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Interactive comment on "Comparing Abnormalities in Onshore and Offshore Vertical Wind Profiles" *by* Mathias Møller et al.

Mathias Møller et al.

mathias.moller1@gmail.com

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We thank Anonymous Referee 1 for the time and comments. In the following, we will do our best to engage the comments and propose improvements for the final manuscript.

Anonymous Referee #1 general comments: The proposed article discusses abnormalities in the vertical wind speed profile from several years old observations at six sites in Northern Europe. Abnormalities are detected by local maximum in the vertical wind speed profile that cannot occur in MOST. The number and height of observed abnormalities are statistically correlated to the mean wind speed to thermal stability. A comparison onshore / offshore is made. Conclusions are a frequent of appearance of theses abnormalities from 65% offshore to 40% of the time onshore. The article is

well in the scope of WES and presents interesting original results. It has an extensive and well organized literature study. The main issue is that conclusions are very much weakened by having, in my opinion, only one onshore site (that has a very "extreme" roughness case) and by the missing analysis of the intensity of the maximum detected: in the present work, the most tiny deviation from MOST, that will have no effect on wind turbine operation, is accounted at an equivalent level as a large fluctuation that will certainly affect significantly the power production/loading. . . The manuscript is a little long, the writing should be more concise

The authors' reply to Reviewer 1 general comment: We gladly find the General comments positive. The main issues, the sites availability, possible extension of analysis into deviation intensity analysis are in details answered in details below. We agree that the text should be more concise and will be shortened it in the corrected version.

Anonymous Referee #1 major remarks: Anonymous Referee major remark 1: MOST 1.1 At several locations in the manuscript, you mention the hypothesis needed for applying MOST without really introducing them. Page 2 lines 9-10: Insist on the hypothesis used to build MOST (explicitly mention the surface layer) and give an approximate value of the region where MOST is valid. 1.2 When discussing abnormalities compared to MOST, do you consider cases where MOST is applicable (all MOST hypothesis are fulfilled) but observations differ from theory or cases where MOST cannot be applied? In the first case the theory is threatened and in the second not as you analyze cases where MOST cannot be applied. This is fundamentally different, please comment on that. Ultimately, it doesn't affect the interest of the work in studying VWP.

The authors' reply to Anonymous Referee major remark 1: MOST 1.1 The authors appreciate the comment, and agree that the assumptions of MOST should be outlined more clearly. The following is suggested to be added after lines 9-10 on page 2: "The logarithmic law describes the vertical development of velocity in the surface layer which is typically only the shallowest 10% of the atmospheric boundary layer. The depth of the surface layer where MO-theory is valid varies with the state of the atmosphere, from only a few meters during very stable stratification to several hundred meters during conditions of vigorous turbulent mixing. ". 1.2 The authors agree that this point should be clarified, and suggest an edit of the last sentence of the introduction to: "Based on this the applicability of the commonly used vertical wind profiles may be evaluated, and the need for more accurate vertical wind profile descriptions can be discussed. It is emphasized that the scope of the paper focuses on the applicability of the theory commonly employed today, and not the validity of the theory itself"

Anonymous Referee major remark 2: Methodology In the method of determining local maximum, the number of maximum and their height are recorded. However, (at least up to section 4.4.2), the smallest local maximum has the same importance as a very large deviation. The former would have an imperceptible impact on wind turbine performance/production/loads but the latter, would possibly have a large one. You mention at the end, section 4.4.3, that most of the maximum found are in unstable conditions where local maximum are very weak, that means with very limited effect for wind turbines. Would a methodology accounting for the intensity of the peaks (maybe eliminating the smallest peaks?) lead to the same results and conclusions? Ultimately, what is the effect on your conclusion of 65%- 75% (p.28 l.20) of profiles inflected? Among this ratio, how many maximums are really affecting WT operation?

The authors' reply to Anonymous Referee major remark 2: Methodology The authors certainly agree that such an analysis is an interesting pursuit. An initial analysis of this issue is given in section 4.4.2, and the percentages of profiles "severely affected" are given in the conclusion. The authors have concluded to perform an additional analysis where the inflections are grouped in bins according to their severity, which will be added as a separate subsection. We do however feel that enforcing a severity threshold for the entire analysis limits the wider scope and relevance of the work which in its current form is not only limited to wind turbine engineering. We do agree that a focus on inflection severity is highly relevant for wind engineering and highly encourage this issue to be the focus of future work. For this reason we have, as mentioned, decided to

have a stronger focus on this issue in the present manuscript through adding another subsection to discuss the topic more in depth.

