



Wind Energy Science

6th March 2023

RESPONSE TO REVIEWER

Dear Reviewer,

We would like to thank you all for your time and insightful comments about our article entitled “A Neighborhood Search Integer Programming Approach for Wind Farm Layout Optimization” (submission WES-2022-82), for appreciating the contribution of this work, and for considering the topic and proposed method as relevant and promising. We have made a large effort to improve the quality of the paper and to address all your comments and suggestions.

See a modified version of our article attached after this letter.

Find below responses to each of your comments:

GENERAL COMMENTS

- Comment: This work presents a new approach (NSH) to solving the wind farm layout optimization problem using a MILP approach that is made more tractable by a simplified wind farm AEP model. The results of the model and the optimization algorithm are clearly compared to previous works and seem reasonably reproduceable. The work appears to be well-founded from a scientific perspective, is relevant to the subject matter of Wind Energy Science, and provides meaningful contributions to field. While the work is reasonably well presented, the English grammar and usage in the work present a barrier to understanding. The manuscript should be carefully, preferably professionally, edited to address these concerns so the material will be more accessible, clear, and useful to the community.

- Response: Thanks for appreciating the contributions presented in the article. Regarding the English grammar and usage, we have conducted a thorough review of it to improve the quality of the manuscript.

SPECIFIC COMMENTS

Abstract

- Comment: Line 4-5: “deficit is aimed” I don’t know what is meant by this.

*- Response: Thanks for pointing out this misleading statement. The word “aimed” has been replaced by “**optimized**”, so now this sentence should be clearer in transmitting*



the idea that in the power-curve-free model is optimized a measure closely related to wind speed deficit.

- Comment: Line 5-6: it is unclear if the heuristic wraps the model (formulations?) or is separate. Consider clarifying.

- Response: Thanks for the suggestion. As an attempt to increase clarity, this is restated as:

“A special-purpose neighborhood search heuristic wraps **these** formulations increasing tractability and effectiveness compared to the full model **that is not contained in the heuristic.**”

- Comment: Line 8: This sentence was confusing to me, but I think I understand. Consider reworking. I think the intended meaning is that the results of the benchmark problems show that using some substitute objective rather than actual AEP can be a good approach.

- Response: Thanks for the suggestion. As an attempt to increase clarity, this is restated as:

“...Numerical results on a set of publicly available benchmark problems indicate that a proxy for total velocity deficit as objective is a functional approach, since high-quality solutions of annual energy production metric are obtained, **when using the latter function as substitute objective...**”

- Comment: Line 10: “match” is probably a bit strong for the presented results, maybe say the results are competitive or something that does not indicate equality.

- Response: Thanks for point this out. The authors agree. This sentence is restated as:

“...Furthermore, the proposed heuristic is able to **provide good results compared to a large set of distinctive approaches that consider** the turbine positions as continuous variables.”

1: Introduction

- Comment: Line 17: I don't think I'm convinced about the importance of wind farm layout optimization by this paragraph. You state that wind energy is important politically, is presumably profitable without subsidies, and is a mature industry. The profit and maturity seem to hurt the argument for why this study is important. It sounds like

things are just fine without WFLO. I'd suggest re-working this first paragraph. You could consider discussing the tight margins of wind developers and OEMs, especially offshore. You could also mention some hard values for how improved wind farm layouts could reduce the cost of energy even further. Basically, be careful to lay a clear foundation for why this work matters. You don't need to cover a lot of detail or history, but do make a clear case.



- Response: Thank you for the remark. Indeed, it may read a bit as a contradiction. Authors re-phrased the paragraph focusing on why lower costs, partially achievable via optimization, are important.

“...For wind energy to become the cornerstone of a successful green energy transition, further reduction in costs - partly achievable by economically refined wind farm designs - will play an important role.”

- Comment: Line 24: I think you are citing Deb (2013) here for an example of a GA, but it reads like you are pointing readers to the GA that Masetti used, only the dates don't line up (2013 vs 1994). Consider reworking this or putting the expected citation (or no citation, you already cited Masetti which presumably has the information on the GA).

- Response: Thanks for the suggestion. The citation to Deb (2013) has been removed as the authors agree with this comment.

- Comment: Line 26: Consider removing “and the associated numerical algorithms” because you are stating “main components”. Nearly all computational methods will have “associated” algorithms. However, I'd argue that the wake combination model qualifies as a “main” component as well.

- Response: Thanks for pointing out this misleading statement. The first sentence of this paragraph is changed to:

“The main components when building an optimization workflow for the WFLO problem are the wake models (deficit and superposition), the program formulation, and the associated numerical algorithms...”

