RC5 - General SUGGESTIONS/COMMENTS

"Monitoring and quantifying wind turbine clutter in DWD weather radar measurements"
by M. Frech et al.

We thank the reviewer for his helpful and constructive comments!

Comment 1: This work reports the impact of WT on weather radar measurements; personally, I find it unbalanced: I mean, a great/relevant emphasis on the spectral component and a little one on key polarimetric variables such as rhoHV, Zv and Zdr.

Response:

Thanks for your comment. To our knowledge the DWD is the only weather service who has a dynamic WT detection algorithm running operationally on an IQ-data level. That algorithm specifically has not been developed to properly identify and quantify the WT impact on dualpol data. That will be an important aspect to be investigated in the WICLEAN project (see summary in paper). Since we now have WT operation data available the goal was to assess the WT detection algorithm for the first time with this unique data source. For completeness, we now show RHOHV and ZDR in section 3.1.

Comment 2: As far as the quality is concerned, I find it good with one exception: the authors fail in introducing and describing the variables. All the variables should be described in an introductory part of Sec. 2, I think. Currently, NCP is found only "inside" the algorithm description (step 1). CR is "hidden" inside step 2. CCOR is not even described! (or am I missing it?)

What is CCOR? Is it the estimate of the Log-power of the non-moving clutter that you are going to remove from the Uncorrected reflectivity factor? Is it intrinsically negative, because you ADD it?

Adding up, I recommend to describe ALL the variables at the beginning of Sec. 2.1.

Response:

For the reading flow, we think it is better to introduce the variables when they are used for the first time. You are correct with CCOR. Thanks for pointing this out! We have added an explanatory section when CCOR is mentioned the first time.

Comment 3: Since the WT polarimetric signatures are as interesting as the spectral ones, I would invite the authors to show the statistics of rhoHV and Zdr, explicitly; not hidden in a combined form inside the Melnikov-Matrasov DR for STAR weather radars.

Response:

We add RHOHV abd ZDR panels in the section. Note that DR is more and more used in weather radar networks. DR is aktually computed in the signal processor from eq. 5 in the cited Rhyzhkov paper. It is NOT computed from ZDR and RHOHV (output variables on processed rangebin level). At DWD DR (rather UDR) is used to better discriminate between non-meteorological and meteorological echoes. the "U"DR denotes a processing stream in the signal processor, where no clutter filter is applied.

Comment 4: There is also another IMPORTANT change to be implemented: I ask the authors to please **separate (split)** the counts when rotor speed is exactly 0 from those when 0<rs<0.5 rpm. This choice could improve the information content of Figures 4, 5 and 6. If the sample size of the observation period were large enough (something that is hard to evaluate a priori), then a peak would appear at small rotor speed, where the variability of TH (and TV) is the largest (much larger than at the energy production rotor speed). However, in order to foresee whether such variability could emerge in the DWD weather radars, it is necessary to know some fundamental info regarding the scan program such as:

- The antenna rotation speed in degree per second of the sweeps with different angles of elevation.
- The corresponding PRF of such sweeps

I ask the authors to add such info in a technical subsection, which gives also info on the radar sites (lat, lon, altitude of the antenna feed, …)

Furthermore, I think it would be interesting to compare observations with rotor speed larger than, say 0.5 or 1 m/s, with the case of not moving (or very slowly moving) WT.

Response:

We have looked into the suggestion to make a finer resolution for the 2-D histogram within 0-1 rpm. With the existing sample size and setup no additional insight could be found.

Note: A rotor speed of exactly 0 rpm (over the "measurement interval" of the wind turbine) is a very rare case (e.g. for maintenance), as this means that the wind turbine is being actively braked. Commonly (to our knowledge), wind turbines are tumbling slowly when not producing energy (e.g. the rotor blades not in the wind).

Information on PRF, antenna rotation speed and pulse length are now included in the text.

Comment 5: As far as Sec. 4 is concerned, I appreciate the efforts to give an evaluation of beam shielding; on the other hand, I find it somehow preliminary, indicative; we are still far from conclusive results. Consequently, I would make it more concise.

Response:

We certainly consider the data shown here as examples (we say so explicitly in the text). But those examples highlight the requirement, to keep WT out of the 5 km range. The question is: What would be a "conclusive" result? Based on observations like this, there will be always large variability (as indicated by the histograms we show).

Comment 6: Finally, I am well aware that this report represents just a case study; however, something more statistically robust would be nice, like quantifying over longer time periods and/or over a larger region the extent of the effect on radar observables as a function of rotation speed and distance (AZ, ELEV) to the WT.

