Reply to reviewers and community comments

July 15, 2026

We would like to thank the reviewers for their feedback and suggestions to improve the article. We have copied the feedback below and added answers in blue. A track changes article is added as a separate document.

1 Reviewer 1

Overview:

van der Laan and Watson have provided a careful and technically rigorous critique of Simão Ferreira et al. (2026) by clarifying the interpretation and the methodological robustness of the proposed "theoretical limit" for offshore wind farm capacity factors. Van der Laan and Watson raise substantive concerns that significantly affect how the original results should be understood and applied.

Most importantly, van der Laan & Watson (vdLW) argue that Simão Ferreira et al.'s (SFLS's) work cannot be considered a theoretical or universal bound analogous to the Betz limit. I strongly agree with vdLW that "the proposed limit ... is not a theoretical limit ... but a model limit ...." The SFLS derived capacity factor limit depends explicitly on modeling assumptions (turbine spacing, the treatment of finite wind farm effects). The language used by SFLS risks overstating the generality and physical inevitability of their result. More precise language would be "a model dependent upper bound under specific simplifying assumptions" rather than a "theoretical limit" which implies a fundamental constraint on wind energy extraction.

vdLW carefully examine the finite wind farm correction of SFLS and show that the correction method is ad hoc and insufficiently documented, that it is not reproducible without subjective interpretation. vdLW give a good-faith effort to develop (and document, and provide the python code via zenodo) an effort to provide an automated method to replicate the SFLS approach and demonstrate the insufficiencies of that method, further emphasizing that the SFLS approach is not reproducible. Further, they show clearly in Figs 5 and 6 that the SFLS model is highly sensitive to this parameter. I recommend this important comment for publication in Wind Energy Science.

Major comments:

1. My main criticism of vdLW is that this comment is not critical enough of the overall premise of SFLS. Wind farm performance cannot be reduced to a single deterministic relationship without careful treatment of atmospheric variability and spatial heterogeneity; this point is not made in the carefully circumscribed comment.
   This is good point. We have added: *It is well known that wake losses are strongly dependent on atmospheric conditions, such as ambient turbulence intensity and atmospheric stability Porté-Agel et al. (2020). Therefore, one can argue that the collapse of the wake losses into a single variable, ϕ, may not be possible*

2. vdLW correctly point out that SFLS's validation of capacity factors not only includes wake effects, but also grid losses, curtailment, and turbine availability losses (lines 187-190), but they could emphasize more clearly the impact of incorporating all these other losses into what SFLS label as wake effects.
   We fully agree and we have now added this as one of our main concerns in the introduction: *The model validation is performed with measurements of net wind farm capacity factors. These measurements also include losses related to grid faults, curtailment, turbine availability, etc., which makes it impossible to isolate wake losses for model validation*

3. vdLW do not comment on the implication of the large scatter in their Fig 6, which it seems might actually suggest that a more rigorous (or at least reproducible) approach than that given by SFLS would imply even more losses, which might argue that national policies overestimate possible capacity by an even larger margin. I recommend that they address this possibility in their conclusions, rather than simply saying that national policies are "difficult to assess."
The large scatter is a result of the large sensitivity of the finite wind farm correction factor. We do not think we can make the conclusion that the large spread automatically means larger losses. Note that we have changed *difficult to assess* to *impossible to assess*.

Minor comments:

1. Figure 2 has both red and green, which would make the bars difficult for colorblind readers. I ran the image through the WES-recommended https://www.color-blindness.com/coblis-color-blindness-simulator/ and recommend they choose different colors.
Thank you for pointing this out. We have now used a more colorblind-friendly color scheme.