The authors consider very important to differentiate the three aspects: wake modelling, problem formulation, and numerical algorithms. Essentially, an optimization program is set up by defining each of them according to the needs of the problem and choices of the designer. As it is well known, there are plethora of wake models that can be used for optimization. Likewise, different problem formulations can be selected, including for example, discrete or continuous modelling, distinctive objective functions and constraints structures, among others. Lastly, it is important to emphasize that for any combination of the previous two components, several solution algorithms can be utilized, for example SLSQP, branch-and-cut, etc.

- Comment: Line 30-34: while the wake model background may not need to be complete, the background given here is not quite correct.

1. I think Niayifar and Porte-Agel (2015) is mostly focused on the wake combination and turbulence intensity to extend the Bastankhah model to multiple turbines. In this light the citation would be better placed with the wake combination citations. Also note that there is a journal paper by the same authors from 2016 on the topic that may be a better source to cite here.

2. The Jensen cosine model was actually proposed by Jensen in 1983, so it may be good to cite that paper for the Jensen cosine model as the original source, though the Thomas et al. paper does provide some clarifications.

3. The list as given seems to show several smooth and differentiable wake models, but the combined citations seem to really only refer to two distinct wake models. I'd suggest making this a little more clear in the discussion.

4. While the sum of squares or linear combination statement is correct to my knowledge, it may be worth mentioning that the two methods have been used with local and freestream velocity conditions. This makes for four distinct proposed wake combination methods.

– Linear/freestream: Lissaman 1979

– Sum of squares/freestream: Katic et al. 1986

– Linear/local: Niayifar and Porte Agel 2015, 2016

– Sum of squares/local: Voutsinas 1990 Update: I saw you do discuss this nuance later. It may ok as is, but it did seem incomplete to me at first.

- Response: Thanks for this really good point. As the reviewer says, the wake model background may not need to be complete, but it should be improved respect to what was presented in the first version. The rest of the paragraph has been edited as

“...For formulating tractable frameworks, the designer needs to rely on the so-called engineering wake models. These are essentially mathematical representations which can be expressed in terms of analytical equations after significantly simplifying complex physics modelling, while still capturing to a good extent the underlying nature of the phenomenon under analysis. Scientific articles in this field have proposed and validated engineering wake models with smooth and differentiable velocity deficit shape, two examples are the Bastankhah's Gaussian (Bastankhah and Porté-Agel, 2016) or its simplified version (IEA Wind Task 37, 2019), and the Jensen cosine model (Jensen, N.O., 1983). Likewise, the aggregation of individual wake velocity deficits can be done through linear superposition (Lissaman, 1979) or root sum squares (Voutsinas et al., 1990), with local or freestream velocity conditions (Porté-Agel et al., 2020).”

The authors consider that with these modifications the wake modelling state-of-the-art review complies with the points highlighted by the reviewer.

- Comment: Line 49: the jump from gradient-based and gradient-free algorithms to discrete algorithms was not clear and needs motivation. Consider stating the connection and purpose of the jump for those unfamiliar with the algorithms ((1) discrete methods are generally a sub-set of gradient-free methods, and (2) why are we talking about them here?)

- Response: The authors do not agree with this point, as they believe that there are no discrete algorithms, but rather two modelling philosophies with respect to variable types: continuous and discrete optimization. In the paragraph from lines 36 to 48 (in the new version of the manuscript) the continuous optimization technique is discussed. For this, both gradient-free and gradient-based algorithms have been utilized in the literature. The following paragraph from lines 49 to 54 is new, where the latest work of (LoCascio et al., 2022) is discussed. In the next paragraph (lines 55 to 65) the discrete optimization technique is discussed. Within this field, both gradient-free and gradient-



based algorithms have also been applied. To clarify this, the following sentence has been added:

“...Algorithms utilizing explicit gradients are also a valid approach in this field (Pollini, 2022)...”

This is an article very recently published that adopts a discrete modelling technique as well, using gradient-based algorithms to address the WFLO problem.

The objective of paragraph from lines 66 to 73 is to introduce the motivation of the focus of this manuscript which is integer programming modelling, presenting the inhering modelling benefits of discrete optimization in this context. Lastly, lines 74 to 84 picks up this idea and discusses state-of-the-art in integer programming for the WFLO problem. The next two paragraphs in lines 85 to 94 and lines 95 to 105 discuss the contributions of this manuscript.

- Comment: Line 59-69: consider also citing <https://wes.copernicus.org/articles/7/1137/2022/>

- Response: Thanks for giving notice to the authors for this very interesting work. This manuscript is cited in the paragraph from lines 49 to 54, right after the paragraph that in general discusses continuous optimization. The authors think that is a great addition to expand the concept of applying simpler objective functions that mitigate the complexity of optimization programs, while still being very competitive finding good solutions compared to more sophisticated models. Note that this article is again cited in lines 92 to 94 to contrast it with the proxy objective function proposed in the authors' manuscript.

