Optimization of wind farm operation with a noise constraint - Reviewer response

Nyborg et al.

November 2022

General response: The authors would like to thank the reviewers for taking the time and effort to review our manuscript. The comments given by the reviewers contribute to a significant improvement and better clarity in the manuscript. The reviewer comments have been copied to the list below and the responses to each comment is written in blue. Changes needed in the manuscript will be done accordingly.

Reviewer 1 - General comments

The topic of the article, which is the optimization of wind farm operation in presence of noise constraint, is meaningful for the research community and within the scope of WES. The ideas it conveys are useful to address the problem of noise emission; the article is also an application of an optimization for a discrete variable problem, and the approach can be used in other studies not related to the noise emission topic. The objectives and hypothesis of the research are clear and clearly outlined. The discussion of the methodology and the results is backed up with sufficient detail. The article is generally well structured.

For these reasons, I think the article deserves to be published in the WES journal. Prior to publication, I would like to ask the authors to address the comments below. I included in "specific comments" some suggestions that I think can make the article more effective. In "technical corrections" you can find a list of typing errors, the request to improve clarity of some sentences, or suggestions to improve the presentation quality.

The authors would like to thank the reviewer for the very positive general feedback on our manuscript! It is clear from the comments that some of the explanation and discussion could be improved and that a general better clarity could be gained. Thus, the specific and technical comments given by the reviewer are a great help to obtain an overall improvement of the manuscript. We have further addressed each comment in the lists below.

Reviewer 1 - Specific comments

- I would modify the title to cover the use of two sound propagation models of different fidelity. For example: "Optimization of wind farm operation with a noise constraint and two sound propagation models of different complexity".
  Thank you for the suggestion to change the article title, which definitely covers more of what

is done in the paper. We are however a bit afraid that the suggested title is too long and would therefore prefer to keep the article as is.

- The abstract conveys the meaning of the article, but it doesn't give key results. The abstract should present in brief the main results of the article and offer a short description of their interpretation.
The abstract is modified to include some of the key results of the paper including the improved optimization observed when using WindSTAR over the ISO model.

- 56-60: "the aim of this paper is ... in the specific flow case". I think this does not cover the true aim of this paper. I would say the aim of this paper is to present a computational framework to optimize the operational mode of turbines in a wind farm in presence of a noise constraint. This is the main goal. Then, there is a second one, which is to show the eventual advantages of using a more complex and computationally expensive sound propagation model in place of a simpler one. The sentence "but could be transferred to layout optimization with noise constraints by considering the Annual Energy Production (AEP) of the wind farm instead of the power production in the specific flow case" is not clear. Instead of this sentence, I suggest you summarize the main elements of novelty of this article and explain its expected impact.
The introduction is updated with aims more fitting to the content of the paper. Furthermore, as suggested by the reviewer, the main elements of novelty and their expected impact have been included.

- Section 2. I think it would be beneficial to add a couple of sentences at the beginning of the section (below the main title) to introduce the structure of the computational framework; you should briefly explain how Topfarm and PyWake are combined and how the sound propagation model come into play. I would anticipate here the sentence at lines 186-187: "the modeling in the problem is divided into two parts: the wind farm wake modeling and the sound propagation modeling" and add some details to it.
The description of the structure of the framework has been extended and more explanation about the combination of Topfarm and PyWake has been provided.

- Section 4.2. I think you should add some details about the vertical wind profile used for these simulations, consistently with what is done in the next subsection.
The order of the subsections in section 4 has now been changed such that the subsection "Test cases and constraints" comes before the subsection "CT sensitivity of WindSTAR". Furthermore, it has been specified in which conditions the studies in section 4.2 are performed.

- Section 5.3. In my opinion this section does not bring any new result, so it can be shortened, removing parts already covered before, and merged with conclusions.
Thank you for the suggestion. We have still not fully decided whether to remove the section or not in the revised manuscript, since several new discussion points have been suggested by the reviewers. We might end up merging the "further discussions" section with the conclusion and make a "future work" section as well.

Reviewer 1 - Technical comments

- 6-7: the sentence "Thus, as the WindSTAR model introduces a higher complexity of the sound propagation computations, it likewise introduces a higher computational time" is redundant

and can be removed.
Adopted

- 21-23: "A method for effectively choosing at which operational mode each of the wind turbines should operate during varying atmospheric conditions is therefore needed". The link between operational mode and the atmospheric conditions is not clear. What do you mean with atmospheric conditions? You should name the parameters you are considering, like temperature, air density, ...
The explanation is elaborated with a clear definition of the considered parameters.

