Dear Jonas Kazda,

We are pleased to inform you that the Associate Editor report for the following manuscript is now available:

Journal: WES
Title: Mitigating Impact of Spatial Variance of Turbulence in Wind Energy Applications
Author(s): Jonas Kazda and Jakob Mann
MS No.: wes-2019-10
MS Type: Research article
Iteration: Minor Revision

The Associate Editor has decided that minor revisions are necessary before the manuscript can be accepted. Please find the Associate Editor Report at https://editor.copernicus.org/WES/ms_records/wes-2019-10.

We kindly ask you to revise your manuscript accordingly and to upload the revised files, a point-by-point reply to the comments, and a marked-up manuscript version showing the changes made in your File Manager no later than 06 Dec 2019: https://editor.copernicus.org/WES/file_manager/wes-2019-10. Please find all information on manuscript submission under https://www.wind-energy-science.net/for_authors/submit_your_manuscript.html.

Your revised manuscript will be reviewed by the Associate Editor and you will be informed about the outcome by separate email.

Besides adjustments requested by the Associate Editor or Referees, please check your manuscript carefully for typos, missing co-authors and their affiliations, terminology, updates of data in tables, or updates of variables in equations. All these have to be clarified with the Associate Editor and therefore have to be included before you submit your revised manuscript. Should your manuscript be finally accepted it will not be possible to include such rather substantial changes anymore when your manuscript is in final production (proofreading).

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In case any questions arise, please contact me. Thank you very much for your cooperation.

Kind regards,

Natascha Töpfer
Copernicus Publications
Editorial Support
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on behalf of the WES Editorial Board

Authors' response:
We are very grateful for the associate editors report, and for the reviewers' comments and their time spent with the review. All comments are addressed and discussed below.

Anonymous Referee #2

The following are points the authors could consider.

Authors' response:

We kindly thank the reviewer for the time he / she spent with the review of the manuscript. Please find our answers to your comments below.

Page 2, line 18. "There are", rather than "There is". Line 25. "performance will be addressed". Page 3, line 3. "variation of spatially separated sensors". Line 5. "between sensor and reference" or "between the sensor and the reference"

Authors' response:

The phrases were updated as suggested by the reviewer.

Page 6, line 15. "The remaining ... remained infinite." What *does* this mean? And 'infinite' – is this something to worry about?

Authors' response:

The statement was updated to the following.

"The integration range of the other integrals of the analytical solution remained infinite."

The value of the integral with infinite integration range is well defined for the investigated integrand in the present case.

Page 7. Would a physical argument also help? "Physically, as the points move sufficiently far apart the variance must cease to change, simply because the turbulence is no-longer correlated. It is the fact that the turbulence is correlated (over a distance less than order the integral length scale) that the variance will be reduced below an asymptotic level, necessarily falling to zero at zero separation. The form of Fig 1 is therefore as expected." Qualitative physical arguments are worth having.

Authors' response:

We have extended the physical argument along the comment of the reviewer as follows.

"At large separation distances, typically in the order of the integral length scale l, the second-order moment of wind speed at the points a and b becomes uncorrelated."

Also, could a boundary layer scale – e.g. an overall height – be given in association with Fig 1 and Table 1?

Authors' response:

The boundary layer height does not directly enter into the Mann model, only indirectly through the length scale parameter. The length scale parameter is taken from the paper by Sathe et al (2013) and are believed to be representative of the neutral offshore boundary layer.

And it would still be interesting to know how the asymptotic levels change with T. Could some example be given? – of halving or doubling?

Authors' response:

The impact of the measurement duration T on the asymptotic level of the normalized spatial variance of the second-order moment of wind speed can be understood from Eq. 14. As such, for instance, halving the duration T will result in an increase of the asymptotic level by the square root of two.

Page 8, line 27. "Thereby the local realizations of turbulent structures can be taken into account." Is this really the case? There is not much structural information, is there?

Authors' response:

The statement was rephrased to the following.

"Thereby the local realization of turbulent flow can be taken into account."

Page 9, line 1. Suggest "between turbines arises from the spatial". Line 2. ", an investigation was made on the".

Authors' response:

The phrases were updated as suggested by the reviewer.

Case of Fig 2. Presume ABL conditions of Fig 1, Table 1 are typical of those relevant here. Should this be said? (Some overall ABL height would be useful.)

Authors' response:

The ABL conditions in the Lillgrund study are classified implicitly using turbulence intensity, as shown in Figure 2 and 3. The impact of ABL conditions on the comparison of the observations from the Lillgrund study with the analytical solution from Figure 1 is discussed on page 10, line 12 to page 11 line 27.

Page 10+11. What level of turbulence intensity is taken to be indicative of neutral conditions? 7%?

Authors' response:

Based on the observations in Figure 3, the measured turbulence intensity of 7% to 8% could indicate neutral conditions, as discussed on page 10 line 13 ff. Given that the turbulence intensity is measured at the turbines the ambient turbulence intensity is likely to be lower at neutral conditions.