- Comment: Line 71: how is modeling economic metrics an advantage of the discrete model? This can and has been done in a continuous space for optimization. see <https://onlinelibrary.wiley.com/doi/epdf/10.1002/we.2310>

- Response: In this line it is stated “capacity to include the number of WTs as a variable and to model overall economic metrics as Net Present Value (NPV)”. In the mentioned article “Optimization of turbine design in wind farms with multiple hub heights, using exact analytic gradients and structural constraints” the focus is, as the title states, on how to optimize wind farm layouts accounting for WT design. The problem assumes fixed number of WTs. It is clear that the difference lies in the fact that by a discrete modelling technique the number of WTs is considered an optimization variable. With variable number of WTs, the modelling of overall financial metrics as NPV would expose the trade-off between the number of WTs in the farm and the wake losses (AEP) vs investment costs. This is in general not possible in classic continuous optimization frameworks.

- Comment: Line 72: (ii) can be done continuously, but it is more difficult

- Response: In this line it is stated “...ease of modeling any shape of project area or forbidden zones, convex or non-convex...”, meaning exactly what the reviewer says in this comment.

- Comment: Line 72-73: why is (iii) specific to a discrete formulation?

- Response: In this line it is stated "...capacity to model extensive integrated models to support electrical systems optimization...". The cable layout

optimization problem, which designs the electrical network to connect the WTs towards the substations, is a discrete optimization problem. It would be straight-forward to formulate a unified optimization program for the simultaneous wind farm and cable layout optimization problem, if the WFLO is modelled in a discrete way. With both problems being in the same modelling universe, it would be clear which optimization algorithms to explore. If the WFLO is modelled continuously, the authors cannot envisage a tractable way of tackling the unified problem.

- Comment: Line 73-74: what is the distinction between "cost functions" (iv) and "economic metrics" (i)?

- Response: An economic metric is defined in this context as the expression used to value a project from the financial perspective. NPV and IRR are examples. Cost functions refer to the mathematical representations to calculate the value of components that are required to fully compute economic metrics.

- Comment: Line 74: fully continuous WFLO has been done with multiple turbine types <https://www.wind-energ-sci.net/4/99/2019/wes-4-99-2019.pdf>

- Response: The authors are aware of this. It is mentioned about the possibility to *easily* do this. Therefore, the sentence is rewritten as:

"...**ease of** incorporating multiple WT types, among others..."

- Comment: Line 76-77: consider elaborating on this idea and why discrete optimization is well suited to overcome the convexity problem

- Response: This is not the meaning of this sentence. The non-convexity nature cannot be overcome. Nevertheless, because of this feature, it is not possible to formally prove optimality. Due to this, usually different solution algorithms will converge into different final solutions. By having a diverse set of available solution algorithms, the likelihood to obtain better solutions for a given problem instance is increased.

2: Physics Modelling

- Comment: Line 97-99: I'm not sure how this statement "No particular . . ." relates to the first sentence in the paragraph. Also, Thomas et al. 2022b specify some restrictions on the mathematical structure for controlling wake diameter and deficit, at least for their purpose. Specifically, the wake deficit and wake diameter must be separately controlled

- Response: Thanks for the feedback. To improve readability and connection between sentences, the statements have been rewritten like this

“The proposed MILP models and general optimization framework in this article can be easily applied to many wake deficit models. No particular properties on smoothness or differentiability **are required from these models for optimization purposes. Additionally, no** specific demands on mathematical structure in connection with

controlling wake diameter and deficit (Thomas et al., 2022b) **are stemming from the optimization programs proposed in this article...**”

- Comment: Line 101: from which source? there are two references

- Response: Thanks for pointing out this redundancy on the references. Reference (Dykes et al., 2015) has been deleted, because (Baker et al., 2019) is an indexed paper containing the information about wake model and benchmark results.

- Comment: Line 105: why is Thomas and Ning 2018 cited here and at line 32 for the simplified Gaussian? For the original Bastankhah wake model, I'd suggest citing Bastankhah 2016. For the simplified model, use the citation given in the following sentence (IEA Wind Task 37 2019)

- Response: Thanks for the suggestion. For the original wake model, it is cited (Bastankhah and Porté-Agel, 2016) and for the simplified model (IEA Wind Task 37, 2019), as suggested by the reviewer.

- Comment: Line 110: d_{ij}^{\parallel} and d_{ij}^{\perp} are not used in Eq. (1), though the coordinate frame clarifications are helpful, the symbols used seem extraneous at this point in the paper. You could possibly include these symbols as additional equations following the equation explanation of Eqs. (1) and (2) in preparation for use later in the paper.