- 41: what do you mean with "meteorological conditions"? You should be more specific.
By "meteorological conditions" we refer to the wind speed, wind direction and temperature used in the ISO 9613-2 model. This is now elaborated in the manuscript.

- 44-45: "Thus, the attenuation of the sound has further been shown to change with the stability of the atmosphere (Barlas et al., 2018)". The link between this sentence and the text before it is not clear.
The paragraph is modified with a better context and link between the sentences.

- 47: remove "immense".
Adopted

- 51: "is along with the ISO 9613-2 model used for optimization in this study." Change with "is used along with the ISO 9613-2 model for optimization in this study."
Adopted

- 55: replace "the model" with "it".
Adopted

- 85: Frequency (f) is used in the equations above and should be introduced after eq 1.
The frequency is now defined after Eq. 1

- 108: "in the r-direction". Is it "radial direction"?
Yes, with the r-direction we were referring to the radial direction. This is now specified.

- 108: " a grid resolution of ...". I think you should define the coordinates system (what are the r and z directions?).
The coordinate system is given relative to the considered wind turbine and receptor pair and relative to the terrain (only relevant in case of complex terrain). The manuscript is updated with a definition of the coordinate system.

- 113 and 126: "too excessive" remove "too".
Adopted

- 128: "to be uncorrelated and the average" is it "average pressure"?
Yes, or more specifically average sound pressure level. This is now specified.

- 132: "From which the attenuation of the sound will henceforth be referred to as the transmission loss, TL". This sentence is not clear and should be rephrased.
The manuscript is updated with a definition of the transmission loss. The transmission loss includes the relative sound pressure level, the geometrical spreading and the atmospheric attenuation.

- 143-144: "While the ISO 9613-2 model can be evaluated on a laptop, the amount of physics included in the WindSTAR model require a cluster for the computations". Can you support this sentence with quantitative information (i.e., execution time for the same simulation with the two models)?
  Examples on execution time for both models are now added to the manuscript. The example considers the total computational time for the range of frequencies and sound propagation from a wind turbine to a receptor 1000 m away.

- 149: "which estimates i.e. the Levelized Cost of Energy (LCoE)" remove "i.e.".
  Adopted

- 149-150: "Topfarm has among others previously been used" replace with "Topfarm has been previously used".
  Adopted

- 163: "presented" change with "presented here".
  Adopted

- 168: "PyWake is loosely coupled to WindSTAR and wrapped with the Topfarm optimization framework". This sentence is unclear and must be rephrased.
  In relation to one of the specific comments by reviewer 1, more explanation about how PyWake and Topfarm are combined is given.

- 168: "Furthermore, the" replace with "The".
  Adopted

- 169: "Ct curve" replace with "Ct (thrust coefficient) curve".
  Adopted

- 169: "wind turbine type" replace with "wind turbine under consideration".
  Adopted

- 170: "an iterative downstream manner" unclear, try to use different words.
  By this, it is meant that depending on the wind direction, the first wind turbine considered in the computations is the far upstream turbine followed by the wind turbines further downstream. This is now specified in the manuscript.

- 171: "(Bastankah et al. , 2014)" remove brakets.
  Adopted

- 172-176: "WindSTAR has previously been coupled ... in the presented work". I think these sentences do not add much to the discussion and can be removed. In case you want to keep them, you must explain more clearly the meaning of the sentence "However, the source strength is provided through the operational modes and the Qian wake model is further not yet available in the PyWake framework, but only loosely coupled with WindSTAR through an implemented Fortran version" and why this is important for the rest of the analysis.
  The paragraph has been reconsidered and removed.

- 193: remove "both model parts (red." and ")".
  Adopted

- Figure 1: Can you modify this chart to highlight differences between the approach based on WindSTAR and on the ISO standard?
  The chart is modified to be more general for both models and the differences are elaborated in the manuscript.

