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Comment on wes-2022-80

Anonymous Referee #2

Referee comment on "Optimization of wind farm operation with a noise constraint" by Camilla Marie Nyborg et al., Wind Energ. Sci. Discuss., https://doi.org/10.5194/wes-2022-80-RC2, 2022

General comments

In this paper, the authors propose a method to optimize the power production of a wind farm while setting noise constraints at specific receivers by changing the operational modes of the individual wind turbines. Two sound propagation models are considered: one simple engineering model (ISO 9613-2) and one physical model based on the parabolic approximation (WindStar). The optimization takes into account the effect of the wind turbine wake with a simple analytical model. The approach is tested on various configurations and quite convincing results are obtained. One interesting result is obtained in the specific case where the wind speed direction is aligned with a row of 7 wind turbines. In this case the optimal power production is not achieved when all wind turbines are in the least noise reducing mode (m=0). This result was surprising (to me), and could be more highlighted by the author if they believe it can be exploited in practical operations.

Some of the choices made by the authors are questionable (source directivity neglected, only 1 frequency per octave band in the propagation calculation, quite rigid ground chosen, ...). The choices are not clearly justified, and it is difficult to assess their impact on the optimization results.

In summary, the approach seems original and of practical use to adapt the curtailment plan to a given wind farm and to specific atmospheric conditions. However the assumptions used and the choice of the input parameters deserve to be better justified and discussed. Thus, I recommend that the authors revise their work according to the major corrections detailed below.

Specific comments

1. Propagation model assumptions and implementation

- Directivity: you assume line 80 that the wind turbine has a uniform directivity with Dc = 0. This is quite surprising because the wind turbine has a marked horizontal directivity, with a difference of typically 6dB between downwind and crosswind directions (see Oerlemans and Schepers 2009 for instance). Why don't you account for it?

- Turbulence influence: you explain lines 136-141 that you neglect the scattering effect due to turbulence. But turbulence has also a strong effect on the sound power level. As shown by Buck and Oerlemans (AIAA J. 2018), the sound power spectrum can vary by at least 5dB at low frequencies depending on the atmospheric turbulence level. As you consider the source strength of wind turbines a given by the manufacturer, this can be a limitation. Could you comment on this issue?

- Number of frequency calculations: you consider only one frequency per octave band in your calculations. This is sufficient for the sound power spectra because it is broadband, but it is not sufficient for propagation effects because of ground interference phenomena. Indeed the octave band center frequency can as well be at the interference minimum or maximum which completely changes the result. Usually several frequencies per octave band (or 1/3 octave band) are considered to correctly calculate the relative SPL spectrum to avoid this problem.

- I don't understand your definition of the SPL in Equations (4) and (5): you need to replace p_free by p_ref = 20 microPa if you want to obtain SPL, or you need to add a Delta if you want to define the SPL relative to free field! Also, if this is a ratio of squared amplitudes there is no factor of 2. See for instance Equation (3.6) of Salomon's book Computational Atmospheric Acoustics.

- Source modeling: you write line 128-129 "the use of the 3 distributed point sources has previously shown good comparison with field measurements (Nyborg et al. , 2022)." In this conference paper the comparison is not that convincing, with many 1/3 octave bands in Figures 5, 8 and 10 with more than 5dB difference between WindStar and measurements. To validate this approach, it would be interesting to compare the results with the WindStar model in Cao et al. (2022) including 36 point sources. Have you done it?

- You explain Lines 110-111 that there are numerical issues at 4 and 8kHz with the WindStar model: could you explain why? There is no reason why the parabolic equation should not work at these frequencies!

2. Input and output parameters

- Ground model and parameters: you use in WindStar the ground impedance model of Attenborough (1985) that has 4 parameters: flow resistivity (sigma), porosity, tortuosity and grain shape factor. Thus it is not sufficient to give the values of sigma. Also you consider in Section 4.3 a value of 2x10^4 kPa.s.m−2. This seems quite high, usually the value of flow resistivity for grass ground is below 1000 kPa.s.m−2. In your previous study by Cao et al. (2022) you used 250 and 500kPa.s.m−2. Can you justify the use of such a high value?

The same question can be asked regarding the ISO9613-2 model, as the ground factor is 0 in this study, while it was 0.5 in Cao et al. (2022).

Follow-up question: what is the influence of a change in the ground impedance on the optimization results?

- Atmospheric conditions: you consider in Section 4.3 a logarithmic profile. You justify it line 447-448 where you write that “the logarithmic inflow profile is deemed acceptable for the flat terrain in the studied wind farm cases”. However this profile is only valid for neutral conditions. For stable conditions that typically occur at night, more significant wind shear is present and other profiles such as the power law profile need to be used (see for instance van den Berg, Wind Energy 2008). Could you justify the choice of neutral conditions? Also, you seem to neglect the effect of the temperature profile on the results. Is it a valid assumption?

