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**MS type: Research article**

**Title: A semi-empirical model for near sea surface wind speed deficits downstream of offshore wind parks fitted to satellite synthetic aperture radar measurements**

**Journal: Wind Energy Science**

### **Answers to the reviewers**

#### *General comments to both two reviewers*

*We would like to thank the editor and both reviewers for their continued effort and for the constructive feedback on our revised manuscript. We appreciate that the first round helped improve the clarity and overall presentation, and we have carefully considered the remaining points raised in the second-round review.*

*Below we provide a detailed, point-by-point response to all comments. Reviewers' comments are reproduced in black, and our responses are given in blue italics. For transparency, we also indicate precisely where changes were made in the new revised manuscript (page and line numbers).*

*Beside the current file containing the answers to the reviewers, two additional pdf files are attached. One refers to the new pdf version of the paper after taking into account the reviewers' comments. The last pdf file is the file that highlights all the changes that were made to the first submitted version (difference between the revised version and old version of the paper). In the latter file, the changes are characterised by the underlined blue text, which is the new correct text and the strikethrough text in red, which is the text that has been removed.*

### **Reviewer N#1**

Thank you for considering my earlier comments and for the clear improvements to the manuscript's readability and organization. That said, I still have some reservations about the physical consistency of the wake2sea model, and several of my original concerns remain. I offer the comments below in a constructive spirit, with the hope that they help further strengthen the work.

Following their reply regarding overfitting the SAR data, I agree with the authors that with 30 SAR images, the information content is far greater than the eight control parameters, so classical

overfitting is unlikely. My earlier point concerned model adequacy not overfitting. A physically inaccurate model can still match the dataset well, and the large disparity between the dataset's degrees of freedom and the model's parameter count implies a non-unique inversion: multiple parameter sets and alternative model forms could achieve similarly good fits. Consequently, a close match to the SAR images does not, on its own, establish that the wake2sea model is physically correct.

*We thank the reviewer for the thoughtful and constructive feedback. To address this concern, complementary independent validation using additional datasets (flight campaign observations) were added in the new Section 5.2. These validation efforts directly address model adequacy and are described in detail in response to the following comments. In addition, we would like to emphasize that we tried to develop a semi-empirical model, which, by definition, cannot be expected to replicate all physical aspects perfectly. The approach is somewhere between rigorous physical models following all the conservation laws in the 3D dynamics of the boundary layer and machine learning techniques, which are purely data driven. We decided to take this route because with our model there is full transparency about the structure, the simplifications and the resulting estimated parameters. Together with the fact that the model replicates the basic advection-diffusion character of the “real” system (associated with the conservation of momentum), we think that some users may prefer this approach to pure machine learning methods.*

As a follow-up to an earlier comment, I am afraid the manuscript still does not present an independent verification of the wake2sea model. The FINO-1 comparison is introduced as “an independent validation” (line 368); however, as noted previously, the FINO-1 site was included in the inversion used to tune the model parameters. This makes the FINO-1 results effectively in-sample, except for how the measured wind speed is interpolated to a 10-m altitude, so they cannot serve as an external check of model performance. Validating the wake2sea model against data from a different region can strengthen confidence in the model approximations used.

*We thank the reviewer for reiterating this important point and we agree that independent verification is essential. We apologize that our wording may have led to a misunderstanding. No FINO-1 measurements were used in the inversion/training or in any tuning of the Wake2Sea parameters. The inversion relies on the SAR-based observations (via the NRCS forward operator) together with background information; FINO-1 does not enter the cost function and is not assimilated. The FINO-1 platform is located inside the spatial domain of the study, but this does not imply that FINO-1 measurements were used for tuning or calibration and hence we consider FINO-1 data as independent information suitable for validation.*

*To remove any ambiguity, we revised the text at the beginning of Section 5.3 (around former line 368) to state explicitly that FINO-1 data are used only a posteriori for evaluation, and we adjusted the phrasing from “independent validation” to “independent in situ evaluation (not used for inversion/training)” to make the distinction clear:*

