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Comment on amt-2021-267

Anonymous Referee #2

Referee comment on "Identification of concurrent clear-air and precipitation Doppler Profiles for VHF radar and an incorporating study of strongly convective precipitation with dual-polarized microwave radiometer" by Shih-Chiao Tsai et al., Atmos. Meas. Tech. Discuss., <https://doi.org/10.5194/amt-2021-267-RC2>, 2021

The manuscript "Co-observation of strongly convective precipitation using VHF atmospheric radar and dual-polarized microwave radiometer during a typhoon passage" by Tsai et al. presents roughly 30 minutes of data collected in 2013 by the VHF Chung-Li radar in Taiwan during the passage of the Typhoon Trami. An abundant part of the paper is spent on the description of the data processing while the case study is presented as an application of the measurement and processing technique. The observations are connected with the presence of an intense updraft which has brought substantial amounts of liquid water above the freezing level likely causing an intensification of the precipitation through various ice microphysical processes.

GENERAL COMMENTS:

I had a hard time understanding what is the main message of the paper. The title of the paper suggests a measurement report where the main focus is the actual measurements and the combination of radar and radiometer observations. However, the abstract, the conclusions, and the vast majority of the paper focus on the techniques implemented for the processing of the VHF radar Doppler profiles. In particular, the introduction of the paper discusses only the state of the art in terms of Doppler peak finding algorithms without any mention of previous observations of strong, convective precipitation with VHF radars or the combination of radar and radiometer. In the introduction and conclusions, those measurements are discussed as an "Application example" of the profiling techniques. If it is really the author's intention to put the focus on the profiling technique I suggest modifying the title of the paper accordingly, alternatively, the manuscript should be significantly improved towards a more measurement-centric approach. Considering the importance that is given to the profiling technique in the introduction and conclusions of the paper and also the aim of AMT I will proceed in my review considering this as the main message of the paper.

The paper presents two distinct algorithms to identify the peaks in Doppler radar spectra. The introduction of the paper focuses on VHF and UHF radar techniques, for which, I must

admit I am not an expert. However it seems to me that the introduction could benefit from additional considerations for similar techniques that have been developed for precipitation and cloud radars (1-100 GHz) which can be considered sensitive to hydrometeor particles only, but the mathematical foundations of peak finding in Doppler spectra are effectively the same. Also, it seems to me that the identification of air and hydrometeor peaks in VHF radars is a far easier task (with respect to peak identification in cloud radars). This is probably because of the selected application example. The respective mean peak location seems to be separated by roughly 10 m/s while their width is around 1 m/s avoiding the problem of the overlapping of the Doppler peaks. Honestly, I do not see why one cannot perform separation and tracing of the peaks using basic clustering algorithms in these cases.

The motivation for the development of a new profiling technique is unclear to me. The introduction part stresses the unavailability of a unique algorithm that performs both the peak identification and their tracing along the vertical, however, it is unclear to me how the algorithms that perform the tracing alone work without a peak identification. Also if established algorithms for peak separation and tracing exist what prevents one from combining them instead of developing a new one from scratch?

The algorithms themselves are not presented very clearly. The core component of the algorithms relies on the implementation of a certain function in well-established commercial software (MATLAB, namely the contour and the peakfind functions) with not much addition from the authors. The original processing steps are presented in a way that is very difficult to interpret with little or no graphical examples. Moreover, the algorithm is implemented via several numerical or logical functions that rely on specific parameters which appear to be quite arbitrary. There is a consistent lack of explanation of the rationale behind the implementation choices which makes it difficult to properly judge the quality of the technique or to suggest improvements.

The authors mention that the technique is automatic, but it seems also that several parameters have to be selected by the user and the application example proposed only provides a limited amount of data to prove the technique's performance. I have the feeling that the algorithm parameters have to be tuned according to the specific use case scenario in order to get the best performances. This makes the procedure of Doppler profiling a rather manual task and not an automatic one.

Finally, the quality of the presentation and the graphics needs to be improved in order to facilitate the reading and understanding of paper content.

