The manuscript titled “Identifying Insects, Clouds, and Precipitation using Vertically Pointing Polarimetric Radar Doppler Velocity Spectra“ by Williams et al. describes a two-part algorithm to distinguish radar signal related to hydrometeors or insects in vertically pointing polarimetric Doppler radar measurements. Two techniques, one relying on the morphology of the co-linear (CoPol) Doppler spectrum and the other on the linear depolarization ratio (LDR) specrum, are independently applied and the resulting hydrometeor/insect masks are then combined. The performance of the method is shown by a good agreement between the hydrometeor mask and cloud base height retrieved from ceilometer, which provides an independent observation.

The authors make a good effort to illustrate the problem they are addressing (Figs. 1 and 2) as well as the reasoning behind the chosen approaches (Section 3, Figs. 3, 4, 6 and 9). Variables are clearly defined, and except a few small exceptions (see specific comments below) the description of the algorithm is detailed enough to easily follow what has been done. I also appreciate that the authors provide so many cases with the resulting hydrometeor mask in the supplemental material. The authors are addressing a common issue with cloud radar observations and although the concepts behind the described algorithm are not novel, the manuscript provides the community with a practically applicable method. My main concern with the algorithm is related to the choice of thresholds: the authors demonstrate their choice of thresholds (Figs. 7 and 10) using a sub-set of one hour data for two example cases, one case for each algorithm branch, without explaining why the specific sub-set was taken or if the thresholds obtained for the one hour example cases are generalizable to other cases. Furthermore, I find the presentation of the work lacking and needs to be improved before the manuscript can be published.

**Minor comments**

1. Page 1 Lines 15-17 (abstract) “The insect-hydrometeor discrimination method uses CoPol and XPol spectral information in two separate algorithms...”
   This sentence might be misleading because the algorithm does not utilize the XPol spectra as such, but the LDR spectra. Although strictly speaking the LDR is based on “XPol spectral information”, I’d suggest to reformulate for clarity.

2. P. 1 L. 25-26. Gives the impression that the hydrometeor mask bottoms are always within +/- 100 m from ceilometer cloud base height when Fig. 13a indicates that this is the case for 71% profiles. The authors should acknowledge here that although most, not all, hydrometeor mask bottoms are within the +/- 100 m from the ceilometer base to accurately reflect the content of the paper in the abstract.

3. P. 2 L. 26-27 and Fig. 1. Information on where observations in Fig. 1 are obtained is missing.

4. P. 2 L. 27-34 and Fig. 1. The example nicely illustrates the motivation for the study. Have the authors considered also showing in Fig. 1 the reflectivity masked with the hydrometeor mask developed in this study?

5. P. 3 L. 13-14. It is not clear to what the “*operational* Doppler velocity spectra processing routines” refer to, is this the operational ARSCL processing, the algorithm presented in this study, or perhaps something else?

6. The introduction does not provide enough detail on how insects and hydrometeors have been identified in previous studies to allow the reader to understand the difference in this manuscript.

7. P. 3 L. 9-14. In my opinion, the description of the algorithm here is more detailed than is necessary for an introduction. Instead, the authors could make clear the novelty of their approach compared to previous work.

8. P. 3 L. 17. I suggest to also mention the (quantitative) comparison with the ceilometer measurements here, as they are more substantial for evaluating the performance of the algorithm.

9. P. 4 L 14. There appears to be a problem with the formulation as it seems to me there should not be a Doppler velocity power spectra for each velocity bin. Perhaps the authors mean “The received signals are processed to yield co-polarized [...] and cross-polarized [...] power at each velocity bin $v_i$ and range gate $h_j$”, or alternatively “The received signals are processed to yield co-polarized [...] and cross-polarized [...] Doppler velocity power spectra at each range gate $h_j$”.

10. P. 4. L. 24-25. Unfortunately I don’t understand what is meant by “retrievals for the GE mode available in the ARM data archive”. Is the algorithm presented in the paper also applied for the GE mode and made available in the ARM data archive?