- Transmission loss: you introduce it line 131 but you haven’t given its definition.

3. Discussion on the results

- The axis of Figures 7, 12 and 15 are difficult to read: are these distances in meters? It would be clearer to use the distances in kilometers or in terms of rotor diameter D. Also add the receiver number 1 to 4 as used in the following tables and figures. Finally, it would also be useful to give the distance of each receiver to the closest wind turbine.

- You write line 342 “Thus, a higher L p,j could be expected at these positions due to scattering of sound into the shadow zone.” This is not granted as the levels are quite small in the shadow zone, even if scattering due to turbulence is taken into account. Thus the overall SPL will be dominated by the low frequencies that are not in the shadow zone yet (see Figure 13 of Barlas et al. (2017) at 1386m for instance). Note also that at moderate distances the levels can be higher upwind than downwind (Figure 13 of Barlas et al. (2017) at 252m, 630m and even at 1008m), thus the fact that the receiver is upwind does not necessarily imply that the level should be lower, as you do sometimes (see lines 418-420).

- Sensitivity of the results on input parameters: in Table 2 you show that a variation in wind direction can have a significant effect on the results. Wouldn't it be better to optimize the operational modes for a range of wind direction (and maybe wind speed) values? Also could you comment (in the discussion section?) on the sensitivity of the results on other input parameters such as ground impedance, wind speed profile, temperature profile, turbulence level? (see previous paragraph on input parameters).

- Scatter plot of Figure 11: the results for ISO9613-2 are quite surprising because all wind turbines are highly curtailed (mode 5 or 6), except one that is close to one of the receiver. Isn't there a problem with this solution?

- You mention in Section 5.3 the use of a gradient-based approach that requires continuous function in the optimization process: is it possible to use such an approach with WindStar? Is it compatible with the use of discrete variables? (operational modes)

- You write line 449-451: "In addition, the turbulence effects in the atmosphere are neglected due to the high computational costs. This will in some scenarios, i.e. when considering receptors in the upwind position of a wind turbine, lead to higher uncertainties due to the omitted scattering of sound": As mentioned previously, atmospheric turbulence plays also a role on the noise emission, and would change the low-frequency part of the spectrum. This may need to be added in the discussion section.

4. Organisation of the paper

Abstract:

- the abstract usually includes the main results/conclusions of the paper. This is missing here.
- the sentence "The optimization is performed by use of the TopFarm framework and the PyWake wind farm modeling" is difficult to understand for someone who hasn't read the paper. It needs to be clarified that TopFarm is used for the optimisation algorithm and that PyWake is used for flow modeling.

Section 3 Optimisation flow:

- I think it would be easier to understand this part if you present the optimization problem given by Equation (7) before the flow chart of Figure 1 that is quite complex
- You mention line 204 that the flow chart reduces when using the ISO9613-2 model.

Wouldn’t it be interesting to add another flowchart for this simpler case?

- Equation (7): I think the constraint is not on the sum of the SPL L_p_ij! Correct this equation
- Line 194, you introduce the power of a wind turbine P_i as a function of U0, theta and m_i. Is this power calculated in Topfarm or in PyWake? Please add a sentence in the corresponding section to precise how the power is calculated as a function of U0, theta and m_i.

Section 4.2: it is not clear what are the ground impedance, temperature and wind speed profiles that are used in the WindStar calculations. Are they the same as those described in Section 4.3 for the test cases? If so you should consider reorganising Section 4.

Appendix A: I am not sure this appendix is necessary, as the results seem quite similar to U=10m/s. If you decide to keep it, add a brief description of the content of the Appendix at the beginning.

Technical corrections:

- You use “receptor” throughout the paper, where I think you should use “receiver” According to Oxford dictionnary a receptor is “a sense organ or nerve ending in the body that reacts to changes such as heat or cold and makes the body react in a particular way”. Usually the term “receiver” is preferred in the context of environmental acoustics.
- lines 41-44: quite long sentence. Consider splitting it into two sentences to make it clearer.
- Line 64: I suppose C_T is thrust coefficient, but you need to define it! Throughout the paper you use C_T a lot, you could sometimes replace C_T by thrust coefficient.
- You write line 104 that “the GTPE model is approximated to a 2D model by assuming independence of the direction of propagation from the source”: independence of what? Do you mean that wind speed and temperature are supposed to be independent on range or independent on the azimuthal angle? Please rephrase and clarify.
- Line 149: you write “which estimates i.e. the Levelized Cost of Energy (LCoE) and the AEP of the wind farm in question”. Rephrase and define AEP.
- Line 156: extend => extent
- Line 282: “it is considered negligible compared to the high transmission losses expected”. Not very clear please rephrase.
- Line 292: “The hub height wind speed is kept at U0 = 10 m/s,...” Not true as Uhub is 9.3m/s for the SWT-2.3-93 turbine. Correct this.
- Line 358 page 18: rephrase the sentence “This is even though the...”