Wind Energ. Sci. Discuss.,
https://doi.org/10.5194/wes-2019-40-RC1, 2019

eawe european academy of wind energy
Open Access WIND ENERGY SCIENCE DISCUSSIONS

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

Anonymous Referee #1

Received and published: 19 August 2019

REVIEW:

Comparing Abnormalities in Onshore and Offshore Vertical Wind Profiles. WES-2019-40

The proposed article discusses abnormalities in the vertical wind speed profile from several years os observation 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.

Printer-friendly version

Discussion paper

General comments: 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

Major remarks:

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.

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?

3- Onshore/Coastal/onshore 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). Hovsore, 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 on of the tables detailing the sites?

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): 11âĂŚ29 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.

Minor remarks:

Are there other tools to detect abnormalities (deviations from MOST)?

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.

In the description of the measurement equipment, more information are needed on the LiDAR data: time/space resolution, volume probed. . . The LiDAR data may be affected

Printer-friendly version

Discussion paper

by longitudinal and vertical space-average that may smooth out small maximum (p18 l.15-16)?

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"... 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...

p.20 l.19-21: A shallow surface layer is a possible explanation. Could it be estimated from sonic anemometer profile to verify your hypothesis?

Technical corrections:

Revise the use of abbreviations for Sec. Fig. Tab.... Use Figure sub-numbering when more than 2 figure (a,b,c,d...)

p.2 l.21-22: unclear sentence

p.3 l.7: define IBL the first time you use it p.3 l.11: "short-lived phenomena" → I guess you speak about space rather than time, reword to make it clearer.

p.4 l.18: why a new paragraph here? p.4 l.19: this sentence is a bit "lost" here... p.4 l.26-27: please rephrase

Fig. 2 is cited much later in the text, please move it at the right location.

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

Section 3.1, please move all references to the way you got the data to the acknowledgements.

p.14 l.14: define MABL

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 on-shore/coastal/offshore.

p.15 l.8: “observed” may be better appropriated than “displayed”

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.

p.16 l.16: double “that” to remove.

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.

p.19 l.16-17: this sentence has already been said p17 l18-20

p19 l23: change “These profiles...” by “The latter profiles...”

p.24 l.14: “Recalling that the reference wind speed at 100m increases...” p.24 l.14: add a coma “...height, the increase”

p.28 l.12: remove “Through”

Fig12: please switch the two figure on the right to make the figure consistent.

Interactive comment on Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2019-40, 2019.

Printer-friendly version

Discussion paper