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Atmospheric Measurement Techniques Discussions

Interactive comment on "A Gaussian Mixture Method for Specific Differential Phase Retrieval at X-band Frequency" *by* Guang Wen et al.

Anonymous Referee #3

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Estimating Kdp is a quite difficult task in radar meteorology, being estimation based on computin a derivative of a range profile affected by noise and various measurement errors. Different methods exist, but are difficult to compare each other, since all comes with advantages and disadvantages. The manuscript by Wen et al. describes a novel approach, called Gaussian Mixture Method (GMM) is proposed and compare its performance with the classical method based on linear regression (LR). Results in terms of QPE computed with respect to 4 raingauges at different distance from the radar shows a slightly better (but not for all the raingauges, only for the two farther from the radar) performance of the new method. It is not easy to provide a convincing validation of Kdp estimation, but some suggestions to improve it are provided below (note than pages 2-14 describes the method, but only pages 14-16 are related to validation).

More difficult is to get an ultimate proof of the effectiveness of the method, but only a set of clues. The paper is correctly structured, but needs some improvement in order to be accepted for publication. The acceptance of the method will be depend also on practical aspects, such as the computational effort and robustness that, unfortunately are not considered in this manuscript. In the following, general and specific issues are listed.

General comments:

There is a problem in the use of symbols that affect the clarity of the manuscript. Kdp is estimated from the range derivative of differential propagation phase Φdp, whereas the differential phase (usually indicated with Ψdp)that includes the differential phase upon backscattering, that is not negligible at X band) is measured by radar. Moreover, in a paper about estimation methods, the estimated variable should be clearly distinct from the intrinsic variable. Usually, estimated variables are indicated with a hat on top. Adopting these standard symbols (or another set of symbols authors could prefer) would increase the readability of the manuscript.

Aspects related to computational cost are neglected. Some figures about the computational efficiency of the method need to be provided.

Introduction is quite confusing. It recall several Kdp estimation methods (some of them are missing, such as Vulpiani et. al. 2012, https://doi.org/10.1175/JAMC-D-10-05024.1), but mixes general Kdp estimation methods with other methods based on self-consistency of dual polarization measurements that are valid strictly in rain. One of the advantages of GMM is that it can provide the variance associated the Kdp estimate. I think the authors should explain why such result is important and how it can be used. Other methods, such as Kalman filter can provide the variance of the estimate, but even linear regression can provide a standard error as the measure of the goodness of fitting. The manuscript states that $\sigma(\Phi$dp) is constant. However, notoriously $\sigma(\Phi$dp) varies depending on SNR, width of Doppler spectrum and copolar correlation

coefficients that are not constant along the range.

Specific issues:

Page 1: Line 10, raingauge data used for validation are relative to two years and 3 months, not three years.

Page 1: Line 15, please use different symbols for the measure differential phase shift and its component related to propagation.

Page 2: Line 5-6, "Therefore. . .". This is something to be demonstrated, it is not a consequence of the previous statement.

Page 5: Line 25-30: Is there an implication of this sentence for Kdp estimation ?

Page 6: Line 10. What is "the maximum detectable range" ? is it the unambiguous range determined by the selected PRF ?

Page 6: Line 10-25: are the height above the ground or ASL ? for the elevation of 0.8°, at 4.4 km from radar the height of radar beam is 314.6 m. Is it correct ?

Page 8: Line 1-2: There is something odds in the sentence. Are clutter-contaminated echoes well identified or not ?

Page 8: lines 9-11. What is the need of eliminate hail contamination ? Is not Kdp computed in hail ?

Page 8: Lines 18-32: I recommend to use degrees consistently in the manuscript and add a legend on x-axis (this is valid for all the figures showing profiles). Given the range of unambiguous differential phase, it seems that MZZU features the alternate polarization scheme. About the jump at the beginning of the profile in Fig. 3b: to be interpreted as ïĄď co there should a peak of Zdr. Is this the case ?

Page 9. Lines 1-2: negative Kdp can be due also to non uniform beam filling (Ryzkhov 2007 https://doi.org/10.1175/JTECH2003.1), which is a further source of error in com-

puting Kdp (see manuscript at pag 15).

Page 10. Again, please be sure that the interpretation of differential phase upon backscattering is correct.

Page 11. Line 29. Section explains how unfolding works. Sometimes there are false alarms in unfolding. Could you provide the rate of correct unfolding for the dataset used?

Page 13, line 22-23. Could authors please provide more details on how the weights are derived?

Page 14: Line 23. Please provide information about how hourly rain is obtained from instantaneous radar measurements.

Page 15: Line 13-24. The presence of too many missing data for LR indicates a clear advantage for GMM. However, it is not clear what is the cause of these missing data. Surprisingly is the presence of Kdp estimates beyond the edge of differential phase shift rays, especially for LR. Is it the effect of smoothing and/or extrapolation?

Page 15: Line 25. Notoriously, in rain, Kdp, Zh, and Zdr exhibit a self-consistency property. I recommend authors to exploit this property to validate their method to see whether the Kdp estimate is consistent with Zh and Zdr measurements.

Page 32: Figure 7: Please use larger font size.

Page 34: A Kdp rain algorithm is supposed to work well (at least better than a R(Z) algorithm) for high rainfall rate. Instead, there are evident underestimates for high rainfall rates that likely affect RMSE. How about LR: does it yield the same behavior?

Page 36: NB is not in mm.

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