2 Reviewer 2

The authors present a comment on earlier work by Simão Ferreira et al. on a possible theoretical limit for offshore wind energy extraction. The authors express some strong reservations regarding the validity of the claims presented earlier by Simão Ferreira et al., and provide compelling evidence for these. I believe that the manuscript presented is important, in particular given the impact that the earlier work seems to have had (e.g. influencing Dutch policy). I believe the manuscript should be accepted eventually for WES, but have formulated some comments and suggestions below

1. Page 2, line 26: "the proposed limit of the capacity factor... " This is maybe a bit philosophical, but possibly apart from the field of mathematics, all 'theoretical' limits in physics are based somehow on models. The difference is rather the validity/accuracy of the models used to construct the limit, and maybe it makes more sense to directly formulate it in these type of terms. In addition, the term 'model limit' is ambiguous and could point to a limit of a model (e.g. in its range of applicability) rather than a 'theoretical' limit on a physical process that was obtained from a model.
To our understanding, a theoretical limit is always based on fundamental physics, such as the speed of light, while the limit introduced by Simão Ferreira et al. (2026) is not. Following the reviewer's suggestion, we have now rephrased *model limit* at several places. For example, in the abstract we have changed this to: *a limit that is strongly dependent on the finite wind farm correction.*

2. Page 2, line 29: "The plotted limit labelled as theoretical limit.... represents a normalized gross annual energy production (AEP), but this is not clear from the paper". I'm not sure what your point is here, nor whether this is a criticism or a clarifying comment to the earlier work. Moreover, as far as I see, CF is by definition a normalized AEP?
Simão Ferreira et al. (2026) write in their figure legends *theoretical limit*, which we find misleading, because it simply represents the normalized gross AEP. Therefore, we clarify this in our comment article.

3. Page 2, line 77: "The equation for $C_{f,0}$, is the analytical limit that Simão Ferreira et al (2026) propose, though this is not clearly stated in their work." Looking at the work of Simão Ferreira et al (2026), I do not believe this is correct. Their "theoretical limit" is obtained using $x = \phi = U_r/(U_0\varepsilon)$ with ε the estimated finite wind-farm losses (see discussion on page 2 and 3 of their paper). A more obvious (possibly well-known/trivial in the resource assessment world) theoretical upper limit would be the evaluation of equation 3 given $x = \phi = U_r/U_0$, so using $\varepsilon = 1$. This simply means that we do not expect turbines or farms to be able to speed up the flow while still extracting energy. Given a wind distribution and rated wind speed, the CF naturally follows from the rated power (and C_P). I believe it would be relevant to make this point more explicitly, and possibly add a curve to the figures that corresponds to this more obvious theoretical maximum.
We think the reviewer misunderstands the *theoretical limit* line. We have noticed that is common

misunderstanding of the readers of Simão Ferreira et al. (2026). The limit line is plotted by evaluating the equation for $C_{f,0}(x)$ (Eq. 3) for $x = 1$ to $x = 2$ and plotted as $C_{f,0}$ vs x. Hence, the limit line is exactly obtained for $\varepsilon = 1$. The only place where the wake losses are used is in the determination of the x-location for the specific wind farm data points, as discussed in Sect. 5 using Fig. 5. We have added the following in Sect. 2 after introducing ϕ: *An important realization is that for varepsilon* $= 1$, *we obtain the normalized gross wind farm AEP limit that is referred to as the theoretical limit in Simão Ferreira et al. (2026), which is often misunderstood by readers of this work.* If the reviewer is not convinced, then we suggest that the reviewer makes the plot because this is how we realized it ourselves. Note that the only way to get different limit lines is to change the Weibull shape parameter k, as also shown by Simão Ferreira et al. (2026), Fig. 13. However, Simão Ferreira et al. (2026) use a constant $k = 2.4$ for their main results.

4. Page 4, line 87: the assumption of constant thrust coefficient in the Frandsen model is problematic for above rated conditions. Above rated, CT drops with wind speed, so that the infinite wind-farm loss in Eq 4 should gradually approach one. This another major issue in the model that could be pointed out more explicitly
   We fully agree and we have added: *It should be noted that the use of constant thrust and power coefficients are not realistic assumptions.* However, it turns out that the modeled wind farm capacity factor was calculated with cut-in and cut-out wind speeds, as clarified in Community Comment 1, meaning that also a model for the thrust coefficient above rated was employed. However, cut-in and cut-out wind speeds are not used when calculating the wind farm capacity factor that is used to calculate ϕ, which we do not understand. We have added this information at the end of Sect. 2.