Anonymous Referee major remark 3: Onshore/Coastal/offshore sites 3.1 The article claims to analyze both onshore and offshore sites. In reality, according to me, mainly offshore sites are discussed as only one site is really onshore (Ryningsnas). Høvsøre, Skipheia and Valsneset are clearly much coastal as observed in fig 13. Ryningsnas is in a forest, witch is a bit "extreme" in terms of surface roughness. The discussion on the effect of surface roughness on abnormal events would really be improved by the analysis of several "really" onshore sites with more moderate roughness.

3.2 What is the sense of analyzing coastal sites as a whole? It may make more sense to divide coastal sites in function of the wind direction, a offshore fetch and an onshore fetch? This is partially confirmed by fig13. 3.3 Can you make appear the offshore/coastal/onshore classification in one of the tables detailing the sites?

The authors' reply to Anonymous Referee major remark 3: Onshore/Coastal/Offshore sites 3.1 The authors agree that the discussion may have been raised by having more traditional onshore sites. The data sites were however limited to the sites where we had available data. We are proud to have gathered and analyzed a large amount of data from several sites and we are grateful for access granted to the data. We do however agree with Anonymous Referee 1 and regret not to have access to data from sites at a traditional onshore location, that would enhance the analysis.

3.2 Regarding the possibility of splitting the coastal sites into onshore and offshore sectors, this is the scope of another paper currently in press (https://iopscience.iop.org/issue/1742-6596/1356/1). This was not done in the current manuscript since the change in occurrence of inflections from onshore to coastal to offshore was the main scope. The authors believe the dependence of infection occurrence on site location was clearly portrayed without splitting the coastal sites into onshore/offshore sectors, it was therefore avoided.

Anonymous Referee major remark 4: Stability 4.1 Stability bins seems to be the same at all sites (p.14 l.1), is it realistic for both offshore and forest sites? See for example: Sanz-Rodrigo et al. Journal of Physics: Conference Series 625 (18 juin 2015): 012044. Dupont et al. Agricultural and Forest Meteorology 157 (15 mai 2012): 11âAËŸ S29 4.2 How much is the sensibility of the choice of the stability classes (p.27 l.4)? Can you give an order of magnitude.

The authors' reply to Anonymous Referee major remark 3: Onshore/Coastal/Offshore sites 4.1 The authors agree that the stability bins ideally should not be the same for onshore/offshore locations, as presented in suggested works, which investigate this issue in depth. However that would make the direct comparison between the sites impossible. Sanz-Rodrigo et al. (2015) states that his seven classes are 'somehow ambiguous' and in the conclusions of his work we can read that 'it is convenient to adopt certain conventions when it comes to measuring and defining stabilities". That is exactly what we did: for simplification and comparison ease we followed 5 bins classification, as we found it is used for offshore sites as well, for example in the following works:.

Barthelmie, R. J., Churchfield, M. J., Moriarty, P. J., Lundquist, J. K., Oxley, G. S.,Hahn, S., Pryor, S. C., 2015, "The role of atmospheric stability/turbulence on wakes at the Egmond aan Zee offshore wind farm," J. Phys. Conf. Ser.,625(1), p. 012002.

Barthelmie, R. J. (1999). The effects of atmospheric stability on coastal wind climates. Meteorological Applications, 6(1), 39-47.

Motta, M., Barthelmie, R. J., & Vølund, P. (2005). The influence of nonâĂŘlogarithmic wind speed profiles on potential power output at Danish offshore sites. Wind Energy: An International Journal for Progress and Applications in Wind Power Conversion Technology, 8(2), 219-236.

Alblas, L., Bierbooms, W., Veldkamp, D., 2014, "Power output of offshore windfarms in relation to atmospheric stability," J. Phys. Conf. Ser.555(1), p. 01200

4.2 In authors opinion, changing the stability bins classification between 5/7/9 bins will not change the main findings of the manuscript, however we are grateful for pointing this issue and we will possibly switch to more than 5 bins classification scheme in our future work. Anonymous Referee #1 minor remarks: Anonymous Referee minor remark 1: Are there other tools to detect abnormalities (deviations from MOST)?

The authors' reply to Anonymous Referee minor remark 1: Yes, there are many ways to detect deviations from MOST. As briefly mentioned in the Summary and conclusions, future studies may benefit from using several methods for identifying abnormalities. One method could be measuring the deviation from the vertical wind profile predicted by MOST (i.e deviation from a log-law profile) for each 10-minute profile. Anonymous referee minor remark 2: The mast speed up effect description (p.18 l.14-16: and p.25 l.2-8) is very important, it has to be in the site description part.

The authors' reply to Anonymous Referee minor remark 2: The authors agree that this is critical and will move this to the description in the revised manuscript.