- Response: Thanks for the suggestion. The authors consider that defining d_{ij}^{\parallel} and d_{ij}^{\perp} is useful at this point of the paper because variables \bar{x}_ℓ , \bar{x}_i , \bar{y}_ℓ and \bar{y}_i are introduced here. Trying to improve readability, this paragraph is restated as “where u_∞ is the inflow wind speed, C_T is the thrust coefficient, $\bar{x}_i - \bar{x}_\ell$ is the stream-wise distance from the hub generating wake (\bar{x}_ℓ) to hub of interest (\bar{x}_i) along freestream **(let this difference be d_{ij}^{\parallel})**, $\bar{y}_i - \bar{y}_\ell$ is the span-wise distance from the hub generating wake to hub of interest perpendicular to freestream **(let this difference be d_{ij}^{\perp})**, σ_y is the standard deviation of the wake deficit, k_y is a variable based on a turbulence intensity, and D is the WT diameter.”

- Comment: Line 115: consider removing one of these duplicate mathematical statements.

- Response: Thanks for noticing this typo. It has been fixed.

- Comment: Line 125-130: The references used to arrive at Eqs. (6) and (7) were given in the introduction, but I think it would be helpful to provide them again here.



- Response: Thanks again for this contribution. The references have been added above Eq. (3), Eq. (4), Eq. (6), and Eq. (7).

- Comment: Line 133: why is the power curve non-differentiable specifically at rated wind speed? The definition provided in this manuscript is non-differentiable at the rated

power, but the continuity of the power curve is just dependent on the power curve definition, so this statement is not correct in general.

- Response: The authors have modified this subsection as follows to improve the technical rigor

“Suitable power curves are required for computing AEP. Often, power curves are not perfectly suitable for optimization, due to the usual non-differentiability in several points throughout the function. Generally, a power curve is zero below cut-in wind speed, zero above the cut-out wind speed, and constant between the rated wind speed and the cut-out wind speed. In this particular study, between the cut-in and rated wind speeds the curve is assumed to be smooth, convex and monotonically increasing. The simplified power curve for a generic turbine as a function of wind speed u is modelled through...”

“...where p^{rated} is the nominal power at (and above) rated wind speed u^{rated} . The other turbine characteristics are the cut-in wind speed u^{cut-in} , and the cut-out wind speed $u^{cut-out}$. In this definition, the WT power curve is not differentiable at u^{cut-in} , u^{rated} , $u^{cut-out}$, since in these points the left and right hand side derivatives are different. Be aware that the optimization programs proposed in this manuscript are not dependent on WT power curve differentiability.”

The non-differentiability discussed here is naturally dependent on the power curve definition. However, the one presented aligns with the usual function recurrently implemented in the literature.

3: Optimization Models

- Comment: Eqs. (12) to (14) The presentation here is difficult to follow. Perhaps consider breaking them up into more equations with more explanation and grouping by interval (1, 2, a+1, m+1, m+2).

– the statement “for $a = 1, \dots, m$ ” should be applied to each numbered equation it applies to individually.

– I’m not sure how the delta u is supposed to be applied in Eq. (13). Re-working the presentation of these equations should help.

– are “a” and “l” being used for the same thing here? If so, correct. If not, please clarify.

– There may be a better way to present the interval values, the above are just my ideas at the moment.

- Response: Thanks for this observation. This part has been reworked as presented in the next page.

Authors hope that this improves the readability of this part. The statement “for $a = 1, \dots, m$ ” has been applied individually in Eq. (14) and in Eq. (15), meaning each of the

subintervals sampled within the cubic subdomain of the whole WT power curve domain. The value of Δu has been explicitly declared in the paragraph preceding the equations. Lastly, a and l represents different things. The variable l is for any interval in the whole domain of the curve, while a is for an interval located in the cubic domain.

“
cut-out speed. Each isometric interval **within the cubic domain** of length $\Delta u = (u^{\text{rated}} - u^{\text{cut-in}})/m$, is approximated with a constant power value, see Fig. 1.

An interval l of the **whole domain** is characterized by three parameters u_s^l , u_m^l , and u_h^l with the next properties

$$u_s^1 = -u^{\text{ini}}, u_h^1 = u^{\text{cut-in}}, u_s^{m+2} = u^{\text{rated}}, u_h^{m+2} = u^{\text{cut-out}} \quad (12)$$

$$u_s^2 = u^{\text{cut-in}}, u_h^{m+1} = u^{\text{rated}} \quad (13)$$

$$u_s^{a+1} = u^{\text{cut-in}} + (a-1)\Delta u \text{ for } a = 1, \dots, m \quad (14)$$

$$u_h^{a+1} = u^{\text{cut-in}} + a\Delta u \text{ for } a = 1, \dots, m \quad (15)$$

$$u_m^l = 0.5(u_s^l + u_h^l) \quad (16)$$

”

Equation (12) defines the lower and upper limits for the extreme intervals $l = 1$ and $l = m + 2$, Eq. (13) formalizes the lower and upper limits for the first interval in the cubic part, $a = 1$, and the last one $a = m$, respectively. Equation (14) expresses the lower limits for intervals in the cubic part ($a = 1, \dots, m$), while Eq. (15) does it for the upper limits. Equation (16) presents how to determine the extracted wind speed associated to the interval l of the whole domain, which is the average value of u_s^l and u_h^l .”

