amt-2022-316.R2 Authors answers to Reviewer 1, comments dated 6.8.2023 (3rd Revision)

I had a lot of fun reading the improved version of the manuscript. I'm glad that a lot of time information was reduced by introducing named periods (P1 through P4). There are only few remarks and some technical issues left:

1) I'm missing a statement that the high stability of the measurements at the still turbine proves the high quality and low noise of the total system, including radar (transmitter, antenna, receiver) and wind turbine. Variations during WT motion are representing physical changes and are not caused by noise. One can rely on these measurements!
We have added it in the conclusion at line 467.

2) (Probably not for putting it into the manuscript): It looks like Z_h is more stable than Z_v for Z_v in P1. This is true in logarithmic scales. But when transforming back to linear reflectivity values, the range between 56,75 dBZ and 56,25 dBZ is a delta of 52000 mm^6/m^3 whereas the range from 38,5 dBZ to 41,5 dBZ is only 7000 mm^6/m^3. So, there is no prove that Z_v is varying stronger than Z_h.
Interesting viewpoint, thank you: I see you evaluating the variability in an "additive" way, I mean using a linear scale. Personally, I would evaluate it in a "multiplicative" way, which is using Log-transformed values. As you suggested, it is probably not worthwhile to discuss such different viewpoints inside the manuscript.

Technical points

l 73: Those constructive and destructive interference maxima occur also under conditions of a rotating WT rotor. But they are less visible as an average over a wider rotation angle range is determined.
We have rephrase the paragraph accordingly.

l 79: Neely et al is missing in the literature. Please, check if all cited literature is given in the bibliography and if all literature in the bibliography is cited in the text.
We went through the whole references and bibliography to check this issue. In the end we opted for leaving out the citation of Neely et al.

l 129: I do not see 7 to 11 rpm (only 7 to 8 rpm in Fig. 1).
Yes, the first half of the day (0-12 UTC) is not shown in Fig. 1, which covers only the second half of the UTC day (12-24 UTC); we have modified the text.

l 132: Please, indicate that 17:00 through 17:10 is P1 and can be found in Fig. 2. Otherwise one is astonished to see P2 quoted in l 144.
It is now indicated.

Figure 1: blue (instead of blu), typo corrected.

l 218: I know, I am petty, but: PsiDP, especially PhiDP are not a property "at any given range" but they are (normally) a property of the atmosphere between the radar and range r (plus backscattering phase plus Psi0). Could we say "PsiDP _of_ any given range"? I think "_at_" is wrong.
Well, none of us is native speaker. Since the dependence of the range, *r*, is not explicitly shown in eq. 3 [we simply write Ψ, not $\Psi(r)$], I have opted for removing "at any given range".

l 289: Fig. 5 (not 4) for ZDR, typo corrected.
l 300: Fig. 6 (capital F), typo corrected.

l 304: This is not the first relative minimum (but the third). Please, find a better formulation. We have deleted "first".

l 312: ZDR is only "expected" to be 0 dB for a rotating WT. As long as the WT is standing still both reflectivities (Z_h and Z_v) show random values and thus ZDR is "twice as random". 0 dB is the expected average and is achieved after averaging over sufficient wide rotation angles of the WT.
Yes, indeed: 0 dB could be expected only by averaging several rotations of the WT. Actually, as you pointed out, Zdr is "twice as random": hence, 0 dB is not at all expected! We have deleted "expected".

l 316: I probably would add: These data show that already comparable small changes of the state of the WT can introduce significant impact on the radar observables.
We have further elaborated your valuable suggestion; our proposal is: "these data show that even at zero rotor speed, other changes of the state of WT can have a large impact on the radar measurables".

Figure 6: Could you add behind "(from 17:13:31 to 17:14:51 UTC" , P2b)?
Yes, please! Thank you. Similarly, we have specified in the caption of fig. 7 that the 140 echoes shown (8.96 s) belong to the selected interval "P2a".

l 346: It is a few tens of seconds before 17:40 UTC (i.e. starting at 17:39:41), yes, and maybe we do not need to list the exact time when, for instance, rhoHV went to 1; hence, we have opted for "around 17:39:40.

l 354: larger fluctuations of the median, maximum, and minimum reflectivity, "median" added.

l 356: Shouldn't it be Around 17:39:40 UTC?
Exactly so, thank you; and it has helped me to find another "bug" (see line 365 below).

l 357 and 359: These bounding values for Z_h and Z_v are random values. There is no scientific reason behind them being larger or smaller than the median average achieved during WT operation mode.
Yes, you are right: we have added "randomly" (and put the less relevant part between brackets).

Figure2, line 292 and more: The stable period is happening from 17:39:40 on. This is the last 20 seconds before 17:40. This period is cited several times and its duration is as often called 20 s as 40 s. In l 292 it is even given as the period before 17:30. Please, give a unified time and duration for this event.
You are right: I apologize for this error, which has occurred more than once; it is me, MG, who should be blamed. It looks like if I had in mind 17:32 instead of 17:40, as well as "20 s past" instead of "20 s to" (see also you 4 additional helpful comments below)… I am really sorry about this; thank you for your patience.

l 365: not 40 s but 20 s (see the given times behind).
Corrected. We apologize, also the time was wrong: the last 20 s of our study period ends at 17:40, not at 17:32!! (see also, your precious comment at line 356) Thanks to you, we could find this wrong time reference.

l 383: not 40 s but 20 s (see l365), corrected.

l 391: 17:23 to 17:38 is only 5 minutes, not 6; typo corrected, thank you.

l 399: shouldn't it be: "due to the antenna stare mode of the x-band radar", yes, thank you for pointing it out.

As you already followed my wish to further investigate your data I want to formulate some more ideas:

1) For a reduced data set of only "energy production mode" (i.e. pitch angle 2°) the mean radar observables could be determined as a function of nacelle orientation. This is the only varied parameter as long as you measure in stare mode, as pitch angle (blade angle) is fixed and variations due to rotation of the blades are averaged out.

2) The opposite data set of non-rotating rotor can be evaluated for extreme values achieved and on the gradients that occur due to variations in nacelle and pitch angles. Under still standing rotor and radar antenna you sample the "directivity pattern" of the WT (i.e. the differential scattering cross section) with a very high resolution (although you cannot vary all possible parameters, i.e. the radar is at a fixed position). We could learn about the strongest values of the differential scattering cross section and the "beamwidths" of these strongest values. My assumption is, this cross section looks more like an irregular "moravian star" than like a smooth "potato".

Thank you very much for these two interesting ideas for future investigations.