Reviewer #2

Summary: This concise paper is an important application and demonstration of KDP estimation uncertainty for a devastating flooding case in China. The authors perform a robust statistical analysis of the different parameter choices for popular KDP estimation algorithms, compared to the operational algorithm for the CINRAD radar networks. This type of work has been done for synthetic observations, but not for real cases with such dense ground-based (gauge) observations for evaluation.

The paper is free of any major fatal flaws. However, there are a large number of mostly minor comments that need to be addressed. One structural comment -- there is not really a good concluding paragraph (see the first comment below). Additional proofreading is necessary. For these reasons, I suggest MAJOR revisions, although this is somewhere between major and minor.

We sincerely appreciate the reviewer for constructive comments on our paper. We have amended the manuscript as suggested. Please see below our response to your comments.

Comments:

1. The conclusion section is a nice summary of the study, but it sort of ends abruptly without wrapping up. The authors need to include a concluding paragraph (it can be brief), that brings the focus back out to the broader perspective. This is sometimes referred to as the “funnel technique” or structure. What do the results of this study contribute to the community’s knowledge or application of rainfall estimation? What do you recommend for future work, and how will your efforts contribute to the main goal of mitigating losses from devastating flooding events? Some answers to those questions are needed.

We thank the reviewer for the good suggestion. We have added a paragraph discussing the implications of this study as follows,

From the perspective of operational applications, the effect of smoothing on K_{DP} estimation is interesting. Our results show that the use of smoothing factor has minimal impact on K_{DP} for hourly rainfall accumulation below 100 mm, while its impact becomes more significant as the rain rate increases. This suggests the importance of employing an adaptive window length as used in the LSF method. However, current LP or Maesaka algorithm uses a fixed window length or a single smoothing factor. It is recommended to develop a new LP algorithm with an adaptive window length in the future. In addition, we speculate that the underestimation of 201.9 mm h^{-1} rainfall accumulation can be attributed to the inadequate assumptions about raindrop microphysics and unquantified vertical air motions. Although we cannot quantify their contributions in the Zhengzhou event, more delicate observational experiments are suggested to ascertain their impact on radar-based QPE.

2. L27: “seem falling short” should be “seem to be falling short”?

Corrected.

3. L31: What is meant by “parameterized reflectivity factor”? That is not standard usage, unless it means something different from the traditional equivalent radar reflectivity factor?

‘Parameterized reflectivity factor’ has been replaced by ‘equivalent radar reflectivity factor’.

4. L32: remove “the” before “attenuation effects”

Corrected.

5. L43: “infrastructures” should be “infrastructure”

Corrected.

6. L67: Maybe a reference for KDP being immune to beam blockage would be helpful to readers less familiar with the dual-pol products.

(Lang et al., 2007) has been added in the revised manuscript.

7. L69: “data was” should be “data were”. Same in L134.

Corrected.

8. L72: “derivation” should be “derivative”. Also, maybe adding “derivative with respect to range” or something like that to clarify?

Agree. This sentence has been amended as:

K_{DP} is one-half the range derivative of differential phase shift.

9. L78: The Maesaka reference should not be in parentheses according to the style guide…same in L147, L169, and elsewhere; also, no capitalization needed for “Linear”

Corrected. Specifically, the algorithm developed by Maesaka (2012) is referred as Maesaka algorithm in the revised manuscript.

10. L98: are there units for these numbers? Are they range gates?

Yes, they are range gates. In the revised manuscript, they have been expressed in km.

11. L105: Is there an objective routine for the removal of spikes, or is this done manually? (Either is fine, just indicate how it is done for reproducibility)

Both were done for ensuring the data quality of Φ_{DP}. In the revised manuscript, the following has been added.

To minimize the impact of those spikes on KDP estimation, the following procedures were made:

– Firstly, a linear fit was made to the raw $\Phi_{DP}(r)$ data for an interval of 5 km. The fitted values were labeled as $\Phi'_{DP}(r)$.