- **Comment:** Eq. (16a): ξ , η , and u are specified as design variables, but I think η and u are state variables dependent on ξ , so it seems that ξ represents all design variables. I’ve only seen design variables represented in the sub-scripted variables under the “maximize” in the optimization equation.

- **Response:** The reviewer is right about the fact that η and u are state variables fully dependent on ξ . However, the authors do not agree with the observation that the subscripted variables under “maximize” should only present fully independent

variables. For completeness, authors chose to present all variables required in an optimization program, regardless of relation of dependence between them. On the other



hand, the article explains clearly the difference between binary variables λ_i and the state ones η , u , and τ .

- *Comment:* Section 3.2: this approach appears similar to the FLOWERS model found in <https://wes.copernicus.org/articles/7/1137/2022/wes-7-1137-2022.pdf>. I'd suggest contrasting the method in the submitted manuscript to the FLOWERS model, perhaps in the introduction, but referring back to it again here.

- *Response:* The manuscript is cited in the paragraph from lines 49 to 54, right after the paragraph that discusses continuous optimization in general. Thereafter, this article is again cited in lines 92 to 94 to contrast it with the proxy objective function proposed in the authors' manuscript. Finally, this article is referred back in this section in lines 266 to 268 as

“This proxy objective function is very useful for formulating the program in the MILP category. While the work in (LoCascio et al., 2022) focuses on a different formulation (likely more accurate analytically than the one presented here) that is non-linear but gradient friendly, hence useful for continuous gradient-based optimization.”

- *Comment:* Line 204: I need a little clarification regarding which “outlook” the IEA 37 studies follow.

- *Response:* The authors have rewritten this sentence to improve readability as follows

“Albeit the formulation of Sect. 3.1 represents to a very large extent the physics ruling the problem, it has a considerable number of variables and constraints that may hinder the capacity to tackle larger problems. The model presented in this section neglects power curve and AEP calculation and aims at simplifying the power-curve-based version.

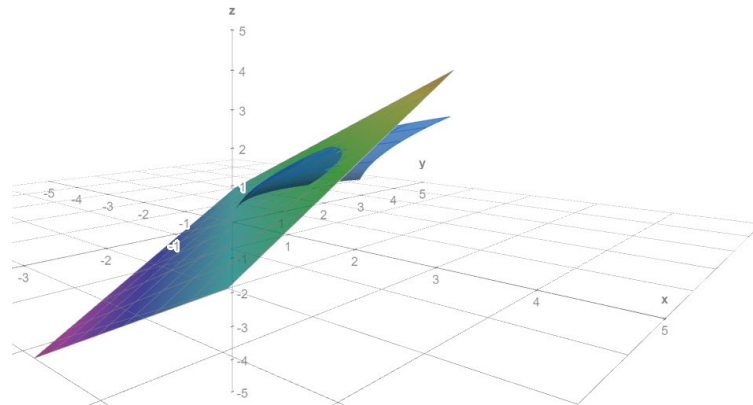
The model deploys a strategy to account for the combination of Eq. (3) and Eq. (7) to calculate velocities, since the case studies from the IEA Wind Task 37 follow this methodology for AEP computation. It would be possible though to consider the linear superposition model if necessary. However, the power-curve-free model does not support the application of Eq. (4).”

The authors mean that that approach is considered in this section for AEP computation, following the methodology implemented in the IEA37 Wind Task.

- *Comment:* Line 211: please provide justification for dropping the square roots. Why is the model expected to be correct if the square root is simply “dropped”?

- *Response:* The authors acknowledge that by simply dropping the square roots the model is not “correct”, but expect that the resultant expression, incorporated in the MILP model, is “good enough”. Line 241 in the new version of the manuscript is added: “...the arguments of the square roots in Eq. (19) define a function closely related to the full root-squared expression...”

To have a better idea about this premise, please see the below plot



The plane plot is of the function $z = x + y$ and the other one is for $z = \sqrt{x} + \sqrt{y}$. Note how these two functions follow relatively close to each other for non-negative values of x and y . A similar behavior is expected between the original root-squared expression and the other one that ignores them.