- 220: I would reword the title, for example "Test cases for the optimization framework".
  Adopted

- 224: replace "has a size" with "consists of".
  Adopted

- 227: I think you can remove "In order to have dwellings in a near distance of the wind farm at which the noise constraints should be fulfilled".
  Adopted

- 229: add the sentence "Here, the noise constraints must be fulfilled" at the end of the paragraph.
  Adopted

- 230: remove "original".
  Adopted

- 235: "the sensitivity of the Lp" change with "the sensitivity of the Lp in a greater number of turbine operating conditions;"
  Adopted

- 235: "while the higher LW values of the larger turbine type introduce a larger need for optimization". Change with "moreover the higher LW values of the larger turbine introduce a larger need for optimization".
  Adopted

- 243: replace "These distances" with "These nondimensional distances".
  Adopted

- Figure 2 and Figure 3: I think it would be best to use the same limits for the x-axis in the two figures.
  The x-axis is changed so it reaches 20 m/s in both figures. Note, that data is not available beyond 11 m/s for the smaller 2.3 MW turbine.

- 250: "considered outside of" replace with "not influenced by".
  Adopted

- Caption of Figure 4: "in the wake of (a) wind turbine".
  Adopted

- 260: "The wind turbine type" replace with "This".
  Adopted

- Caption of Figure 5: "CT sensitivity of WindSTAR obtained transmission loss" not clear.
  The caption is rephrased and a clear definition of the transmission loss is added earlier on in the manuscript.

- 287: "are used" is "is used".
  Adopted

- 294: "in the wake" add here "of an upstream unit".
  Adopted

- 302: "are representative to a hard" is "are representative of a hard".
  Adopted

- Figure 7, figure 12, figure 15: there are no units in the x and y axes labels.
  The unit [m] has now been added to the Easting and Northing coordinates.

- Figure 8, figure 10, figure 13, figure 14, figure 16: the two subplots on the left are not clear. I suggest plotting the lines in a 2D plot. The color (or line style) is enough to distinguish the 7 turbines.
  A 2D plot was originally used for the figure, but concluded too messy for the 20 wind turbines case. This is why we also include the scatter plots because they convey the same message of the operational modes for the different turbines. However, the figures showing the 7 turbines is changed back to 2D.

- 343: "In general, it should for all optimization cases be kept in mind" change with "In general, it should be kept in mind for all optimization cases".
  Adopted

- Figure 9, figure 11, figure 17: the colormap should use a discrete number of colors equal to the number of operational modes.
  Adopted

- 358: replace "this is" with "this occurs".
  Adopted

  404-405: "Hence, the CT curves ... the wind farm". Not clear.
  The sentence is rephrased.

Reviewer 2 - 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.

The authors would like to thank the reviewer for the extensive and detailed review of our paper. We acknowledge that the assumptions made in the paper need to be better justified and have taken this into account in the revision of the paper by adding more discussion of the influence of the mentioned parameters (for example the ground impedance and the stability) on the modeling results and by adding clear arguments for the assumptions done. We would further like to thank the reviewer for the detailed specific and technical comments. Implementing the comments to the revised manuscript significantly improves the presentation of the study and the overall messages of the paper. However, we would like to emphasize that some simplifications/conservative choices of parameters were needed in the model when used in an optimization context and that the paper is considered a first demonstration of the newly developed framework. Therefore, we hope that the reviewer can acknowledge that extensive sensitivity studies are left to future work.

The reviewer refers in many instances to the paper by Cao et al. 2022. As co-authors we have a detailed insight into this paper and have deliberately chosen to not consider this study for the work done in our presented manuscript. There are several shortcomings in this paper and therefore we do not trust its results. The most severe issues are:

- The paper of Cao et al. 2022 does not account for measurement variability and uncertainty. Out of 33230 minutes of available measurement data only 50 minutes were used in this study.
- Several simplifying assumptions have been made in the model used by both Nyborg et al. and Cao et al. For instance the flow profile is a constant in the propagation direction and does not take into account the terrain effects. Only the wind turbine wake is considered by a simple linear model. Therefore, it is expected that differences between measurements and computations are obtained for both methods.
- Based on the fact that a small amount of data was used for validation in Cao et al. 2022, we are unsure if the study is representative of the performance of WindSTAR.
- The data at the measurement locations furthest away from the turbine had very low signal to noise ratio and is therefore very sensitive to background noise estimates. We therefore question if the background noise estimates are statistically appropriate in many instances.

We could further elaborate about the shortcomings of the Cao et al. paper, but this is not the place here. We hope that with this additional information the reviewer agrees with our reasoning of why we chose not to refer to this study.

The individual comments have been further addressed below.