5. The use of the Frandsen model is questionable, and the model is definitely not strongly validated. First of all, follow-up work by Calaf et al (Phys. Fluids 2010) has shown that the Frandsen models significantly underestimates the wind-farm induced surface roughness. More importantly though, both the Frandsen model and Calaf et al.'s extension are based on the assumption that the wind-turbines are in the surface layer of the boundary layer (so $z_h < 0.1H$ with H the BL height). Given z_h in the order of 200m for modern large turbines, this would require $H > 2000$m, which is rarely the case for offshore conditions (because of free atmosphere stratification effects). In practice, wind-farm blockage will start playing an important effect – this could or could not be more detrimental the power reduction predicted by the Frandsen model. However, the main point here is that the use of the Frandsen model is extremely heuristic at the least, and far from it's expected range of validity
   These are good points. We have added the following: *Furthermore, alternative infinite wind farm models exists, as, for example, the model of Calaf et al. (2010), who suggested a modification to the model of Frandsen (1992), based on large-eddy simulations of infinite wind farms using finite wind farms with lateral periodic boundary conditions. These infinite wind farm models rely on a geostrophic draw law that is commonly expressed by two constants (A and B), which can be shown to depend on atmospheric conditions as atmospheric stability (Floors et al., 2023), boundary layer height (van der Laan et al., 2020) and capping inversion (Liu et al., 2021).*

6. In my opinion, the choice of $f_{loss} = 0.9$ is very poorly documented (Simão Ferreira et al. point to a technical report by Mortensen et al., 2015, which only provides some very vague estimates, with 10% being the upper range of the losses). This could be better discussed
   We agree and we have added *The model validation is performed with measurements of net wind farm capacity factors. These measurements also include losses related to grid faults, curtailment, turbine availability, etc., which makes it impossible to isolate wake losses for model validation. Simão Ferreira et al. (2026) suggested an additional loss factor of 0.9 to account for this but they do not provide compelling evidence for their chosen value.* in the introduction.

7. Line 212: "While the model ... and infinite wind-farm wake losses". The model can provide an upper limit for no wake losses (this was possibly not pointed out by the original authors), but it cannot provide a limit for the infinite wind-farm cases (cf. point 5 above)
   We have added *using a simple model* to highlight that these bounds are model dependent.

Minor:

1. Abstract: "wake loss of an infinite wind farm" → "turbine wake losses in" (to avoid ambiguity with farm wakes which is another hot topic of research nowadays)
   Adopted.

2. Page 2, last line: analytical model cannot account for → does not account for
   Adopted.

3. page 3, line 75: define A. At this point, it may be useful to directly also equate x to U_r/U_0
   This a typo, A should be λ.

4. line 100: wake loss for a finite wind farm → (cf. minor point 1) + I would expect the wake loss to represent a power loss (rather than a velocity loss as implied by the definition of epsilon)
   We understand what the reviewer mean but decided not add information regarding this.

5. line 120 - $M_{rows} = 2.5$. What does this mean?
   We do not fully understand where this comes from. We have added: *It is not clear where $M_{\mathrm{rows}} = 2.5$ is based on.*. Note that the original authors have reduced this 2.5 for small wind farms, in CC1 and CC4, which we have noted in our article.

6. Line 141: "and this is related to the fact that a concave polygon is mathematically undefined". Not sure what you mean by this, and if so, how is step 1 and the algorithm on line 125 then well posed?
   The problem is that a concave polygon requires an additional parameter meaning the number of edge turbines is not unique. We have added/modified: *It is clear that the polygon does not find all edge turbines at the eastern side of the farm, and this is related to the fact that a concave polygon requires an additional parameter that determines how tightly the polygon follows the wind farm layout shape, meaning that the number of edge turbines is not unique and user dependent.*

7. Line 185: define A_w
   This is a typo and should be λ.

3 Reviewer 3

The manuscript under review presents a comment on a publication by Simao Ferreira et al. published earlier in 2026 that raised some attention also outside the academic community. Van der Laan and Watson – i.e., the authors of the comment – raise and elaborate on a number of concerns that may highly impact the interpretation of the findings from the earlier study. Overall, I believe, it is very relevant to raise these concerns and discuss them at a proper level in the community. The public discussion in Wind Energy Science confirms this but also shows that the comment may not be complete (and without shortcomings and misinterpretations) as commented by one of the authors of the original work. Considering this and several more structural shortcomings (see below), I am of the opinion that the "comment" under review should not be published in its current form and therefor suggest a rejection. This is not to stop or hinder the critical discussion of the previous study but rather to ask the authors to find a more comprehensive and diligent way of doing so.