11. P. 4 L. 22-35. The citations provided seem all to refer to datasets. The authors should also provide references for technical information on the instrumentation as well as data processing and retrievals applied, where available.

12. P. 4 L. 31. Technical specifications for Doppler lidar are missing: at least the model and wavelength should be mentioned.

13. P. 6 L. 4-5. It is not clear what is suspicious about the lack of LDR above the cloud base.

14. P. 6 L. 24. I did not find the mentioned sequential spectra profiles in the supplemental material.

15. P. 10 L. 2. “By eye” is rather ambiguous, and requires a trained eye from the reader to follow the authors thoughts. The authors could elaborate in text or add labels in the figure to make it clear what they wish to communicate.

16. P. 10 L. 4-8. Sect. 3 describes the characteristics of the radar measurements, specifically LDR and CoPol Doppler spectra, typical for different kind of targets (cloud, precipitation, insects) and forms the basis of understanding the authors reasoning in the following section. I found it confusing to discuss the hydrometeor mask here, before the algorithm has been described. I suggest simply stating that the mask will be discussed later and moving all of the content regarding the mask to a more appropriate section later. Although Fig. 4d could also be moved to a later section, I agree with the authors choice to include the mask in the same figure as the other variables to allow easy comparison.

17. P. 10 L. 6. It is not obvious why it is surprising that the hydrometeor mask is affirmative (after all, the reader does not know how the mask is defined yet), more specifically it is not clear where in the time-height domain the presence of hydrometeors is surprising. I would argue that from about minutes 20 to 40 below 1.5 km it seems rather obvious from Fig. 4a and b that there is nearly continuous precipitation. Since spectra in Sect. 4 is mentioned (P. 10 L. 7-8), I gather that the authors intend to point to the layer below 1.5 km from minutes 45 to 55 (?) as particularly challenging to discriminate between insects and hydrometeors. Clarification needed.

18. P. 10 L. 12. This claim is not supported by what has been shown so far, specifically it has not been shown that examining the texture of the Doppler spectra helps with discriminating between insects and precipitation when precipitation is occurring. I suggest the authors show a spectograph and/or individual spectra for this time (similarly as in Fig. 3 in Sect 3.1.) in Sect. 3.2 and describe which features in the CoPol and LDR spectra suggest the presence of hydrometeors and insects in specific regions of the velocity-range domain. Given the central challenge of separating insects and precipitation when they occur in the same pixel, and the benefit of the evaluation in Doppler velocity domain provides in this regard, I think extending the discussion in Sect. 3.2 would be beneficial.

19. P. 10 L. 19. I believe here should probably be LDR spectra, not XPol?

20. P. 10 L.31-32. “it is assumed that the power in any location ($v_i$, $h_i$) is due to either insect or hydrometeor scattering, and not both.” Could the authors comment on the validity of these assumptions, namely that all signal is related to insects or hydrometeors but nothing else, and that both cannot occur at same height and Doppler velocity?

21. P. 10 L.31. Should $h_i$ be $h_j$?

22. P. 12 L. 5-6. “signal power is expressed in decibel units to remove signal magnitude dependencies that occur between cloud droplet (order of 10 dB) and raindrop (order of 50 dB) observations.” I cannot follow the authors’ logic here, it seems to me that although using decibel units there is a difference in magnitude, 10 vs. 50 dB as stated by the authors.

23. P. 12 L. 19. “texture shown in Fig 6a” - probably a typo, texture is shown in Fig. 6**b**.

24. P. 12 L. 25 and Fig. 7. Why did the authors choose to exclude spectral regions with LDR here? I understand the purpose of the CoPol texture algorithm is to determine whether insects are present when no LDR is available, but I do not see the reason to exclude these data from Fig. 7.

25. Figs. 6d,. 7a,b, 9d, and associated text. I don’t see why the authors are showing and discussing STD(texture). From Fig. 7b it is seen, and on P. 13 L. 15 it is stated that STD(texture) and Max(texture) closely correlate, so that no additional information is gained from STD(texture) and it is also not used for the insect-hydrometeor-detection algorithm. I suggest omitting STD(texture) because it plays no role in the described algorithm (if the authors wish they could include a sentence stating that STD(texture) was investigated), or moving the figures and discussion to an appendix.

