Wind Energ. Sci. Discuss.,
https://doi.org/10.5194/wes-2018-60-RC3, 2018

WIND ENERGY SCIENCE DISCUSSIONS

Interactive comment on "A Framework for Autonomous Wind Farms: Wind Direction Consensus" *by* Jennifer Annoni et al.

Anonymous Referee #3

Received and published: 23 November 2018

This approach is an interesting alternative to make use of the information from the neighbouring turbines to correct the wind direction (WD) signal from SCADA at the turbine locations. Although it promises valuable contribution to the often overlooked, messy WD data processing, it could benefit from a more thorough investigation to study the sensitivity of the developed consensus to some of the local inflow characteristics.

More detailed comments/questions are listed down below:

p.1 : line 9-10 (Abstract) – "Oftentimes, measurements made at an individual turbine are noisy and unreliable" is rather a blanket statement and it not clear if only the WD signal is referred to or not. It should be noted that SCADA does/might provide some crucial information, and used in many applications in WF operation, so a re-wording is suggested. # p.1 : line 11 (Abstract) – By taking the nearby turbines into account, the

WD signal at an individual turbine can be improved, however, it is equally important to not to lose the local information in certain applications. This remark will be repeated several times in these comments.

p.2 : line 3-4 – What is actually meant by the unnecessary yaw movements? This should be elaborated further... if it is the turbine responding "too fast" to the WD changes, then it can argued that a simple low-pass filter might be enough. However, in the field it is generally the opposite effect as the turbine is generally "too slow" to respond to the highly variant WD (due to its inertia) which is one of the factors that might affect the behaviour of the power curve for highly turbulent flows for example.

p.3 : line 1-2 – "...facilitate wake steering WFC..." and more - the benefit of having a reliable WD signal is important for any kind of wake modelling really, including other WFC scenarios, operations management and conditions monitoring.

p.3 : line 18-19 – Is the "proximity" to define the connected turbines estimated in a relative proximity manner, i.e. taking the (reference) incoming WD into account? Otherwise, especially for the investigated terrain, it might be a risk to connect the flow-wise uncorrelated turbines, e.g. highly different turbulence levels (hence very different variance in the local WD), etc.

p.4 : line 6-7 – It is very true that the turbines that are several kilometers apart would experience highly different local WDs, however, it could also be the case when the turbines are much nearer. It should be clarified how near is nearby (possibly depending on local flow conditions) and how much of the local characteristics are kept and how much of it is smeared among a larger area.

p.5 : line 13-14 – "...based on nearest 10 turbines" Is there any particular reason for the selection? As indicated in the previous comment, how much of the local information is intended to be kept in such a network should be indicated and the reasons should be argued. (e.g.Figure 2b shows some of the connected turbines are much further apart than the others in the same local network. It is hard to argue that the contribution

from those turbines should be included - especially with the same weight, as will be mentioned later).

p.6 : line 6-7 – "The objective function, fi(xi)..." sentence should be omitted as it is confusing compared to eqn. (2)

p.7 : line 2-3 – Why the weight is equal to 1? Especially given that the correlation between some of the turbines would be lower than others, simply due to distance and local terrain differences.

p.8 : line 24-25 – How is lambda and rho related to/different than the weight wjk? Their explicit definition and tuning procedure should be included.

Overall Section 3: – It should be discussed as to why this rather complex methodology is/should be selected over an educated but simple interpolation (maybe combined with a low pass filter if the noise is a serious concern).

page 9 : Figure – The met towers should be visible in Figure 2 to understand the validation cases

page 10 : line 4 – Is the time step shown in Figure 3 different from the 500h used during tuning the parameters lambda and rho (i.e. is it an 'independent' dataset?) It is hard to assess how much of a difference is to be expected between the neighboring turbines, given the terrain (applies also for Figure 4). In that sense, the visual comparison might be, at least partially, misleading.

page 10 : line 16-17 – The error sensitivity to wind speed (WS) might be due to the turbulence intensity (TI) behavior with respect to WS. Another color coding or, in general, an additional sensitivity analysis to TI at the sodar would tell a more detailed story.

page 11 : Figure 4 – Is it also a "snapshot" or an average over some period? Again, one could argue that in 'real-time' we would/might expect higher variance over a relatively big terrain.

page 11 : line 2 – Again, due to the turbine's inertia, we generally see the opposite effect in fact - turbine is having a hard time to catch up with (truly, not just noisy) variable WD sometimes. Therefore, it is generally expected the (non-intentional) misalignment to occur due to the bias in the sensors, rather than the noise.

page 11 : line 3-4 – An important factor in terms of defining the true error might be the difference between the wind vane measurements and the actual nacelle position (as mentioned very briefly later in the paper during the filtering of the data for the last validation case). In most of the turbines' SCADA, there exist a separate signal (than the wind vane measurements, generally called wind direction) which is called Nacelle position or Nacelle direction. Those two signals differ quite a bit, especially in high turbulence cases (turbine not following the highly variant WD as mentioned earlier here). It is important to clarify what is 'corrected' in this study is only the wind vane measurements which may differ from where the turbine is actually facing.

page 13 : Figure 6 – The effect of the changing TI levels between the two yaw error cases defined in Figure 6(a) should be clarified.

page 13 : line 3-6 – Again, that might be due to the difference between the WD measurements on top of the turbines vs. Nacelle position

page 13 : line 13 – Suggest rewording to "truth" : reference, baseline, true value

page 13 : line 13-14 – Not clear how and why the error estimates from the consensus are more reliable than the sodar measurements? Depending on the equipment itself surely, sodar has been shown to agree ver well with the met mast measurements on the site (e.g. Steven Lang and Eamon McKeogh (2011) LIDAR and SODAR Measurements of Wind Speed and Direction in Upland Terrain for Wind Energy Purposes, Remote Sensing)

page 14 : Figure 7 – The difference between the consensus and the wind vanes seem to increase with increasing "immediate wake" effects on top of the nacelle, again

pointing towards the (added) turbulence sensitivity. Overall, the effect of turbine not being able to follow the highly fluctuating WD, hence dynamic misalignment and underperformance, is more of a physical phenomenon due to turbine inertia. Correcting the signal would not necessarily solve that problem.

page 15 : line 7-9 – 'Smearing' the small timescales in local WD might be useful for many other analysis, however, if the turbine avoids yawing as a result, we might risk to lose power still. Correcting the bias in the signals on the other hand, is a much more useful outcome of such an approach.

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