We believe that the reviewer has been influenced by the Community Comment 1 (CC1), which suggests that our concerns are misunderstandings, but this is *not* the case. The valid misunderstanding the CC1 point out are mismatches in input parameters that were *not* well defined in the original work; however, adjusting these input parameters do not change our main conclusions. In addition, CC1 claims to have solved the issue of the non-reproducibility of the ad-hoc finite wind farm correction factor; however, we have added their updated results in our revised article to show that the original authors still cannot replicate their original results of the ad-hoc finite wind farm correction factor (see Fig. 6 in the revised article.

Specific review comments:

1. [rather a formal aspect] Checking the acceptable manuscript types for WES (`https://www.wind-energy-science.net/about/manuscript_types.html`), I would expect a peer-reviewed comment only as a comment to a paper published in the same journal. As the two journals in our case seem to differ quite a lot in various aspects, I believe another manuscript type might be more appropriate here (e.g., a research

article repeating more of the original work and describing the made amendments in event more detail, or a brief communication focusing on the impact of the previous study it is suggested shortcomings.)
We understand the reviewer's concern. Our main reason for submitting a comment article in WES is that we can have an open discussion, which does not exists for the journal of the original article. Furthermore, our main audience is based on the readership WES. Note that we discussed carefully with the editor whether we could publish a comment article in WES about an article in a different journal. The WES editor in chief agreed that we could.

2. [abstract] The abstract is very short and mentions "a number of concerns" and the single conclusion on the previously proposed "theoretical limit" only being "a model limit". I'd suggest that the authors detail this a little further – e.g., if this is the conclusion, what is their specific suggestion / next step? Is the previous study shown to be wrong or the results just associated with a higher uncertainty? The authors need to be more specific here.
We have modified the abstract including a stronger focus on the non-reproducibility of the finite wind farm correction factor. We also added: *Therefore, the model of Simão Ferreira et al. (2026) cannot be employed to assess the feasibility of national policies.*

3. [l 16] Interviews regarding the previous findings are mentioned, and it seems that the publicity around the original study has been the driver for the authors to draft their comment. Though this seems very obvious, it is not mentioned explicitly in the manuscript. I'd suggest that the authors are more specific on this aspect.
This is a correct observation of the reviewer. We have now added the following on the introduction: *Hence, the work of Simão Ferreira et al. (2026) has had a major political impact in the Netherlands.*

4. [l 19] "complex" (in "complex models") is not specified properly here, but it seems that the authors refer to mesoscale models like WRF as a better truth. This should be further detailed here – not least because also mesoscale models / simulations depend on specific model assumptions and wind farm parametrizations that should not be considered as a (high-level) standard solution.
We have replaced *complex* with *numerical*.

5. [p 2] The specific concerns are introduced in detail before introducing the basic details of the model and its application. This is not optimal as it requires the reader to have read the original study in great detail and depth beforehand. I recommend to the authors to find a better way of introducing their list of concerns.
We can understand the reviewer's concern; however, it would require a very long introduction if we would explain all the necessary information to understand the listed concerns. Also note that this is a comment article. In our opinion, it does not make sense to read our comment article without having read the original article.

6. [l 63] k=2.4 is mentioned here, but it is only stated in l 72 that Simao Feirrera et al. used this single value for k in their study. This detail should already be mentioned at the first location.
This is a good point, we have added the following: *Simão Ferreira et al. (2026) employed a constant Weibull shape parameter ($k = 2.4$) for simplicity, while the Global Wind Atlas 4.0 (Davis et al., 2023) suggests that k varies between 2.0 and 2.6 for the 72 offshore wind farms..*

7. [Figure 3 / 4] For both figures, "model results" should be specified further – which model here?
We have clarified this to: *model results of Simão Ferreira et al. (2026).*

8. [ll 192] The sentence lists four wind farms but states them in pairs (which becomes clear when reading the sentence twice) – this is a bit confusing. I'd suggest to first list all four wind farm, and then explain the issue about the pairs.
We have modified this to: *We disregard results of four wind farms: Baltic 1, Baltic 2, Princess Amalia and Luchterduinen, because their measured capacity factors were only available in pairs (Baltic 1 – Baltic 2 and Princess Amalia – Luchterduinen), not individually..*

9. [Figure 5] It is very hard to differentiate between red and pink symbols – please use another colour.
We have replaced pink with cyan. Also note that the cyan results now represent the output of CC4, as explained previously.