– Then, the point with $|\Phi_{DP}(r)-\Phi'_{DP}(r)|$ exceeding 10° was identified as clutter.

– Finally, a cubic spline interpolation was made to the identified clutter points.

These steps can effectively remove majority of clutter signals, however, local perturbation of Φ_{DP} can be on the order of 10° given the area of interest is so close to the radar. Therefore, we have also manually checked the Φ_{DP} fields and removed significant clutter signals.

12. L117: Is there a threshold used to define "significantly deviating"?

In the revised manuscript, this part has been amended as follows.

For the HYDRO gauges with hourly measurements, the inverse distance weighting (IDW) approach (Chen and Liu, 2012) was implemented to yield an estimate of hourly rainfall accumulation at a given HYDRO gauge site. Then, the observed value below 50% of the expected one was removed. This method was mainly used for identifying gauges which were not working due to power outages.

13. L122: "This may attribute" should be "This may be attributed to"

Corrected.

14. L154: "elevation angle dependence of KDP" – this confused me at first. I believe the authors are referring to the viewing angle of raindrops effect, and not any sort of vertical profile of KDP effects? Maybe some sort of clarification would help.

In the revised manuscript, this part has been amended as follows.

Here, radar observations at the elevation angles of 1.5°, 2.4°, 3.3° and 4.3° were used for the following considerations. (1) The dependence of observed KDP on the viewing angle is expected to be negligible at small radar elevation angles, i.e., smaller than 4.3° (Bringi and Chandrasekar, 2001); (2) Given the strong ground clutter contamination, we discarded the data recorded at the lowest elevation angle and K_{DP} estimates at elevation angles of 1.5°, 2.4°, 3.3° and 4.3° corresponding to heights about 0.083 km, 0.132 km, 0.182 km and 0.237 km, respectively, over the station were used.

15. L223: the word "cry" is not correct, and I don't know what is being stated.

This sentence has been amended as:

With a larger σ (Fig. 5b), $R(K_{DP})$ is still well below OTT/gauge observations.

16. L234-238: There shouldn't be any downdrafts experienced at the surface owing to mass continuity (i.e., downdrafts become diverging outflow at the surface). So, this argument does not make physical sense. There almost certainly will be downdrafts above this level, perhaps in the region where the radar is sampling. Are there any low-level

divergence observations from the radar or surface stations that could be used to estimate the downdraft speed at radar beam height? (This is admittedly crude, but would help indicate if the scale of the downdraft is significant or not.)

The reviewer raised a very good point. In the revised manuscript, we have added a figure showing the time serials of Doppler velocity and the radial divergence over the Zhengzhou national climatological station. The results indicate sustained divergence from 16:00 to 17:00 over the site reporting the 201.9 mm report. Therefore, we agree that the existence of downdrafts should not be ruled out.

17. L242-243: I'm not sure 6-10 m/s winds would be consistent with a "tornado." Is this a misunderstanding of the cited reference? In other words, did the storm produce a tornado, but the sampled winds were obviously much weaker? Please clarify.

We have clarified this point in the revised manuscript. Firstly, it is 'a tornadic squall-line storm'; Then, the wind speed was '4-min running averaged'.

18. L246-248: This seems really unlikely, in my opinion. Are there any references to support this conjecture about extremely narrow heavy rain shafts? This is important because it is alluded to in one of your conclusions (L325).

We have rewritten this part in the revised manuscript as follows, and the corresponding sentences in conclusions have been deleted.

Although this effect is difficult to quantify, we argue that it plays a minor role for the rainfall underestimation. By manually checking the movement of storms (merged K_{DP} observations at https://github.com/HaoranLiHelsinki/Figs_Zhengzhou, we found that the storm propagation speed is on the order of several kilometers per hour, contrasting with the much smaller radar sampling volume. Given the rapid changing nature of the storms, the sampling effect does not seem to be a major factor responsible for the rainfall underestimation.

19. L258: "imposes obviously oversmoothing filter" should be revised, maybe something like "imposes an overaggressive filter that obviously leads to oversmoothing"?

Corrected.