Practical evidence of the accuracy of this simplification is presented in Table 1.

- Comment: Eq. 20: how did you get to $b_{\{i,l\}} + b_{\{l,i\}}$ and the $l > i$? I don't see offhand how those terms come from combining eqs. 18 and 19 as stated.

- Response: As variable $z_{i\ell}$ represents that both WT's in i and j are selected, then when it is zero, the mutual influence given by summing up both $b_{i\ell}$ and $b_{\ell i}$ must also be zero. By defining the second sum with $\ell > i$, the number of variables is halved after this symmetric property.

4: Neighborhood Search Heuristic

- Comment: Alg. 1, Line 13: check spacing

- Response: Thanks for noticing this typo. It has been fixed.

- Comment: Line 263-264: what is meant by "stopped until"?

- Response: Thanks for noticing this misleading statement. It has been restated as "...The complete model is sent to the MILP solver with ξ as warm-starter, stopped **when** reaches **either** optimality or the assigned maximum computing time T ..."

- Comment: The NSH algorithm seems similar to the one developed by Paul Malisani and presented in "A Comparison of Eight Optimization Methods Applied to a Wind Farm Layout Optimization Problem" by Thomas et al. (<https://wes.copernicus.org/pre-prints/wes-2022-90/>). Consider comparing and contrasting the approaches.

- Response: Thanks for giving notice to the authors for this very interesting work. The authors have added this work in the introduction (paragraph from lines 95 to 105) as



“The second main contribution is the proposition of a new special purpose neighborhood search heuristics in order to speed up the generation of high-quality solutions. This heuristic, wrapping both formulations, has a twofold functionality; first to increase tractability, and second to redirect the optimization search in terms of a specified objective function with higher fidelity. Similar neighborhood search methods have been proposed in the literature, as the Discrete exploration-based optimization (DEBO) (Thomas et al., 2022c), which is a two-steps process composed by a greedy initialization and a local search block. While the method proposed in this manuscript shares most of the advantages of the mentioned approach (no gradients required, can handle unconnected and non-convex boundary constraints, and so on), it actually improves the DEBO algorithm as among others, i) significantly less AEP function evaluations are required, and ii) it is based on well-establish integer programming theory, relying in efficient implementations of the branch-and-cut algorithm. The main numerical results indicate good computational performances for a set of publicly available benchmark case studies compared to state-of-the-art gradient-free and gradient-based approaches (Baker et al., 2019).”

This article is referred back in this section in paragraph from lines 298 to 307 as

“... One of the advantages of the NSH compared to the DEBO algorithm (Thomas et al., 2022c) is the reduced number of AEP evaluations. In an iteration k , only $|S|$ evaluations are required. Likewise, many of the other expensive calculations are done in a preprocessing stage...”

“...Another difference between the NSH and the DEBO is that the latter only changes the position of a single WT in a given iteration, while the former considers simultaneous modifications of several WT positions.”

5: Computational Experiments

- Comment: Line 286: why these parameter values?

- Response: In the line 320 has been added the reference for benchmarking “The main parameters of the wake model in Sect. 2.1 are fixed to $CT = 8/9$ and $ky = 0.0324555$, according to (Baker et al., 2019)”

- Comment: Line 299: it would be nice to see all non-default parameters (the introduction “for example” seems to indicate that only some of the non-default parameters are given). Consider putting in a table with the non-default parameter values.

- Response: This paragraph has been modified to “...The selected MILP solver is the commercial branch-and-cut algorithm implemented in IBM ILOG CPLEX Optimization Studio V20.1 (IBM, 2022). Apart from the number of threads and time limit settings, a few other parameters are also set to different values compared to the default choices as well. One is the parameter returning high-quality feasible solutions early in the process, for which, the (CPX_MIPEMPHASIS_HEURISTIC) is activated. The intention is to generate more feasible layouts which is important for the neighborhood search algorithm. Additionally, strong branching is used for variable selection given the large size of the models

(CPX_VARSEL_STRONG is selected). The intention is to reduce the size of the search tree and thus the memory requirements compared to default settings.”

Since these are the only settings that have been changed from default values, we choose not to add a table for this purpose to avoid enlarging the paper’s length.

- Comment: Line 305-316: was this sampling method compared to any other methods?

- Response: Not being the objective of the manuscript to evaluate different sampling methods, this has not been exhaustively investigated by the authors. One of the experiments not included in the article was to use a Delaunay-triangulation-based sampling of the 1300 m radius circumference. Using the same algorithm parameters, the

presented method in the manuscript consistently improved the Delaunay one. Because more experiments should be done to elaborate a comprehensive comparison, no discussion is presented in this matter. This could be an interesting support work to perform in the short-term future.