10. [l 204] “possibly due to differences in model implementation” is a very vague formulation – the authors should be more specific about their assumption / suspicion here.
This has now been resolved, since we get the same results as Simão Ferreira et al. (2026) when using the same input variables, as shown in the added appendix.

11. [l 214] The statement “makes it impossible to draw strong conclusions about a limit for finite wind farms” seems key to the comment and requires further elaboration. I understand this is linked to the observed scatter – but where should we draw a line between possible / impossible? I do not say that I disagree with the authors here, but I strongly suggest to be more explicitly here and give more reasoning.
The full sentence already reads *The large sensitivity of N_{free}, as shown in Figs. 5 and 6, makes it impossible to draw strong conclusions about a limit for finite wind farms.*, which we think is clear enough.

12. [ll 224] The statement in the conclusions “it is not trivial to use ..” is gain a hint that needs further elaboration. Generally, I feel, the authors must find a better balance between criticizing the original study for their approach in the interpretation of the results and correcting the technical approach behind that.
This sentence has been modified following Reviewer 1, Comment 3.

4 Community Comments 1 and 4

The original authors have added community comments (CC) as a reply to our comment article, CC1 (Sørensen et al., 2026) and CC4 (Simão Ferreira, 2026) (CC2 and CC3 are just copies of CC1). CC1 is a 49 page document and CC4 is a script used to generate the figures in CC1. With this information, the original model results of the wind farm capacity factor and wind farm wind factor can be reproduced (using fixed values of the finite wind farm correction factor). The original authors have further developed our automated method for obtaining the finite wind farm correction factor. However, the original authors cannot reproduce their own results of the ad-hoc finite wind farm correction factor, which means that our main concern with the original work of Simão Ferreira et al. (2026) remains unresolved. Rather than copying the 49 pages of CC1 here, and reply to every detail, we choose to discuss the main points and changes of our revised version.

4.1 Adaptations

We appreciate the uploaded script from CC4 since it is now possible to replicate the original results of the modeled wind farm capacity factor and the wind farm wind factor. The original authors label the discrepancies as errors of our comment article. However, these errors originate from the fact that the original work did not list all the input variables correctly, nor a script was made available by the original authors. In our revised version, we have now changed the following:

1. Employ wind farm dependent latitude to calculate the Coriolis parameter instead of fixed latitude of 55° (This has a minor impact as shown by CC1).

2. Correct the Weibull scale parameter to hub height. We initially understood that this was already done in the provided input from the original authors.

3. Instead of using the wind farm rated power provided by the original authors, we now calculate the wind farm rated power by multiplying the number of wind turbines with the turbine rated power. For example, for the Arkona wind farm, the provided wind farm rated power was 385 MW, while it consists of 60 6 MW turbines (360 MW).

4. Employ cut-in and cut-out wind speeds of 3 and 25 ms^{-1}. By inspection of the uploaded script from CC4, we now understand that the wind farm wind factor does not include the cut-in and cut-out wind speed by first calculating a wind farm capacity factor without cut-in and cut-out wind speeds and then solving for the wind farm wind factor using Eq. 3. This was not clarified in CC1. We have made a clarification in the revised comment article.

5. We had used the ϵ_2 parameter as implemented in PyWake v2.6.18 following the work of Sørensen and Larsen (2021) (Eq. A14). We have now added the formulation of ϵ_2 as suggested by CC1 and CC4 in order to obtain the same results as Simão Ferreira et al. (2026). We understand that newer PyWake versions now use ϵ_2 as used by CC4.

6. We have added an appendix to verify that our script can reproduce the original results (Table S1 of Simão Ferreira et al. (2026)) up to the expected rounding error.

7. We have added the missing neighboring wind farms Borssele Kavel III and IV and Borssele Kavel V for the automated script. These wind farms were missing because we only searched for neighboring wind farms using the original wind farm database consisting of 72 wind farms.