Response:

This will be addressed in more depth using data from the WICLEAN project. Furthermore, as indicated in the paper, we are just receiving WT operation data since spring/summer 2025. We will revisit this analysis with a much larger data set (ideally when having a multi year data set)

RC5 - DETAILED SUGGESTIONS/COMMENTS

Figures 4, 5, 6: please, add another column in the raster figure in order to **separate (split)** the counts when rotor speed is exactly 0 from those when rs > 0. In this new additional column just put only the count when rs is exactly zero (perfectly still WT). Maybe, at low rotor speed, you may have BINS with different resolution, e.g., 0<rs<0.2 rpm, then 0.2<rs<0.5 rpm, …

Response:

We have looked into this, see response on comment 4 above. We were not able to obtain conclusive results.

Lines 75-89: I appreciate very much this introductory part which explains the reasons behind the spectral WT detection algorithm developed by DWD by evaluating in real time IQ data. Most of these findings have been shown in the papers already cited in the introduction, among others. Maybe they can be cited again here together with others such as, by way of example,

Angulo, I., Grande, O., Jenn, D., Guerra, D., and de la Vega, D., 2015: Estimating reflectivity values from wind turbines for analyzing the potential impact on weather radar services, Atmos. Meas. Tech., 8, 2183–2193

Bredemeyer, J., Schubert, K., Werner, J., Schrader, T., and Mihalachi, M.: Comparison of principles for measuring the reflectivity values from wind turbines, 20th International Radar Symposium (IRS), 26–28 June 2019, Ulm, Germany, 1–10, https://doi.org/10.23919/IRS.2019.8768171, 2019

Response:

We cite now both papers. Thanks for pointing this out!

Lines 99: Instead of “It is obvious that weather situations with a large spectral width are not addressed here”, I think

“As a consequence, weather situations with a large spectral width cannot be addressed here” would much more appropriate.

Response:

Thank You very much. We have changed this accordingly.

Lines104-105. Sorry, I cannot possibly agree. With a ~ 150 m range resolution (coherent demodulation, hence ~c tau / 2), I would say it could be visible in 6-12 oversampled gates, or even more…

Response

We are not sure what the reviewer wants to point out here. We clearly say that oversampling is applied with the 25 m raw-rangebin resolution. We now say "five or even more successive range gates". Ok?

Line 107: “It is more likely a strong fixed target”, “which includes the case of a perfectly still WT”, I would add (or something similar).

Response:

Thank you for your comment. We have enhanced Line 93 accordingly (text position differs slightly from your original suggestion).

Lines 128-129: please, Keep it simple! Use in both line an HPBW of ~ 1 deg, do not use 0.9 deg in one line, and then 1 deg in the other … Consequently, in line 127, say the width is ~ 166 m, instead of 150 …

Response:

Thank you for your comment. We see what you mean. The half power beam width of the antenna is 0.9°. The calculation underlying figure 2 uses 1°. To resolve this we have deleted "(here a beam width of 1° was used)" and reproduced the figure with 0.9°.

Line 128: Please, write Half Power Beam Width (you may use the acronym) and NOT simply beam width, which is ambiguous.

Response:

We have changed "antenna beam width" on line 128 with "half power beam width of the antenna".

Page 6, Line 141-142:

The variables listed here (CR, NCP CCOR URHO UDR) should be introduced at the beginning of Sec. 2 , (I presume H and V are related to the polarization states, are not they.

Is URHO, the raw rhoHV, I mean not corrected for Signal-to-Noise ratio?

Is DR the Melnikov-Matrasov depolarization ratio? (36th AMS Conf. on Radar Meteorology and then Ryzhkov et al., JAMC 2017). I see you are introducing DR at page 12 (too late; as stated, the definition should appear ath the beginning of Sec. 2). And what is UDR? Does it mean using raw ZDR, I mean, using observed ZDR without any attempt to correct for attenuation? Or using URHO? Or what? In figure 10, bottom pictures you show UDR at ELEV 1.5 deg and 3.5 deg.

Response:

Thank You for Your comment. We enhanced the text accordingly (RC1 had already pointed this out to us.).

Minor:

I would avoid using the abbreviation “elevation” to mean “angle of elevation”. You may use “EL” or “ELEV” or another abbreviation that you may have declared at the beginning of the manuscript.

Response:

Thank you for your comment. We enhanced the text accordingly (changed "elevation" to "antenna elevation" throughout the text).