- Comment: Line 334: how do we know it is “still strong enough”? What was the bar?

- Response: Thanks for the feedback. The authors agree with the fact that this expression may sound as comparative to a well-defined standard. Instead, this sentence has been modified to

“..In spite of this deterioration, the linear correlation is still **considered quite strong**..”

Although the range of correlation coefficient values and the corresponding levels of correlation vary depending on the application context, a correlation in the interval [-1 to -0.80] is usually deemed as ‘Very Strong Negative’, and between [-0.79 to -0.60] as ‘Strong Negative’. See for example reference <https://www.ccsenet.org/journal/index.php/cis/article/view/59661>.

Since the authors do not aim to provide a formal definition of this aspect, adding the word “considered” should highlight the subjective meaning intended.

- Comment: Line 346: which model is “exact”? All the models presented in this paper appear to be approximations.

- Response: This comment refers to the general finding of the article that focusing on total wind speed minimization (or its use to calculate an approximated AEP function) is a promising research line for the WFLO problem.

- Comment: Line 349: Perhaps the “deterioration” is partly due to “dropping” the square root?

- Response: This is true and it is actually discussed in the lines 377 to 383.

- Comment: Line 349: “this” is unclear, state meaning explicitly

- Response: It has been replaced “this” by “...**Case III**...”.

- Comment: Table 1, Fig. 4: beautiful use and presentation of correlation. Nice work!

- Response: Thanks!

- Comment: Line 363: It would be helpful to provide more information about the tuning process.

- Response: Since the authors consider that there is not an optimal way of tuning these parameters, no extensive discussions are deployed. Some general annotations are given in the following lines about the reasoning behind the presented values of C , T , and V .

- Comment: Line 363: My understanding of C , T , and V was incomplete and I had to go back and re-read previous sections and this sections to get straitened out. I'd suggest adding more explanation of these inputs when you introduce the algorithm.

- Response: Done. This is carried out by
“..The main inputs are $C = \{467, 590, 1014\}$ (set of candidate locations), $T = \{1, 1.5, 2\}$ h (set of max computing times for each candidate location), $V = \{2, 4, 6, 16\}$ (set of neighborhood search sizes). See Sect. 4...”

- Comment: Fig. 5: – are the times shown clock time or CPU time? – while run time is helpful, it can vary drastically depending on implementation, language, system, etc. You may want to consider also including a count of total calls to your objective function.

- Response: It is indeed clock time and it has been clarified in Line 385.

To the authors' knowledge, function evaluation metric is usually used to assess metaheuristic algorithm's performance as they depend upon the number of generations and the size of the population, so it is an indication of the efficiency of the algorithm, considering a given computing time to assess the fitness function once. Some gradient-based solver also provide this metric. However, the proposed method uses an exact formulation and calls an external state-of-the-art solver using branch-and-cut method to get high-quality solutions. Authors see that the vast majority of works in the operations research field using solvers as CPLEX report clock time as normal practice. See for example [https://link.springer.com/article/10.1007/s10732-015-9295-0#:~:text=Relax%2Dand%2Dfix%20\(RF,in%20their%20sophisticated%20lot%2Dizing,or%20https://www.sciencedirect.com/science/article/pii/S2211692317300188](https://link.springer.com/article/10.1007/s10732-015-9295-0#:~:text=Relax%2Dand%2Dfix%20(RF,in%20their%20sophisticated%20lot%2Dizing,or%20https://www.sciencedirect.com/science/article/pii/S2211692317300188). This is usually the case because branch-and-cut black-box solvers do not easily provide this information.

- Comment: Fig. 6, 8: – Are your wind turbine markers to scale? – This figure is missing axis labels – This figure is missing units for the tick labels

- Response: Markers are not to scale. Figures 3, 6, and 8 have been edited so axis labels and units for ticks are added.



- Comment: 5.3: the baseline of the percentages given is unclear. Is each percentage given using the last step level as the baseline or the original “incumbant” value?

- Response: The baseline is the last step commented. In line 450-451 has been added the sentence

“...As for Case I, improvement percentages are calculated using the last commented step as the baseline...”

- Comment: Fig. 7: perhaps I missed where this was stated, but are all the AEP values here calculated using the full model for comparison? If not, I think they should be.

- Response: Correct. This is stated in line 315-319.

- Comment: Eq. 23: The equation in your reference is general, but you provide a specific version here. It would be helpful to introduce the general form of your equation from your reference and then fill in the specifics. You may also want to use a more concrete reference here than Investopedia. There are many for this material.