At the moment, it seems to me that the paper does not provide sufficiently innovative material to be published. Also, the quality of the presentation does not match the journal standards. I suggest reconsidering the paper for publication after a major revision, in particular, I would suggest the authors focus on the following aspects:

- 1) Use a title that clearly conveys the paper main message
- 2) Improve the quality of the graphics: Label subplots, avoid small fonts and plotlines (plot fewer lines instead), provide graphical (and theoretical) examples of the algorithm processing steps.
- 3) Explain the algorithm design choices with their scientific background. Why is a certain step performed? Why does a certain parameter have that precise numerical value? Why not use already developed processing methods?
- 4) If possible, provide validation of the results against independent measurements.
- 5) Explain better the scientific context of the provided application example. What is the scientific value of the measurements?

DETAILED REVIEW:

L13 - What does it mean that the peak locations are redundant?

L52 (and many other places) - The authors use the term "Doppler parameters" many times without defining it. What are those parameters?

L71 - Why was it necessary to derive 3 Doppler spectra per sample using 3 different FFTs?

L80 - The only motivation I can imagine for constructing such a 2D field is that the contour function works with 2D fields only. Is this the case? The 2D functions are symmetrical around the "true" frequency axis? Why standard deviation is specifically $N_{fft}/8$? Standard deviation should be in the same measuring unit as the axis, which means that 8 has units of seconds, right?

L90 - n is renamed N_{level} . Should use only one variable name

Fig 2 - the subplots should have labels. The second subplot must have a much larger axis, it is not possible, at the moment to appreciate the various contour lines. Also, other figures display spectra as a function of velocity and not frequency, it is better to be consistent and velocity holds a more geophysical significance.

L103:105 - I still think that the gaussian functions are symmetrical, so the centers should not be shifted at all.

L110:113 - I did not understand this phrase. I think this is a very central point since it should explain how to go from a contour map to some peak locations. I think that the explanation could benefit from some visualization of either real data or example data that could show how this process actually works. Also, it would allow discussing some deficiencies of the method. As an example right peak of Fig 2 (first panel) will likely be highlighted by two separate contours but in reality, it seems to me like only one peak with a lot of noise; thus the contour method would identify only one of the two subpeaks shifting its estimated mean Doppler velocity.

L119 - Are the 5,7,9,13 lines separated by the resolution of the Doppler spectrum? Isn't this limiting the estimation of the spectral width? Is the range of spectral widths that one can estimate dependant on the spectral resolution (number of FFT points) then? I think that this is rather arbitrary and in nature, the width of a spectral peak depends on a

variety of factors, especially for precipitation peaks the algorithm choices made here seem very restrictive. Also, what is the reason behind the choice of selecting the Gaussian fit with the minimum standard deviation?

L137 - Why is this passage necessary? Why the findpeaks function does not find representative mean locations?

L130:140 Again, this explanation could be improved with some graphical example on a real or fictional spectrum.

Paragraphs 2.1 4) and 2.2 2) are not part of the respective peak finding algorithms and are redundant. They can be joined in one subsection. Logically, section 2 should be structured as follows:

2 Doppler spectra analysis

2.1 Preprocessing

2.2 Peak identification

2.2.1 Contour method

2.2.2 Findpeaks method

2.3 Estimation of mean peak velocity and width

Fig 3 x-axes should match

Fig 4 a) and b) It is very hard to distinguish between the black/red/open circles. Distinguishing lines is even harder. Maybe this figure could be reduced to a couple of specific portions as it is done in panels c) and d)

Fig 5 Again, not really readable. Focus on height regions or reduce the vertical resolution
Section 3 - Rather than a validation of the method this section shows an application example. There is no independent verification of the obtained results performed. The analysis shows a rather consistent output between the two methodologies. I am not sure if one should take this as a confirmation of the validity of the methodologies applied. Again, the used example case seems easy enough to not pose too many challenges, however, I do not see a clear validation of the methodologies, a comparison of the respective benefits and flaws, a discussion of the advantages of these methods with respect to already established ones.

L298 - Why 1, 1.5, and again 1.5 times V_{shear} as thresholds?

