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Interactive comment on "Automated rain rate estimates using the Ka-band ARM Zenith Radar (KAZR)" *by* A. Chandra et al.

Anonymous Referee #3

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1 General comments

This paper presents the refinement of an existing technique (Matrosov, 2005; Matrosov et al., 2006) for attenuation-based rain rate retrievals from the reflectivity gradient of Ka-band radars, taking advantage of the very close link between rain rate and rain attenuation at Ka-band. The main novelty in this paper is to detect the profiles whose reflectivity gradient is not dominated by attenuation but by microphysics processes, thanks to a threshold on the measured Doppler velocity, determined by simulations with a simple microphysical model. I acknowledge the attempt of the authors to develop a technique which can provide interesting statistics on rainfall in particular thanks to the long-term measurements of the ARM Ka-band radars. However, I am concerned with

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some strong assumptions in this algorithm and their effect on the retrieval needs to be assessed. In other word, this technique seems indeed robust in terms of operability but not in terms of accuracy. Finally, the writing and typing quality is poor and would have deserved more proof reading before submission.

1. In the whole paper, you never mention a third effect on the reflectivity gradient which could be called dynamics effect: the wind shear can produce non-vertical fallstreak of precipitation and lead to strong negative or positive gradient of reflectivities which have nothing to see with a microphysics or attenuation effect. How would you correct for this effect? If no correction can be easily proposed, I would like to see this issue mentioned in the paper as well as the implications on the accuracy of the retrievals.

2. The second major issue with this technique is the determination of the threshold on Doppler velocity to separate the two regimes of precipitation. Firstly, the threshold is defined in terms of fall velocity while radar measure the Doppler velocity, hence, a non-negligible vertical wind could lead to a wrong separation of the two regimes. Secondly, this threshold is certainly the best possible but the two regimes are not so well separated and a different threshold could lead to different retrievals. Some statistics on the variability of the retrieval using different thresholds could help to assess the accuracy of the retrieval.

2 Specific comments

1. P.1810, l.3-7: The whole paragraph needs to be re-written: you don't need to motivate the study of clouds in this paper, I don't understand the last sentence ("Deployments of multiple radars . . .")

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2. P.1810, l.8-11: Can you please describe explicitly why retrieving precipitation can help to characterize the microphysics and radiative effects of clouds?

3. P.1814, l.12: "The results are plotted as a function of the mean Doppler velocity.". This is wrong and is one of the biggest issues of this paper: the results of the microphysical model are function of fall velocity while Doppler velocity is the sum of fall velocity and vertical wind. You never mention the effect of vertical wind in the determination of the two regimes (dominated by attenuation or microphysics effects), while the vertical wind can be significant in convective cores, even at low levels.

4. P.1814 and 1816: 5 m/s is indeed the best fall velocity to separate the two regimes, however, around 5 m/s, the microphysics effects are still significant (gradient of 2dB/km) and can produce a non-negligible error on attenuation-based estimates. In this paper, this problem is resolved by using the non-attenuated reflectivity reference of S-Pol, but what happen if no Rayleigh reflectivity reference is available?

5. P.1815, l.11: The effect of water vapour should be balanced compared to the other effects: "significant" may be too strong. However, you never mention how you take this effect into account in the attenuation-based retrieval of rain rate.

6. P.1816, l.3-6: The figure 6a shows indeed some correlation between the Doppler velocity and the Ze-R relationship, but I don't see two regimes clearly separated by a Doppler velocity threshold which could be determined unequivocally. So this figure can at most confirm the existence of two somewhat separated regimes, but it cannot be used to identify the Doppler velocity threshold.

7. P.1816-1817, section 3.2.1: The full section is obscure and needs to be rewritten:

   (a) Why do you use rain layers of varying depth? Which depth is chosen at the end?

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(b) It is clear that, in case of light rain, the reflectivity may not decrease with height, but it can also happen in case of non-vertical fallstreak of heavy precipitation for which we could observe positive gradients of reflectivity and Doppler velocities larger than 5 m/s. You say that, this allows determining the profiles suitable for the attenuation technique but how the rain rate estimation is done in such cases?

(c) You mentioned in section 2.1 that S-Pol vertical scans above the KAZR are available only every 15 min and your algorithm uses the Rayleigh reference profiles of the S-Pol radar to detect the cases where microphysics effects are not negligible. How do you detect them in the data between two S-Pol vertical scans?

(d) Finally, what is the rate of profiles that are kept for the attenuation based retrieval? How much rainfall do we miss by eliminating such profiles?

(e) " . . . this results in underestimation of the cloud-top heights from the KAZR for higher rain rates.": Since this is not the real cloud top height, I would not use the term "cloud top height" but rather something like "maximum height were the reflectivity is significant".

(f) "A near-linear relationship between the gradient of reflectivity due to attenuation and the rain rate is clearly seen in Fig. 5d.": Matrosov (2005) found indeed a linear-relation between attenuation (i.e. gradient of reflectivity) and rain rate, but there is no way that Figure 5d can allow you to claim this.

