

Interactive comment on “Mitigating Impact of Spatial Variance of Turbulence in Wind Energy Applications” by Jonas Kazda and Jakob Mann

Anonymous Referee #1

Received and published: 24 April 2019

The reviewed manuscript by Jonas Kazda and Jakob Mann presents, as the authors emphasize, the first analytical solution for the quantification of the spatial variance of the second-order moment of correlated wind speeds – which is a very valuable, and well presented, contribution. The authors further introduce three examples of applications and describe for these how the analytically quantified impact can be mitigated. This second part is in my view rather weak at the current stage and should be further elaborated. Apart from that the three individual examples are treated in different detail, the mitigation approaches are not very convincing from a practical view. This point of criticism will be further detailed in the following comments.

Specific comments (following the order of the manuscript):

- Paragraph starting page 2, line 24 / general comment to application “2” – In power
C1

curve testing according to the current standards, the turbulence influence is rather considered a mathematical issue. The actual influence of turbulence on the performance is not yet accounted for. So, I am not sure how relevant this example is here.

- Paragraph starting p. 3 l. 1 – For application “3” it is not clear to me what kind of “sensors” are referred to. Maybe the example is presented in a too general and sufficiently clear way here.

- p. 6 l. 16 – The authors state that Figure 1 shows an “overall agreement”. This conclusion should be justified in a better way. Is the agreement really good enough? (what is the reference level?)

- p. 11 / conclusion of 3.2.1 – As I understand the description, the analytical model is here only used to explain the observed variations, making a location-specific TI measurement necessary. Is this really a mitigation? I am wondering if the model could not also be used for a correction. (This would be my understanding of a more practical mitigation. . .)

- 3.2.3 is really short with not a lot of content. The authors should either further elaborate on this section or possibly leave it out. I could imagine that the approach could here be used for a detailed uncertainty assessment, again as a more practical outcome. Technical corrections: (Only a selection here, a more detailed language review is recommended.)

- Page 1, line 1 – I would suggest a rewording of the first words. . . something like “For the first time. . .”

- p. 2 l. 30 – I believe what the authors mean is the uncertainty of the power curve, and not the power itself.

- p. 3 l. 11 (and several other places in the document) – Comma signs in “expected, spatial” and “two, spatially-” are unnecessary.

- (6) and p. 4 l. 10 – The equation needs to be revised. There is neither a vector r nor

a delta t.

- Figure 1 – I assume the y-axis corresponds to ΔM – would be a more handy label.

Interactive comment on Wind Energ. Sci. Discuss., <https://doi.org/10.5194/wes-2019-10>, 2019.