Anonymous referee minor remark 3: In the description of the measurement equipment, more information is needed on the LiDAR data: time/space resolution, volume probed. . . . The LiDAR data may be affected r by longitudinal and vertical space-average that may smooth out small maximum (p18 l.15-16)? The authors' reply to Anonymous Referee minor remark 3: The authors agree that additional information regarding the LiDAR measurement should be given, this will be provided in the revised manuscript. To our knowledge no evidence of smoothing of maximum has been found.

Anonymous referee minor remark 4: p19, l29 → p20, l2: I don't understand this part. What do you mean by "spectrum of velocity"? Do you mean turbulent spectrum? Something else? Why don't you show them? Additionally, I don't understand what you get from these "spectrum". . .

The authors' reply to Anonymous Referee minor remark 4: By spectrum the authors simply meant within the range of observed velocities. This will be clarified in the revised manuscript.

Anonymous referee minor remark 5: Also mentioned p.28 l.29 p.22 l.16-17: “it was found to be due...” rather approximative statement. You need more proofs to say that. Better say you enlighten a correlation. . . The authors’ reply to Anonymous Referee minor remark 5: The authors agree and will change this in the revised manuscript.

Anonymous referee minor remark 6: p.22 l.19-21: A shallow surface layer is a possible explanation. Could it be estimated from sonic anemometer profile to verify your hypothesis?

The authors’ reply to Anonymous Referee minor remark 6: This could be a possible explanation. The sonic measurement data does not allow for this to be verified due to data availability issues, but it will be added as a possible explanation.

Anonymous Referee #1 technical corrections: The authors have found it most efficient to only comment on the technical corrections if we are not in agreement with the technical corrections. Otherwise, the suggested corrections listed below will be implemented in the revised manuscript.

Anonymous referee technical correction 1: Revise the use of abbreviations for Sec. Fig. Tab. . ..

Anonymous referee technical correction 2: Use Figure sub-numbering when more than 2 figure (a,b,c,d...)

Anonymous referee technical correction 3: p.2 l.21-22: unclear sentence

Anonymous referee technical correction 4: p.3 l.7: define IBL the first time you use it

Anonymous referee technical correction 5: p.3 l.11: “short-lived phenomena” → I guess you speak about space rather than time, reword to make it clearer.

Anonymous referee technical correction 6: p.4 l.18: why a new paragraph here?

Anonymous referee technical correction 7: p.4 l.19: this sentence is a bit “lost” here. . .

Anonymous referee technical correction 8: p.4 l.26-27: please rephrase Fig. 2 is cited much later in the text, please move it at the right location.

Anonymous referee technical correction 9: Tab. 1 and 2 can be merged in one. Remove all information not necessary for the present paper (was pressure used? Humidity?)

The authors’ reply to Anonymous Referee technical correction 9: Pressure and humidity were used in the stability calculations for calculating the potential virtual temperature.

Anonymous referee technical correction 10: Section 3.1, please move all references to the way you got the data to the acknowledgements.

Anonymous referee technical correction 11: p.14 l.14: define MABL

Anonymous referee technical correction 12: Fig 5: The sorting seems to be linked to Z0, a better choice of colors would make the reading easier. For example changing the bars filling as function of onshore/coastal/offshore.

Anonymous referee technical correction 13: p.15 l.8: “observed” may be better appropriated than “displayed”

Anonymous referee technical correction 14: p.18 l.3:5 and figure 6: why not plotting occurrences in a scatter plot (such as fig7 “middle”) that would help comparison. And all sites in the same plot.

Anonymous referee technical correction 15: p.16 l.16: double “that” to remove.

Anonymous referee technical correction 16: Fig 7: the central and right plots can be merged, one of your goals is to underline the difference onshore/offshore, potting in the same graph will enhance comparison.

The authors' reply to Anonymous Referee technical correction 16: This was done intentionally as gathering all lines in one plot created too many plotted lines resulting in an unclear plot. The range on the y-axis is however the same for both plots.

Anonymous referee technical correction 17: p.19 l.16-17: this sentence has already been said

Anonymous referee technical correction 18: p17 l18-20 p19 l23: change "These profiles..." by "The latter profiles..."

Anonymous referee technical correction 19: p.24 l.14: "Recalling that the reference wind speed at 100m increases..."

Anonymous referee technical correction 20: p.24 l.14: add a coma "...height, the increase"

Anonymous referee technical correction 21: p.28 l.12: remove "Through"

Anonymous referee technical correction 22: Fig12: please switch the two figure on the right to make the figure consistent.

Please also note the supplement to this comment:
https://www.wind-energ-sci-discuss.net/wes-2019-40/wes-2019-40-AC1-supplement.pdf

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