8. P. 1817, l.22: ". . . the mean and standard deviation of top 25% of the Doppler velocities . . . ": I guess that you mean the 25% most frequent Doppler velocities?

9. P. 1818, l.11:" The computed reflectivity values from DSDs are compared with the KAZR reflectivity values and corrections are applied to the Ze–R relationship.": How well do they compare? Such comparison should show some scatter

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due to the use of the Ze-R relation and also because of the volume mismatch between instruments. What corrections are applied to the Ze-R relationship? Don't you think that it be would more relevant to derive the relationship directly from the 2DVD calculations? It is well known that Z-R relationships are very variable and plenty of them have been proposed. Can you make some comments about your new relationship: how far is it from the other existing Ze-R relationships at Ka-band? What is the effect of the filtering from Doppler velocities? How representative is it for other rain data (other location, other seasons). Do you think that the relationship needs to be updated for each rain event, and that your technique always requires some disdrometer data?

10. P. 1818, l.18: "The comparison is shown for R > 5 mm h-1...": Why not showing the data below 5 mm/h? It is part of your algorithm and it should help to discuss its limitations. In particular, if the scatter is important, it would give you a good argument for the use of the attenuation based technique.

11. P.1818, l.24-25: "... the comparison in terms of the time series and scatter plot agrees reasonably well." These are only qualitative observations of the quality of the retrieval. If you pretend to be able to "derive robust statistics of rain rates" (from the abstract), I would like to see also some quantitative comparisons (like standard deviation and bias) of the rain rate retrieval with rain gage and disdrometers measurements.

12. P.1818, 25-26: You need quickly introduce the other rain products that you are using to validate your retrievals or at least give some references.

3 Selection of technical corrections

I provide here a selection of the technical corrections because the manuscript need substantial correction overall. I will provide a more detailed list once I'll have an improved version of the manuscript in terms of science.

1. P.1808, l.13: "is implemented" seems unnecessary.
2. P.1809, l.1: The reference should be Mather and Voyles, 2013 (the year is correct in the references list). Same remark at p.1810, l.15.
3. P.1809, l.18: Moran et al. is missing in the references list.
4. P.1809, l.18: There is only one article written by Kollias in 2007 in the reference list.
5. P.1811, l.10: Two verbs in the sentence "The KAZR is a profiling Doppler radar operates at Ka-Band ...". However, I don't think that it is necessary to repeat these characteristics of the KAZR which are identic to the MMCR and already described in the introduction, and because the KAZR is the focus of the following section.
6. P.1811, l.23: The reference should be Feng et al., 2014 (the year is correct in the references list).
7. P.1812, l.9: "... and non-significant ? (non-hydrometeors) are removed.": a word is missing.
8. P.1812, l.16: "The KAZR is heavily attenuated for high rain rate episodes are seen in the reflectivity field...": meaningless sentence, please proofread more carefully.

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9. P.1813, l.1-2: “Consequently, the DSD parameters, in particular the concentration parameter.”: same as above.
10. P.1814, l.19: “In Fig. 3, all calculations are done using the Mie theory (and only attenuation).”: same as above.
11. P1814, l.21: The reference Matrosov (2004) is not in the references list.
12. P1814, l.22: Matrosov (?): year missing
13. P1814, l.27: “derivative”: gradient would be more explicit.
14. P.1816, l.7:”. . .the drops are considerably small that the attenuation . . .” word missing.
15. P.1818, l.8:”. . . diameter bin form disdrometer . . .”: I guess that you mean “. . . diameter bin of the disdrometer . . .”
16. P.1818, l.13: “Rain rates from the KAZR are continuously retrieved in two steps.”: From this sentence, we understand that you use two consecutives steps for the whole dataset. I would suggest replacing “in two steps” by “using two techniques for two categories of rain rate”.
17. P.1819, l.1: The sentence would be more understandable If you replace “. . . are based..” by “. . . classified from . . .”
18. P1819, l.3: The reference should be “Geerts and Dawei (2004)”
19. P1819, l.3 to 8: These sentences are very obscure, please rewrite.
20. P1819, l.7: The reference should be “(Steiner et al., 1995).
21. P1819, l9: The differences can also be due to the errors associated with the different techniques.

22. References: Keeler et al. (2000) is missing from the list, while Kollias et al. (2002) and Zhang et al. (2013) do not appear in the text.
23. Fig.2: What does mean “-ve” in the caption?
24. Fig.3: It is possible to deduce from the text but please mention to which process correspond each group of lines in this figure.
25. Fig.5: Subplot (a): what is the black line? What is the x axis? (c) and (d): the rain rate estimate comes from the disdrometer data?
26. Fig.6: The points in subplot (b) are not easy to see. Please use larger markers like in (a).
27. Fig.9: From only the rain event of fig.5, I see that you have a lot of data. Why are you using only 5 bins in the histograms?

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 1807, 2014.