26. CoPol texture algorithm (Sect. 4.1). Fig. 7b and c do not show clearly separated two populations but considerable overlap in the Max(texture) distributions. Also Luke et al. (2008) mention that differences in the radar Doppler spectrum texture between insects and hydrometeors is sometimes not obvious. The authors should discuss this challenge and its implications for their classification.

27. Sects. 4.1 and 4.2 give the impression that the Max(texture) and Mean(LDR) thresholds for the algorithm were chosen based on one hour of measurements each. Have the authors checked that the chosen thresholds are appropriate beyond the one hour examples?

28. P. 15 L. 1-3 and 12. Should it be $mean[S(v_{i\pm2}, h_{j\pm1})]$ and $STD[S(v_{i\pm2}, h_{j\pm1})]$?

29. The method to estimate noise the XPol or LDR spectra is not mentioned although it is described for CoPol spectra.

30. Figs. 6h, 9h, 10a,b, and associated text. Similarly as for the texture algorithm (see comment 26), I do not see the need to show and discuss STD(LDR) as it is not utilized in the insect-hydrometeor-detection algorithm.

31. P. 15 L. 2. The authors mention that they only include profiles below 1.5 km in Fig. 10 to avoid ice particle scattering. Is the LDR algorithm only applied for signal below 1.5 km or at temperatures where ice is not expected? If yes, this should be mentioned. If no, why are the authors excluding values above 1.5 km from Fig. 10 if the classification is still performed for regions where ice particle scattering is expected?

32. P. 19 L. 6-7. The authors should give an explanation for the “3-member temporal continuity filter”.

33. P. 19 L. 9. I could not find QC2 in the Supplemental Material.

34. P. 20 L. 21-27 and Fig. 13. This comparison between the hydrometeor mask and the ceilometer cloud base height nicely demonstrates the performance of the described algorithm to detect hydrometeors from insects for non-precipitating clouds. Have the authors considered any methods to validate the hydrometeor mask when precipitation occurs, for example by utilizing surface level precipitation measurements?

35. P. 21 L. 6-17 and Fig. 14. Could be omitted. The lidar is similar to ceilometer and does not really provide anything additional to the evaluation of the performance of the algorithm. I also fail to see the value of the total sky imager (TSI) photo for evaluating the algorithm. If the authors wish to show the TSI picture (Fig. 14b) it could be moved to the beginning of the manuscript when the case is introduced.

36. The authors mention that the CoPol spectra texture algorithm allows for filtering insects in the absence of XPol signal. It could also be mentioned that the texture method is applicable for radars without a XPol receiver.

37. P. 23 L. 36-37. The authors should check that the Supplemental Material provided agrees with what is stated in the manuscript.

38. Fig. 3b, d. Is it necessary to show the X-Pol spectograph (Fig. 3b) and example spectra (Fig. 3d)? Fig 3b and d are not discussed in the text, and Fig 3b is near duplicate with Fig. 6e.

39. Fig. 4d and 12b,c. The colorbars seems to have a mistake, the figures look like red is 1 and 0 is white.

40. Fig. 5. What is *ht* in box 10?

41. Fig. 5 Box 5. Should probably be $S^{XPol}(v_i, h_j)$?

42. Figs. 7c, 10c and 13c. Colorbars unnecessary.

43. Fig. 8a-c and 11a-c. All colors not red or blue could be removed from the colorbars.

44. Fig. 13c. What is the purpose of this figure? Could be omitted.

45. Throughout the manuscript both CoPol (XPol) and Co-Pol (X-Pol) are used, should be unified.

**References**

Luke, E.P, P. Kollias, K.L. Johnson, and E.E. Clothiaux: A Technique for the Automatic Detection of Insect Clutter in Cloud Radar Returns. *J. Atmos. Ocean. Technol.*, 25, 1498-1513, doi:10.1175/2007JTECHA953.1, 2008.