L300 - The fact that all parameters are changeable to adapt to atmospheric conditions makes me think that the method is not really automatic, which is in contrast with what is stated in the conclusions.

L350 - I do not agree with this statement. Any object emits radiation at every wavelength. The radiometer will detect the radiation coming from any emitting substance and not absorbed along the path. It is true that ice emissivity is lower than the one of water vapor or liquid water and might be negligible, however, it is not null.

L390:402 - The measuring principle of the radiometer is not discussed enough. In my opinion, it is not sufficient at this stage to only cite Hou et al work to describe the radiometer. The authors should clearly state what are the measuring frequencies of the DPR (I guess 4CH means 4 channels and thus all 4 are used for the LWP retrieval?). The quantities LWP, LWC, and LWR are taken as basically observational truth without any discussion about the measurement uncertainties. In particular, it would be interesting to know how the radiometer estimates the content of precipitating raindrops and cloud water. Also, as far as I remember, radiometers do not work really well when they are covered by water which I would think it is the case considering the intense precipitation event. How does that impact the measurements?

L359 - According to Fig 3 the rain rate Around 05:30 is close to 50 mm/h which makes it very likely that the signal observed in the PD and LW time series is just a result of the decreased accuracy of the radiometer stated in this line

L392 - 402 I am not sure I can understand the author's interpretation of the measurement. According to the observation geometry the radiometer observes only the atmosphere above the melting layer at the radar distance. This means that any drop of LWP is more likely to be connected to decreases of liquid content below (and hence closer to the radiometer). Moreover, in presence of a strong updraft, I expect the LWP to increase above the melting layer, or? I think the authors should explain better their interpretation.

Fig13 (and discussion around the WRF model) - The authors say at L446 that the time resolution of the simulations is 30 minutes however in the figure the drop in liquid water content seems to be connected to a much higher time resolution; the colors vary much faster, what is the reason for that? Also, do the WRF cloud-microphysics implement the processes (like secondary ice generation) that are speculated to be responsible for the observed features? Finally, is the time of the forecast (5.5 hours) enough to be out of the expected spin-up time of models?

L400 - I LWC decreases shouldn't also the radar echo intensity decrease? It is difficult to judge from the plots of Fig 9 what is the average intensity of the signals. I think one can add to the plots of fig 9 a secondary x-axis that shows the profiles of radar reflectivity.

L462 - I do not understand why the authors say that none of the two approaches can identify the peaks completely. What is the meaning of the word "completely" here? Also in the discussion of the methods, it is stated that the two approaches give the same results, this is in contradiction with what is stated here where the approaches are described as complementary

MINOR POINTS:

L17 - "in the tracing process" is redundant

L19 - even WHEN the atmosphere was disturbed severely

L21 - The verb "to signify" does not really make too much sense to me in this phrase

L33 - preposition "in" is incorrectly used

L43 - "more accurately" or "in a more accurate way"

L50 - What is the meaning of "exclusive" here? What has to be excluded?

L71 - points

L72 - The word "respectively" should not be here. Respective to what?

L73 - 64 points?

Fig 1 - This figure is not really necessary to me. Both algorithms are very linear with no forks or loops. It does not add anything to what is already in the text.

L108 - the term "velocity interval" is introduced without explanation

L111 - the separation ... IS larger

L223:224 A further comparison ... is shown

L226 - the word "compared" should not be here

L229 - "no matter what the methods were used, the estimated spectral parameters were in good agreement,"... in good agreement with what?

L305 - What is it meant by the term "apparently"?

Fig.13 - colorbar has no label for the shown quantity

Fig 11 (but also Fig.3) - the time axis is given in floating-point hours. It would be much better to have consistently hours and minutes as the format of times throughout the paper. Here is critical for the examination of the simultaneous measurements

A FEW REFERENCES FOR PEAK FINDING ALGORITHMS IN CLOUD RADARS

Radenz, M., Bühl, J., Seifert, P., Griesche, H., and Engelmann, R.: peakTree: a framework for structure-preserving radar Doppler spectra analysis, *Atmos. Meas. Tech.*, 12, 4813–4828, <https://doi.org/10.5194/amt-12-4813-2019>, 2019.

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