Reviewer 2 - 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?
Directivity effects have been reported in a few publications. The conclusions were mostly qualitative, because there is not enough data to support a quantitative directivity model for a specific type of wind turbine. Up to date, it is not clear if a directivity model should be general or if it differs from turbine to turbine. We make a conservative estimation of the source directivity when assuming uniform directivity with the source strength inferred from downwind measurements, because the studies agree in that the cross wind noise emission should be less or equal to downwind noise emission. The directivity is left out of the computations to keep the focus entirely on propagation. Thus, as the directivity has a high impact in, for example, crosswind positions, we want to avoid this uncertainty.

- 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?
  The source strength given by the manufacturers is the most accurate way to describe source power. Low frequency noise could be modelled, but the uncertainty that analytical wind turbine noise models such as that of Bertagnolio et al. 2017 (https://doi.org/10.1002/we.2) would introduce exceed the benefits by far. We acknowledge that taking the noise emission modeling concerning turbulence effects into account can be part of future work with the optimization framework. Thus, it is left out of this paper to keep the focus on effects of the propagation models.

- 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.
  While this is true for the use of a single point source, using several point sources to represent the wind turbine rotor has earlier been observed to smear the interference patterns caused by ground interaction. Hence, spectral dips are not that pronounced and the spectrum at locations further away from the turbine is broadband. This observation is in accordance with measurements. Thus, the integrated SPL becomes less sensitive with respect to the frequency resolution and performing the calculations for 3 point sources and the octave band centre frequencies reduces the computational time, which is important in an optimization aspect.

- 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.
  Thank you for spotting the error. The equation has been modified so that L_p is changed to ΔL_p. The factor 2 came from the fact that we use p_{free}^2 in the equation. We change the equation to $|p_{free}|^2$ and remove the factor 2.

- 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 con-

ference 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?
The results of Nyborg et al. 2022 show a significant improvement when going from 1 to 3 point sources. Furthermore, independent comparisons with the use of 1 point source, 3 point sources and 21 point sources have been done, which showed a significant improvement going from 1 point source to 3, but a very small improvement when going from 3 to 21 point sources. We are aware of the study by Cao et al. (2022), but the argument is more based on the sensitivity of the model to the number of sources used rather than how well the different uses of the model fits to measurements. A reference for figures showing an example of the SPL spectrum going from 1 to 3 to 21 point sources is added to the manuscript.

- 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!
Since the grid step size follows the wavelength of the considered frequency by $\lambda/8$, the amount of storage needed for these frequencies is excessive and thus causes numerical instabilities. It is further expected that the 4 kHz and 8 kHz sound has little to no effect on the overall sound pressure level due to the large contribution from the atmospheric attenuation. Therefore, these frequencies are omitted in the calculations.

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 $2 \cdot 10^4$ 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 500 kPa s/m^2. Can you justify the use of such a high value?
The manuscript is updated with a definition of the remaining parameters used in the Attenborough model. The flow resistivity of the test cases is chosen to represent that of a hard ground surface and thus follows the Danish standard of the use of the ISO 9613-2 model (G = 0).

- 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).
The use of G = 0 in the ISO 9613-2 model is to follow the Danish standard use of the model. Thus, in order to be conservative, the Danish standard always considers propagation over a hard ground surface.

- Follow-up question: what is the influence of a change in the ground impedance on the optimization results?
It is well known that a softer ground (e.g. snow covered ground) with a lower ground impedance absorbs more of the sound, which would result in a higher power output in the optimization results. Since this article is considered more a "proof of concept" of the developed optimization framework, more conservative choices have been made with regards to e.g. the ground impedance. A bit more discussion of this is added to the manuscript.

- 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?
The logarithmic profile is a widely used profile for both neutral, stable and unstable conditions. We do not recognize that the power law is a more beneficial profile for stable conditions (See for example "An introduction to boundary layer meteorology" by R. B. Stull, 1988). Neutral conditions were chosen to demonstrate the use of the optimization framework. Hence, future work will include sensitivity studies of the optimization in different atmospheric conditions. The temperature profile is included in the sound speed profile, however the gradient is set to 0 (constant temperature) due to the assumption of neutral conditions. We acknowledge that future work with the optimization framework should include systematic sensitivity studies of the temperature profile and stability conditions.

- Transmission loss: you introduce it line 131 but you haven't given its definition.
  The manuscript is updated with a clear definition of the transmission loss.

