**WIND ENERGY SCIENCE DISCUSSIONS**

**WESD**

Interactive comment

# *Interactive comment on* "Wake steering optimization under uncertainty" *by* Julian Quick et al.

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## 1   General comments

### 1.1   Summary of key points

The paper is well written and its topic is relevant. The authors give a good introduction, providing context to problem, and also provide a good overview of literature references.

The authors extend previous work on optimization under uncertainty of wake steering offset angles in a wind farm context. Beyond the already investigated effect of uncertainty in yaw angles, wind inflow direction, they additionally include uncertainty of wind

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speed, turbulence intensity, and shear.

The general setup as an optimization-under-uncertainty problem is sensible. The uncertainties considered essentially translate to changing the 'deterministic' wake model into a spread-out 'stochastic' wake model that is obtained by averaging the deterministic one over the probability distributions describing the uncertainties. Their analysis is well structured, dealing first with an exploratory 2-turbine case and then with a more realistic 11-turbine case. The conclusions are (generally, but not always) well supported.

However, I think there is a problem with the treatment of turbulence intensity (TI). The authors treat TI as if it is unrelated to wind speed statistics, whereas they completely determine it. I will discuss this in more detail below, but as a consequence I think the authors will have to remove the entire current treatment of TI and redo their analysis with correct TI values (derived from wind speed statistics).

Furthermore, I think in the uncertainty estimation and effect threshold choice, there is a lack of justification. Again, I will provide more details below. Either these things should be much better justified or the analysis should be redone with values that you can justify.

## 1.2 Overview of review aspects

My judgments here are based on my current understanding of the work.

1. *Does the paper address relevant scientific questions within the scope of WES?*
   **Yes.** Wake steering is relevant for wind farm control. Taking account of the impact of uncertainty makes it possible to define wake steering strategies that are robust against variations captured by the assumed uncertainties.

2. *Does the paper present novel concepts, ideas, tools, or data?*
   **Yes.** It appears to me that the combination of optimization problem definition,

data used, and set of uncertain variables is novel. Each individually is not.

3. *Is the paper of broad international interest?*
   **Yes.** It is in principle relevant for any wind farm control strategy designer.

4. *Are clear objectives and/or hypotheses put forward?*
   **Not really.** P2L22–29 describes what is going to be done in the paper. This can be reformulated to make it clear what the objectives are.

5. *Are the scientific methods valid and clear outlined to be reproduced?*
   **Yes.** The paper contains the necessary information for the study to be reproduced. However, data files for the wind resources and farms used should additionally be supplied.

6. *Are analyses and assumptions valid?*
   **Not all.** Assumptions about TI are incorrect.

7. *Are the presented results sufficient to support the interpretations and associated discussion?*
   **Almost all.** There is a strange claim about 4% improvement in the conclusions, but that may be a typo.

8. *Is the discussion relevant and backed up?*
   **Mostly, but not always.** There is a lack of justification for the uncertainty estimation and effect threshold choice. A few statements need to be better justified, reformulated, or plain removed (see annotated pdf). The discussion should be made more relevant by placing the paper's results in the context of existing results in the literature (I think there are some that are quite similar).

9. *Are accurate conclusions reached based on the presented results and discussion?*
   **Yes.**

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10. *Do the authors give proper credit to related and relevant work and clearly indicate their own original contribution?*
    **Mostly.** Clearly indicating what their own contribution is could be done more explicitly.

11. *Does the title clearly reflect the contents of the paper and is it informative?*
    **Mostly.** The title is apt, but quite generic. There is quite some activity in this area and it could be helpful to make the title a bit more specific. (I have no concrete suggestion.)

12. *Does the abstract provide a concise and complete summary, including quantitative results?*
    **Yes.**

13. *Is the overall presentation well structured?*
    **Yes.**

14. *Is the paper written concisely and to the point?*
    **Yes.**

15. *Is the language fluent, precise, and grammatically correct?*
    **Yes.** The English is excellent.

16. *Are the figures and tables useful and all necessary?*
    **Yes.**

17. *Are mathematical formulae, symbols, abbreviations, and units correctly defined and used according to the author guidelines?*
    **Almost all, yes.** $\Sigma$ should be slanted, because it is a variable.

18. *Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?*

**A few.** Some clarifications may be useful; these are pointed out in the annotated pdf.

19. *Are the number and quality of references appropriate?*
    **Yes.**

20. Is the amount and quality of supplementary material appropriate and of added value?
    **Could be better.** Data for wind resources and farm layouts should be made available and also (other input) and output files could be made available.


## 2 Specific comments

### 2.1 Treatment of turbulence intensity

Turbulence intensity for a given time interval is defined as the ratio of the wind speed standard deviation over the average wind speed for that time interval: $\mu_{\text{TI}} = \sigma_{u_\infty}/\mu_{u_\infty}$ in the notation of the paper. The time interval used can be chosen, but is typically 10 minutes. Whatever the time interval, the definition in terms of wind speed remains.

In the paper, turbulence intensity is introduced as a variable of its own right and even treated as a random variable independent from wind speed. This is incorrect. Turbulence intensity values should be obtained from wind speed statistic values as presented above. I think it makes no sense to treat it separately. So effectively, I think TI should be removed from the list of variables for which uncertainty is assumed and the analysis should be redone with TI values derived from wind speed statistics.

From the estimation of the TI uncertainty you present, P6L15–18, a time variation interpretation of the uncertainty is suggested. However, from the discussion of $f_{10}$ P4L8–19, I did not get the impression that this was the interpretation you wanted to

use. In any case, the FLORIS wake model is quasi-static, which implies that there is some implicit shortest time scale for which it can be applied, as any changes in the parameters that define it would 'in reality' corresponds to a smooth transition between resulting wake profiles. I assumed that this time scale was 10 minutes, but this would be incompatible with the way in which you estimate TI. But even if your assumed time scale is indeed shorter, then still you cannot assign a value to TI independently of the value of $u_\infty$.

## 2.2 Uncertainty estimation and effect threshold

In your uncertainty estimates, there are some strange decisions. Namely, your chosen $\nu_y$ is a bit smaller than the measured one and your wind speed and direction uncertainty are larger than warranted by your sources.

Furthermore, there is absolutely no justification given for the shear parameter uncertainty, but still you call it a worst-case uncertainty. The impression is that you wanted to also include shear in your uncertainty analysis, but did not find any good value for the uncertainty. This should be better justified, or shear should be removed from the uncertainty analysis.

Your choice of threshold for calling the effect of an uncertainty significant (P9L1) is unjustified, currently. This, in combination with the strange decisions may have influenced the picture sketched in Table 4 and P10L4–6. Namely, it might well be possible that if you hadn't made the strange choices, $y$ rather than $u_\infty$ would have been considered significant.

This whole treatment needs to be either much better justified or redone with values that you can justify.

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## 3 Other comments

I provide various (other) comments in the annotated pdf of the paper provided in attachment. While not as important as the comments I have worked out in this report, they should also be considered seriously by the authors.

Please also note the supplement to this comment:
https://www.wind-energ-sci-discuss.net/wes-2019-72/wes-2019-72-RC2-supplement.pdf

Interactive comment on Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2019-72, 2019.