*“We emphasize that FINO-1 wind measurements are not assimilated and were not used to tune any Wake2Sea parameters; they are used only for an independent in situ evaluation of the results.”*

*In addition, we strengthen the manuscript by adding independent validation using airborne campaign measurements (not used in training/inversion) in the new Section 5.2.*

I appreciate the introduction of the parameter  $\alpha_{\chi}$  to distinguish conditions at the top and middle of the layer; however, its role remains somewhat opaque as it is not further discussed in the manuscript (or perhaps I missed it somehow). A related point is the consistency between Eqs. (2) and (3). Equation (2) parameterizes the wind-speed profile across the layer using  $\alpha_{\chi}$ , whereas Eq. (3) assumes a power law with an exponent  $E=0.1$ . Could you please clarify why Eq. (3) was not used to simplify the vertical gradient of the wind-speed profile if the equation is assumed to be valid within the considered layer? Later in the manuscript, the profile at FINO-1 is treated with a log-law interpolation. This mix of  $\alpha_{\chi}$  profile, power-law, and log-law treatments makes it hard to track which profile governs which part of the analysis and what uncertainty this introduces.

*We thank the reviewer for this helpful comment. We agree that the current manuscript can give the impression that multiple vertical-profile assumptions are used interchangeably, and we will clarify this explicitly. It is important to emphasize that the three approximations used for the vertical wind speed profile are applied in different contexts, which are only weakly interlinked.*

- 1. Eq. 2 is basically to explain why the simple sink term  $-\chi U$  can be interpreted as a vertical diffusion term, i.e. it illustrates that the origin of the applied 2D model is in the 3D dynamics*
- 2. Eq. 3 is supposed to translate the “first guess” U10 wind fields into first guess vertical mean wind speeds, which are then used for the internal calculations. As these “background” wind fields are corrected as part of the inversion process anyway, the details of this transformation is therefore not critical. Please also see the comment from reviewer 2 about eq. 3.*
- 3. The log-profile in combination with the Charnock relation used in Section 5.2. is used to relate the derived u10 wind speeds to the FINO-1 measurements at 30 m height. As*

*discussed in the text, this is not trivial because one must expect additional mixing caused by the wind turbines and this effect is very hard to quantify.*

*We have added text to explain the nature and the purpose of these parameterisations better.*

Line 384: “one can now argue that the assumption of a neutral boundary layer is not realistic, particularly inside a wind-park cluster.” I agree that strict neutrality is uncommon and that stability can materially affect wakes. However, attributing the full discrepancy to stability alone seems a strong assumption.

*It is important to point out that the bias caused by the missing windfarms in the DWD model data is significantly reduced under the assumption of neutrality as well under the assumption of additional mixing. The point we wanted to make here is that additional mixing does in fact reduce the bias even further although we do not have enough information to say exactly how much additional mixing is introduced by the OWFs. We added the following sentence to make this point clearer:*

*“We are not claiming that our procedure for introducing additional vertical mixing is highly accurate, but the analysis shows that the reasonable assumption of additional mixing leads to further reductions of the bias.”*

Section 6 explores limiting behaviors of farm-wake evolution and shape derived from the parameterizations in section 2. I appreciate the intent to build intuition, but I am unsure how these results relate to the rest of the manuscript. As presented, the discussion reflects properties of the chosen parameterization rather than the underlying physics of wake evolution and hence characterizes the adopted model. To strengthen confidence in the consistency of the underlying assumptions, it would help to validate the limiting behaviors in section 6 against higher-fidelity references (e.g., LES or mesoscale simulations).