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.
  Yes, the Easting and Northing coordinates are in SI units. The units are now added to the figures. The receptor numbers are added as well, but adding the distance to the nearest wind turbine makes the figure a bit too packed with information.

- 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).
  We acknowledge that the work by Barlas et al. (2017) reveals higher sound pressure levels even in an upwind scenario. However, this should be considered individually on a case to case basis and not generalised by one case. Thus, in the computations performed in our study, the levels are reduced due to a shadow zone. We add some discussion stating that it cannot be generalised for upwind cases that a shadow zone is present by referring to the work of Barlas et al. (2017).

- 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).
  We acknowledge that a systematic sensitivity study is needed in the future. The work presented

in this article is considered a "proof of concept" of the new optimization framework, which is why the parameters are kept relatively conservative. Our thoughts related to the sensitivity of the optimization to the mentioned input parameters have been elaborated in the discussion section.

- 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?
  We agree that this result is counterintuitive. But upon further inspection we found that it complies with the optimisation objective and is with the given contraints. Therefore we do not think that there is a problem with the result. We expect that the result can be explained by the fact that the middle turbine in the row is close to a higher amount of receptor and thus reducing the operational mode of, for example, this turbine will cause receptors to be above the constraint. Furthermore, the solution should be looked at not only from a constraint perspective, but also an objective perspective. Thus, reducing some wind turbines a lot and leaving other turbines at mode 1 or 2 may result in a higher overall power output than reducing all wind turbines an equal amount.

- 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)
  Comparing to previous studies gradient-based optimization applied to discrete variables in, for example, wind farm layout optimization we expect that this can be applicable for the operational modes as well. See for example Pollini (2022) https://doi.org/10.1016/j.renene.2022.06.019. References to gradient-based layout optimization are now added to the manuscript.

- 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.
  We add a point to the discussion about that the turbulence also plays a role on the noise generation and that this has not been taken into account in the model.

4. Organisation of the paper
Abstract:

- the abstract usually includes the main results/conclusions of the paper. This is missing here.
  The abstract is updated with the key results of the paper, namely the improved optimization using WindSTAR over the ISO 9613-2 model.

- 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.
  It is now clarified in the abstract that Topfarm is the optimization algorithm and PyWake is the 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.
  Eq. 7 has now been moved.

- 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?
  The flowchart is intended to be general so that "sound propagation modeling" can be replaced by either WindSTAR or ISO 9613-2 in the figure, while the inputs can be changed as well depending on which sound propagation model is used. This is elaborated in the manuscript now.

- Equation (7): I think the constraint is not on the sum of the SPL Lp_{ij}! Correct this equation.
  Thank you for spotting the typo! The constraint is corrected to be on the sum of $10\log_{10}\left(10^{0.1Lp_{ij}}\right)$. It should be noted that this was only a typo in the paper and that in the optimization itself the constraint has been taken on the sum of $10\log_{10}\left(10^{0.1Lp_{ij}}\right)$.

- 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.
  The power is calculated in PyWake. More explanation is given to how the power is estimated from 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.
The order of the subsections in Section 4 has now been changed so that the section "Test cases and constraints" defining the ground impedance, temperature and wind speed profiles comes before the section "CT sensitivity of WindSTAR". Furthermore, it is explicitly specified in which conditions the sensitivity studies are performed.

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.
The figures in the appendix support the sensitivity study done in Section 4.3. Thus, by including more wind speeds (below rated and way above rated) we get a more quantified sensitivity study instead of basing it on a single wind speed. Based on this, we have decided to keep the appendix, but have added a brief description of the context to the rest of the paper to the appendix as suggested by the reviewer.

Reviewer 2 - Technical comments

- 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.
  "Receptors" can also refer to noise sensitive locations, which is the case in the presented study.

- lines 41-44: quite long sentence. Consider splitting it into two sentences to make it clearer.
  Adopted
- 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.
  CT (the thrust coefficient) is now defined at the first time it is used.
- 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.
  By "independence of the direction of propagation", we refer to that an omnidirectional source is assumed in order to do the 2D approximation of the PE. This is specified in the manuscript.
- 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.
  The sentence is rephrased and the AEP is defined.
- Line 156: extend = extent
  Adopted
- Line 282: "it is considered negligible compared to the high transmission losses expected". Not very clear please rephrase.
  The sentence is rephrased
- 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.
  Adopted
- Line 358 page 18: rephrase the sentence "This is even though the..."
  Adopted