- Response: From lines 488 to 453, the following described has been added

“.... The general form of the NPV equation (Cogency, 2014) is defined by the sum of the present value of cash flows (Discounted Cash Flow, DCF) of a project under analysis. In Eq. (25), the first sum is a negative cash flow representing purchase of the WTs at the construction stage of the project, while the next term represents positive cash flows coming from trading the electricity in the market. Because of the additive nature of the NPV metric and since the focus is on evaluating investment vs revenues, by maximizing Eq. (25), a fully comprehensive NPV metric is equivalently improved.”

- Comment: Line 474: The last sentence here needs more explanation.

- Response: By expanding the previous paragraph and with the following sentence, the authors consider that the explanation has been improved

“When the number of turbines is fixed to 10, the NPV evolution (green line in Fig. 11b) is driven by the AEP (green line in Fig. 11a). Both curves are monotonically increasing, reaching a final value of NPV of = 456.40 Mill. Eur. The same behaviour is visible for $nT = 50$, although the final NPV is greater (683.53 Mill. Eur), see blue line Figure 11b. In the second study, the positive difference in DCF from the revenues surpasses the associated extra investment costs from the additional 40 wind turbines considered. The significant increase in the number of WTs doubles the computing time, due to the large increase in the number of variables, selecting 50 WTs entails significantly more possible combinations of valid solutions.”

- Comment: Line 476: I’m not sure what you mean, but if it is the main question then I should. Can you be more specific and/or clarify?

- Response: The sentence is restated as



“...An interesting question is whether there is a larger NPV in between the bounds of WT number....”

- Comment: Fig. 11-13 would probably be more clear if combined and corresponding lines were plotted on the same axes

- Response: Thanks for the advice. The recommendation has been followed and the descriptive texts have correspondingly been adapted.

TECHNICAL CORRECTIONS

General

- Comment: There are many grammar and usage errors throughout. The manuscript should be carefully edited to address these concerns so the material will be more accessible and useful to the community. I have noted a few of these below.

- Response: We have conducted a thorough review of it to improve the quality of the manuscript.

1: Introduction

- Comment: Line 13: “Subsidy-free . . . ” check grammar

- Response: Done.

- Comment: Line 19: Because you give a list of parts here (rather than just one primary thing), “consists of” may be more appropriate.

- Response: Done. Thanks for the advice.

2: Physics Modelling

- Comment: Line 132-133: comma after AEP

- Response: Done. Thanks for the advice.

- Comment: Eq. (8): this piece-wise equation contains multiple definitions for some cross-over points. Check the usage of “<” vs “<=”

- Response: Thanks for noticing this typo. The authors had seen it in advance, and it has been corrected.

3: Optimization Models

- Comment: Line 145: check commas to ensure clarity

- Response: Done. Thanks for the advice.



- Comment: Line 200-203: check grammar and usage to ensure clarity

- Response: Done. See full paragraph.

...Albeit the formulation of Sect. 3.1 represents to a very large extent the physics ruling the problem, it has a considerable number of variables and constraints that may hinder

the capacity to tackle larger problems. **The model presented in this section neglects power curve and AEP calculation and aims at simplifying the power-curve-based version.**"

- Comment: Line 205: does "this" refer to the linear superposition model or the powercurve free model? In general, try to avoid "this" where there is any possibility of misinterpretation.

- Response: Thanks for the advice. Corrected as

"...However, **the power-curve-free model** does not support the application of Eq. (4)..."

4: Neighborhood Search Heuristic

- Comment: Line 243: observation should be singular

- Response: Thanks for noticing this typo. It has been corrected.

5: Computational Experiments

- Comment: Fig. 4: This figure is a little busy, consider giving the figures a little more space by removing all unnecessary elements and adding some buffer space between sub-figures and figure elements. I really like this figure overall though.

- Response: Thanks for the advice. The AEP units in this figure has been changed to GWh and a buffer space between the top sub-figures has been added as well.

- Comment: Line 276: radii

- Response: Thanks for noticing this typo. It has been corrected.

- Comment: Line 412: I suggest avoiding starting a paragraph with "As shown in Fig. x" because we don't even know what the subject of the paragraph is yet. The "as shown. . ." should fit well at the end of the sentence.

- Response: Thanks for the advice. It has been corrected and checked throughout the manuscript.

- Comment: Figures in general: – The units given sometimes lead to very large numbers that clutter the figure and impede interpretation. I'd suggest using units that reduce the number of digits required in the tick labels (i.e. GWh instead of MWh, and hours or days instead of seconds)



- Response: For Figure 4 and Figure 11 this comment is particularly useful and it has been applied. For Figures 5, 7, and 9 the ordinate units has been changed to GWh. The abscissa units (s) has been kept according to the needs of the descriptive text.

Best wishes,

Juan-Andrés Pérez-Rúa
Mathias Stolpe
Nicolaos A. Cutululis