8. We have added results from CC4 regarding the finite wind farm correction factor (Fig. 2), and resulting model wind farm capacity factor (Fig. 6) and wind farm wind factor (Fig. 7). These results show that CC4 does not resolve the main issue regarding the non-reproducibility of the ad-hoc finite wind farm correction factor used in Simão Ferreira et al. (2026).

CC4 includes many changes to the finite wind farm correction factor originally detailed in their work and some of these should be considered as new work rather than as a clarification. It should be noted that a script for calculating the finite wind farm correction factor did not exist prior to our comment article. The script of CC4 is based on our initial attempt to replicate the ad-hoc and undocumented method of Simão Ferreira et al. (2026). We have adopted the following changes of CC1 and CC4:

1. Adaption for annual variability of upstream wind farms. This is a logical step that we did not take into account, which we have now adopted. The wind farms that have a temporal dependency for more than 10% of the extracted annual data are: Horns Rev 2, Gode 1 and 2, Thortonbank, Northwind, Nobelwind, Belwind Phase 1, Greater Gabbard, Lynn and Inner Dowsing, Hornsea 1 and Beatrice extension; a total of 10 out of 68 wind farms.

2. Update of alpha parameter of four wind farms to better find the edge turbines: Northwind, Rampion, Rentel and Robin Rigg. We have adopted this. As we already discussed in the comment article, the number of edge turbines is not unique for concave wind farm layouts, and this makes it difficult to apply an automated method to identify them.

3. We have added the results of the finite wind farm correction generated by CC4 in our comment paper, showing that we still do not obtain the originally published results of the ad-hoc correction factor.

We have not adopted the following changes of CC1 and CC4:

1. Modification of the constant $M_{\mathrm{rows}} = 2.5$ to a lower number for small wind farms that have less than 25 turbines (Albatros: 1.37, Alpha Ventus: 1.21, Northwester 2: 1.60). The modification for small wind farms reads: $M_{\mathrm{rows}} = \min(0.5[0.5788(\sqrt{N_{\mathrm{wt}}} - 1) + 1], 2.5)$, which gives a value in between 0.5 and 1.66. It unclear what this is based on and where 0.5788 comes from.

2. Application of special treatment to several wind farms with complex wind farm layouts. Split of Gemini and Thorntonbank to avoid large gaps. Split of Belwind and Nobelwind, and migration of a turbine row to Northwester 2 to avoid intertwined wind farm layouts. We have chosen to not adapt this because how would one use such a method for newly planned wind farms that have complex wind farm layouts?

3. Increase of the number of wind direction sectors from 12 to 36. We find that this is a minor influence. Furthermore, the wind resource only has a resolution of 30°.

4.2 Main discussion

1. CC1 includes a lot of statements that criticize our automated method for obtaining the finite wind farm correction. However, we do not recommend to use this method at all. Our main aim was to attempt to reproduce the reported wind farm finite correction factor with the limited information provided by Simão Ferreira et al. (2026) and their non-peer reviewed document Simão Ferreira (2024). Our

main conclusion is that we are not able to reproduce the ad-hoc finite wind farm correction factor of Simão Ferreira et al. (2026). Furthermore, the original authors can neither reproduce their own ad-hoc finite wind farm correction factor (results of CC4, which are now shown in the revised version of our comment article).

2. CC1 states that the gray horizontal error bars in Fig. 5 are not realistic measures of the model uncertainty. However, the original work and CC1 does not provide any proof of what the uncertainty is. Since the results of the ad-hoc finite wind farm correction factor cannot be reproduced, the gray horizontal errors bars are an honest representation of the model uncertainty.

3. CC1 claims that their limit of offshore wind farm energy extraction is analogous to the Betz limit (maximum power coefficient of a single turbine following actuator disk theory) corrected for finite blades and tip losses. We do not agree. First of all, the Betz limit has been proven to be hard limit for conventional wind turbines while there exists several different infinite wind farm wake models (Frandsen (1992), Calaf et al. (2010), etc.), which also rely on a geostrophic draw law including constants (A and B) that depend on atmospheric conditions, see for example Floors et al. (2023), van der Laan et al. (2020) and Liu et al. (2021). Secondly, the corrections applied to actuator disk models are based on scientific arguments, while the finite wind farm correction proposed in Simão Ferreira et al. (2026) is not. Furthermore, Simão Ferreira et al. (2026) do not provide a method to apply their finite wind farm correction to unseen wind farm layouts, especially when considering neighboring wind farms. They neither discuss at what distance a neighboring wind farm should be taking into account.

