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Comment on amt-2020-511

Jakob Mann (Referee)

Referee comment on "The COTUR project: remote sensing of offshore turbulence for wind energy application" by Etienne Cheynet et al., Atmos. Meas. Tech. Discuss.,
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This is a very well prepared manuscript, a very relevant subject, and a step forward in experimental investigation of the spatial structure of turbulence over the sea. The authors demonstrate a good knowledge of the field and they have devised a fascinating experiment that deserves wide acclaim.

There are some **main issues** with the paper.

It is a pity that the authors spend energy to present the size of the larger rotors of today (up to 164 m in diameter) but that the separations studied are not anywhere near those. In their example study on coherences the lateral separation is only 21 m. It is so small because they cannot use the data from the third lidar (lidarS) because it is misaligned by 7 degrees which they ingeniously estimate. This brings me to the more serious issue which is related to the first. The authors' main result is probably figure 16 where it is shown that the coherence decay coefficient C_x decreases with distance from the coast until it reaches a minimum between 1 and 1.5 km. The variation is very strong starting at a value of 10 and reaching a value of 2 at the minimum corresponding to larger coherence at this distance.

After the minimum the decay coefficient increases significantly again. So between 1 and 1.5 km the coherence is strongest.

However, the issue with the direction of lidarS raises the suspicion that lidarW and lidarN are neither well aligned. If lidarW and lidarN are misaligned (converging) with just 0.5 degrees, then the lateral separation distance d_y at 1000m would suddenly only be 10 m,

not 20 m. The corresponding decay coefficient C_y would be a factor of two too large. I am not convinced at all that lidarN and lidarW do not have this or even larger misalignment. An additional observation casts doubt on the results. Let's focus on the middle plot of figure 17. The coherence here is from a (nominal) separation of 21 m, so it is almost in the inertial subrange since the height above sea level is more than 100 m. Here the coherence is given by theory (Kristensen and Jensen, 1979), compare with Mann (1994) figure 8 top left which shows the squared coherence. At $k_1 * d_y = 0.4$ the squared coherence is 0.5 corresponding to a co-coherence of 0.7. In figure 17 (middle) this happens at a frequency $f = 0.1$ Hz. That corresponds to $k_1 = 2 \pi f / U = 0.045$ or $k_1 * d_y = 0.94$. The theory says (dotted line in the Mann figure) that the squared coherence should be around 0.1, so the co-coherence should be approximately 0.3. A good working hypothesis would be that the beams converge and that they reach a minimum distance at around 1200 m. This could explain figure 16.

In order for this paper to be published the authors have to account for the pointing uncertainty and its possible consequences. I hope they are able to do that because it is apart from this aspect a very nice paper.

Minor issues

p.1 l. 11 "undocumented" -> "hitherto undocumented" (but this conclusion is anyway dubious. See main issues above).

p.2 l.30 Mann (1994) was actually a study of lateral coherence in the marine atmospheric boundary layer.

p.5 l.100: "Bosh Rexroth" -> "Bosch Rexroth"

p.12 eq.1: You could clarify that x is in the direction of the mean wind vector (if it is).

p.13 eq.4: The Davenport model has no theoretical foundation. This ought to be mentioned.

p.13 l.266: "C_y is ab" -> "C_y is an"

p.13 l.277: In my opinion using "dependence" is better English than "dependency".

p.13 l.285: The phase delay has been studied by Chougule, Mann, Kelly, Sun, Lenschow and Patton (<https://doi.org/10.1080/14685248.2012.711524>) both theoretically, numerically and experimentally for vertical separations which are the most relevant. Those phases most likely do have consequences on load on wind turbines although nobody has studies this in detail.

p.14 eq.6+7: C_x is defined from the longitudinal coherence, i.e. with purely longitudinal separation. Is there any justification of the combination of C_x and C_y for an arbitrary separation in the horizontal plane, or this just a convenient interpolation formula?

p.15 l.310-323: You argue that when determining C_x the vertical separation due to slightly slanted beams (or horizontal separation due to beams slightly off mean wind direction) can be ignored. I am not so sure. Given that C_x is small (Taylor's hypothesis is not very wrong) then a lot of the apparent value of C_x could come from these vertical or horizontal separation.

p.16 l.338: Yes, that is certainly a good point.

p.16 sec.3.4: Many places you write R_{ib} where it should have been R_{ib} .

p.16 l.358: "Obukhov length"->"inverse Obukhov length"?

p.19 fig.11: It looks like, as you mention, that the masts are in some complex flow generated by the hilly terrain. Has it been completely ruled out that the sonic on the West mast is not mounted horizontally?

p.21 fig. 13: Just an idea: The vertical stripes are obviously not of atmospheric origin and should be removed. A procedure for that would be sec. 2.6 in Lange et al (2017) <https://iopscience.iop.org/article/10.1088/1748-9326/aa81db/meta>

p.21 fig.14 caption: "a which the" -> "at which the".

p.22 l.446: I can see that sampling volume affects turbulence intensity, but not that the sample rate should do.

p.22 p.453 "range-dependant" -> "range-dependent".