Page 11. Line 22 and line 24. 'more unstable conditions' Repetition. This needs tidying up.

Authors' response:

The phrase was adjusted to circumvent word repetition. The statement now reads as follows.

> *"First, according to Sathe et al. (2013) the integral length scale is larger in unstable conditions than in stable ones, and based on Eq. 14 this results in a larger value of the asymptote."*

Fig 4, right hand fig. Could not the misalignment angle be indicated by a circular arc, and so look nicer?

Authors' response:

We believe the design of Figure 4 is clear and aesthetic.

Figs 3 and 5. Should not the vertical axis have same label as Fig 1?

Authors' response:

We thank the reviewer for the comment. We are convinced that the y-axis label of Figure 3 is sufficiently clear given the definitions provided in the paper. To increase clarity of Figure 5, the y-axis label was updated as suggested by the reviewer.

Page 16, line 14. I don't understand this paragraph.

Authors' response:

The explanation to page 16 line 14 is given in the subsequent paragraph.

Page 14. Paragraph from line 8. Should not wind direction appear explicitly in the list?

Authors' response:

Wind direction becomes relevant in case turbine performance is wind direction dependent due to, for example, topographical effects. The following statement was added to the discussion to clarify this.

> *"In case turbine performance is wind direction-dependent, due to, for instance, topographical effects, the classification also needs to be performed with respect to wind direction."*

Anonymous Referee #3

Very interesting paper, which I recommend for publication after improving some minor aspects.

Authors' response:

We would very much like to thank the reviewer for the time he / she spent with the review of the manuscript. Please find our answers to your comments below.

A citation for further information on Lillgrund wind farm should be provided.

Authors' response:

A selected citation was added to Section 3.2.1 on page 9 line 4 to provide further information on Lillgrund wind farm.

Fig 3 (and 5?) would be nice to write for the y-Axsis that this is again \delta M(…)

Authors' response:

We thank the reviewer for the comment. We are convinced that the y-axis label of Figure 3 is sufficiently clear given the definitions provided in the paper. To increase clarity of Figure 5, the y-axis label was updated as suggested by the reviewer.

There is a principal aspect of presentation which should be improved: The notation introduced in Chapter 2 should be taken as basis of the whole paper. Later on there are several quantities (related to the quantities of Chart 2.) which are not well introduced - mainly clear definitions are missing.

** for RMS of Fig 5 (and corresponding text)

** for TI , \rho and \sigma close to equ. 17

** The Pearson correlation - was already nearly used in equation 4.

Authors' response:

We thank the reviewer for the comment. The discussion of Figure 5 has been extended to provide an improved and more coherent understanding to the reader. Along with Eq. 17, the following definition was added on turbulence intensity TI and σ.

"… where turbulence intensity $TI_{L,\boldsymbol{a}}(T)$ at a point $\boldsymbol{a}$ is defined as the ratio of the standard deviation of wind speed $\sigma_{L,\boldsymbol{a}}(T)$ to the mean wind speed u_L."

$\rho_{TI,\boldsymbol{a},\boldsymbol{b}}$ is the Pearson correlation coefficient defined above Eq. 17 in line 2 on page 15.

The Pearson correlation coefficient in Eq. 17 and the spatial variance of the second-order moment in Eq. 4 are similar, nonetheless different. This can be seen from the derivations of Eq. 17 – 20, which are required to relate these two quantities.

The results and presentations of Chapter 3.2.3 and Chapter 2 should be revised in the way that a common notation is used.

Authors' response:

The notations in Chapter 2 and Chapter 3.2.3 are coherent, as quantities are expressed using the same notation. This can be observed, for instance, in Eq. 18.

A simple explanation of the limits of \detla M for totally correlated and totally uncorrelated should be given. (Here I run in some problems with the results, if the signals at a und b are uncorrelated I expect that \delta M become sqrt{2} (see equ. 10) How can you explain the saturation of Fig 1 for 35%??.) I suggest that the authors discuss this in combination with equations. 21. also \rho should be related to \delta M.

Authors' response:

The asymptotic value of the normalized spatial variance $\delta M_{2,L,\infty}$ depends on the atmospheric conditions. As can be seen from Eq. 13, $\delta M_{2,L,\infty}$ does usually not yield the square root of two, since the standard deviation of the second-order moment $\sigma(\mu_{2,L}(T))$ typically differs from the expected second-order moment of wind speed $\langle\mu_{2,L}(T)\rangle$.

Are equations 14 and 15 is valid for \tau <= T and const for tau> T? in these equations I expected sort (2) and not 2

Authors' response:

The format of Eq. 14 and 15 is correct and is valid for $\tau << T$. The latter was added to the manuscript as follows.

"This approximation is valid for $\tau << T$ and $L << l$."

My suggestion to revise the paper with respect to the used symbols and formulas. Bring all in a consistent way. I have the feeling this can be done best in Chap2 (or you may use an appendix for the formal relations)

Authors' response:

As suggested by the reviewer, the use of symbols and formulas was align within the paper.