4. CC1 highlights a number of validation cases for Lillgrund, Røsand II and Horns Rev 3 offshore wind farms, showing good results (Appendix C of CC1). These are only 3 of 72 wind farms that Sørensen et al. (2026) have investigated. When modeled vs the measured wind farm capacity factor is plotted for the entire dataset using the finite wind farm correction from CC4, we do not get a good match, namely an r^2 value of 0.69, as shown in our revised comment article (Fig. 6c, also shown below in this document). Furthermore, the three wind farm results shown in CC1 are generated with an updated model (Sørensen et al. (2026)) that uses an engineering wake model database to calculate the finite wind farm factor. This is completely different from the original work of Simão Ferreira et al. (2026)).

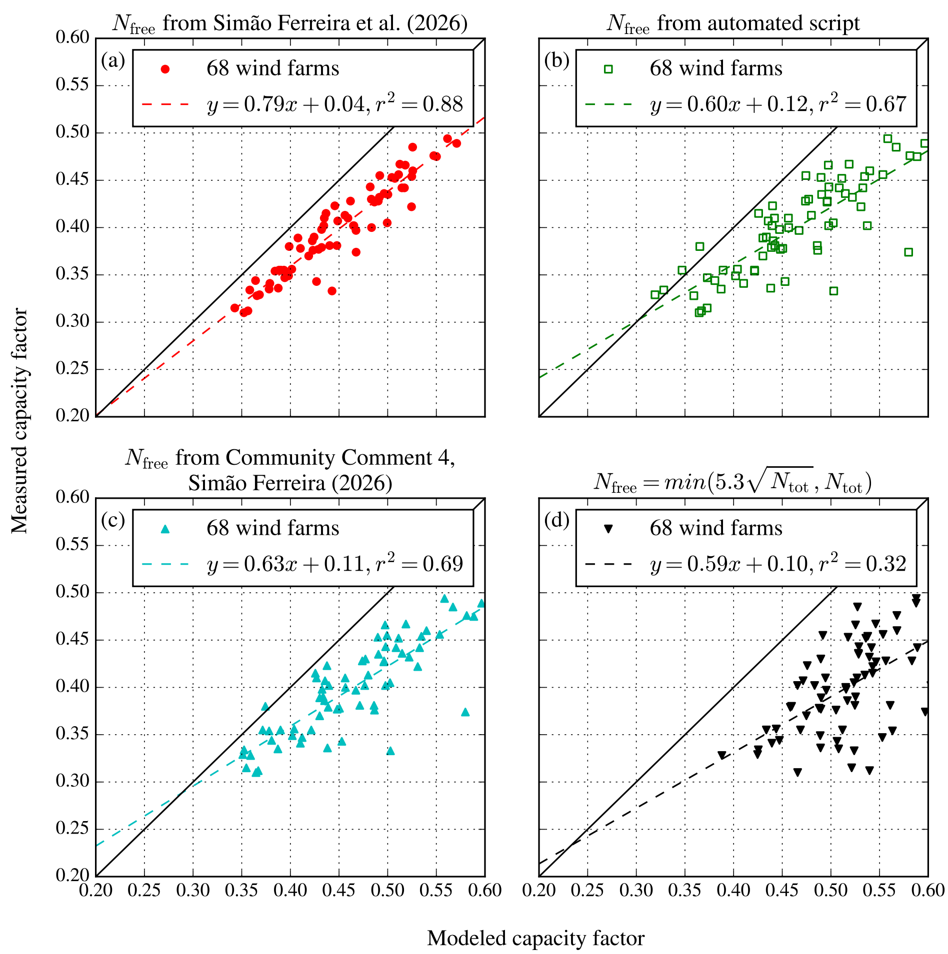

Figure 1: Measured vs modeled capacity factor as using different models for N_{free} compared to Simão Ferreira et al. (2026).

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