Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2019-624-RC2, 2020

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Use of dual-polarization weather radar quantitative precipitation estimation for climatology" *by* Tanel Voormansik et al.

Hidde Leijnse (Referee)

hidde.leijnse@knmi.nl

Received and published: 12 February 2020

In this paper long-term datasets from two different regions (Estonia and Northern Italy) are used to evaluate the performance of polarimetric weather radar quantitative precipitation estimates. Several years of radar and gauge data are used for this. This is a very interesting topic that is highly relevant and timely as long-term high-quality operational polarimetric datasets are becoming more and more available. The paper has a clear focus, which makes it pleasant to read. Some of the English used in the paper could be improved, but it certainly does not prohibit full understanding of the paper. I do have some questions that I would like to see clarified and some suggestions for improvements. In particular, I think there may be an error in at least one of the figures that I think the authors should look at. I think the paper should be published after major

Printer-friendly version

Discussion paper

revisions. Specific comments are given below.

Specific comments

1. I very much appreciate the honesty of the authors about removing low-quality data from the dataset that resulted from radar issues. Because of this, and because of the focus on only the warm season, I doubt whether mentioning "climatology" in the title would be suitable. Please reconsider this, or at least add to the title that this is about warm-season precipitation (which is still very valuable).
2. On line 72, it is stated that the Italian rain gauges have a resolution of 0.2 mm and 1 minute. This means that, if the gauges report rainfall intensities, the minimum rainfall intensity that these gauges would be able to record is 12 mm h^{-1}. Or do the gauges record total rainfall accumulation with a 1-minute time resolution (in which case there is no issue with the total accumulations)?
3. On lines 105-107, the computation of Φ_{DP} and K_{DP} from raw Φ_{DP} is mentioned, along with the fact that "carefully tuned parameter values according to data specifics" are used for this. It would be interesting and highly relevant to include a more thorough description of this in the paper, especially since K_{DP} is a key variable in this paper. I think a one- or two-sentence summary of the method would be nice, along with a short description of the parameters and how they were determined.
4. On lines 107-110, the self-consistency method for re-calibrating Z_{H} is discussed, where Z_{DR} is also used. Later, on lines 124-125, it is mentioned that the use of Z_{DR} for quantitative precipitation estimation is not recommended for C-band radars. I think it should be discussed here why Z_{DR} can be used for re-calibration of Z_{H}.

5. On line 127, the threshold for switching between $R(Z_\mathrm{H})$ and $R(K_\mathrm{DP})$ is defined to be 25 dBZ for Z_H. Using Eqs (1) and (2), this threshold translates to $R \approx 1$ mm h^{-1} and $K_\mathrm{DP} \approx 0.015^\circ$ km^{-1}. These values are much lower than what is cited from the literature ($R = 50$ mm h^{-1} and $K_\mathrm{DP} = 0.5 - 1^\circ$ km^{-1}). What is the reason for using this much lower threshold? I think this should be explained in the paper.

6. In Section 2.2, there is no mention of attenuation that could affect the $R(Z_\mathrm{H})$ estimates. This attenuation could be corrected for using Φ_DP. Is there a specific reason why attenuation correction is not carried out?

7. In Section 2.2, it would be good to mention that the effect of VPR will be limited in the analyses because only data from the warm season will be used, and that only data close to the radars (70 km and 30 km for Estonia and Italy, respectively) will be used.

8. In Section 2.2, it is not explicitly mentioned how precipitation accumulations are computed. I assume (also based on the rest of the paper) that they are computed by simply adding subsequent instantaneous radar QPE values, without any space-time interpolation. It would be good to mention that here explicitly.

9. Is my interpretation of Fig. 1 correct if I say that in Estonia only a circular area around the radar is used (up to 70 km range), while in Italy a rectangular area (60×60 km^2) around the radar is used? If this is correct, is there an explanation of why two different areas have been used? This should be included in the paper.

Printer-friendly version

Discussion paper

10. Equation (3) for Pearson's correlation coefficient is incorrect. It should be:

$$CC = \frac{\sum_{i=1}^{n} (r_i - r_m)(g_i - g_m)}{\sqrt{\sum_{i=1}^{n} (r_i - r_m)^2} \sqrt{\sum_{i=1}^{n} (g_i - g_m)^2}}.$$

11. For the definition of the normalized mean error in Eq. (4), the multiplication with 100% needs to be omitted in order to make it consistent with the results presented in Tables 1-6. I would also like to suggest renaming this statistic to the "normalized mean absolute error", which in my view is closer to what it actually is.

