberlandt, U. (2014). Geostatistical merging of rain gauge and radar data for high temporal resolutions and various station density scenarios. *Journal of Hydrology*, 508, 88-101.

Jewell, S. A. & Gaussiat, N. (2015). An assessment of kriging-based rain-gauge-radar merging techniques. *Quarterly Journal of the Royal Meteorological Society*.

Wang, L.-P., Ochoa-Rodríguez, S., Onof, C. & Willems, P. (2015). Singularity-sensitive gauge-based radar rainfall adjustment methods for urban hydrological applications. *Hydrology and Earth System Sciences*, 19 (9), 4001-4021.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-113, 2016.