12. The authors could consider to also normalize the RMSE in Eq. (6) with the mean gauge rainfall. In this way, all statistics will be dimensionless. This is of course just a choice, and I would also be perfectly fine with leaving the definition as it is.

13. On lines 192-193, the cause for the more severe underestimation of R from Z_{H} in Italy than in Estonia is said to be the fact that there is more intense precipitation. However, doesn't this mean that the employed $Z - R$ relation is not suitable? Differences in raindrop size distribution (DSD) climatologies between Estonia and Italy may also cause differences. So it would be good to comment here on the suitability of the retrieval relations (Eqs (1) and (2)) for both regions.

14. On lines 198-199, it is stated that using different time intervals can help in understanding the effect of temporal sampling differences between radar and gauges. While this is certainly true, it should also be noted that using longer accumulation intervals will also lead to less severe errors (compensating underestimates and overestimates; the $R(K_{\mathrm{DP}})$ curve in Fig. 3 is a good example of this). I think a remark about this should also be added to the text. The same holds for line 219.

15. On lines 215-217, an important statement is made about the improvement that $R(Z_{\mathrm{H}}, K_{\mathrm{DP}})$ gives over the other methods. At first reading, I thought that this statement is too bold given the results presented in Table 1, but on second thought it is correct. What would have helped me is if something along the lines of "(i.e., each statistic is approximately as good at the best of the other two)" after "...other product's weak points" would have been included. You could consider including this here.

16. In Fig. 5, it is interesting to see that of the 4 highest 1-hour accumulations measured by a gauge, 3 of them have significantly higher radar estimates for $R(Z_{\mathrm{H}}, K_{\mathrm{DP}})$ than either $R(Z_{\mathrm{H}})$ or $R(K_{\mathrm{DP}})$. This means that for $R(Z_{\mathrm{H}}, K_{\mathrm{DP}})$, probably the best estimator of R is selected for most of the intervals (i.e., for at least one of the underlying 5-minute intervals $R(Z_{\mathrm{H}})$ is higher than $R(K_{\mathrm{DP}})$, and it is correctly selected for $R(Z_{\mathrm{H}}, K_{\mathrm{DP}})$). I think this merits some more discussion in the paper, especially since this is the case for 3 of the 4 highest 1-hour accumulations.

17. On lines 242-243, it is stated that the normalized bias is much smaller for the 24-hour accumulations than for the 1-hour accumulations. However, looking at the definition of the NMB in Eq. (5), there should be absolutely no difference between the two, if the same underlying samples have been used (i.e., it makes no difference whether you first sum over 24 hours, and then subtract gauge from radar sums, or if you compute the difference first and then sum over 24 hours because subtraction is a linear operation). So what is the cause of these differences? Is it because you use different underlying samples, possibly by taking only accumulations above 0.1 mm (see captions of Figs 4 to 7)? If this is the case, this stresses the importance of low-intensity rain for total rainfall accumulations. This should be explained clearly. The same holds for differences between 24-hour and 1-month accumulations.

18. If I compare the NMB presented for $R(Z_\mathrm{H})$ in Table 3 and the corresponding panel in Fig. 6, I'm surprized at the fact that the underestimation by the radar is so small. Is this because there is an extremely high density of points just above the black line close to 0 mm in Fig. 6?

19. Comparison of Figs 5 and 7 gives me the feeling that there may be an error in one of them. For example, if I roughly add all of the accumulations from $R(Z_\mathrm{H})$ in Fig. 5, the resulting amount of rain is much smaller than when I roughly add all of the accumulations from $R(Z_\mathrm{H})$ in Fig. 7. Furthermore, the number of accumulations exceeding 0.1 mm given in the caption is higher for Fig. 7 than for Fig. 5. This is impossible unless a different dataset has been used. So I suggest to take another careful look at the figures and the results presented in the tables.

20. Figure 7 seems to show two regimes for $R(Z_\mathrm{H})$, where one overestimates and the other underestimates for higher rainfall accumulations. It would be interesting to discuss this in the paper. I'm interested to learn if these regimes are separable by some other variable such as time, temperature, or something else.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2019-624, 2020.