*We thank the reviewer for this comment. The point of this study is to put observations into the centre of the analysis. It is well known that numerical models including wind turbines (mesoscale as well as LES) can have considerable errors as well and we are therefore reluctant to use such data as a reference. We tried to make this point clearer by adding the following sentence and an additional reference*

*“In this section we illustrate some properties of the Wake2Sea model with the parameters estimated from the satellite data set. We would like to emphasize again that we are not claiming that the model is dynamically consistent in all aspects, but that we are rather putting the observation information into the center in this study. In this context we point out that state-of-the-art mesoscale and LES models are far from perfect as well (Ouro et al., 2025).”*

*at the beginning of section 6.*

Additionally, the authors stated in their reply that ‘There is in fact very little known about the shape of the across wake profiles and it must be assumed that it depends on various environmental and installation parameters’. However, I do not agree with the authors that “very little is known”. There is now substantial LES literature on farm-wake dynamics such as (10.1017/jfm.2025.10320), and the references therein, which shows that wakes at the farm scale tend toward a more uniform (top-hat like) profile rather than a Gaussian shape characteristic of single-turbine wakes (see their Fig. 11).

*We thank the reviewer for the helpful suggestion. We repeated the computation with initial top-hat functions and the respective plots can be found in the new figures Fig11c and Fig11d in the revised manuscript. The solutions can still be expressed in closed form (eq. 35). We also added a reference to Lanzilao and Meyers (2025).*

I appreciate the authors’ effort to clarify scope by stating that wake2sea is suited for the German Bight. However, this raises a question about model accuracy. If the model correctly approximates the underlying wake physics, one expects it to transfer, perhaps with modest re-calibration, to other regions which are governed by the same physical laws. Conversely, if the model reproduces the German Bight cases but not beyond, this would limit its general utility and would naturally raise concerns about the correctness of the model's (wake2sea) underlying assumptions.

A minor point, there is an undefined reference in line 447 ‘... discussed in the previous subsection ??’.

*We thank the reviewer for the helpful suggestion. The correct reference to the subsection was added. Our opinion is that the “correctness” of a model is best demonstrated using observation data and this is basically the core aspect of this entire study. As the German Bight is the only area where we have access to independent validation data, we think it is an honest approach to say that this empirical model was tuned to the conditions in the German Bight and the applicability in other regions would require additional validation work, which is beyond the scope of this paper. We have made the validation with independent observation data even stronger by adding comparisons with flight campaign data in the new Section 5.2.*

I appreciate the progress made. Nevertheless, I still have concerns about some underlying assumptions and the model’s utility. I therefore recommend **major revision**. If feasible, please include an out-of-region, out-of-sample validation. Alternatively, or in addition, benchmark the calibrated wake2sea model against higher-fidelity references (e.g., LES or mesoscale simulations such as WRF) and report quantitative comparisons. This would substantially strengthen confidence in the model, as the current manuscript does not yet provide a direct validation for the wake2sea model.

*We thank the reviewer for the thoughtful and constructive feedback, and for highlighting the scientific value of using SAR data to improve wind wake modelling.*

*We agree that independent verification is required. In this revision we therefore added independent validation using airborne campaign measurements that were not used in the inversion. The new comparison (Section 5.2, Figure 10b,c) shows that Wake2Sea substantially improves agreement with airborne observations relative to the baseline DWD winds (bias reduced from  $-0.93$  m/s to  $-0.30$  m/s; RMSE reduced from  $1.26$  m/s to  $0.74$  m/s for  $n = 2,197,035$  collocated points). This provides an independent observational check of model performance beyond the SAR tuning set.*

*The new section now starts as:*

*“5.2 Independent validation of Wake2Sea with airborne campaign data*

*To provide an independent evaluation of Wake2Sea beyond the SAR-based inversion, we compare modeled 10 m wind speeds with airborne campaign measurements collected over and downstream of offshore wind farms. ....”*

We modified one sentence in the abstract

*“Comparisons with data measured at the fixed platform FINO-1 adjacent to the first German offshore wind park Alpha Ventus as well as with airborne campaign measurements showed that the proposed inclusion of wakes in the atmospheric model data leads to a significantly improved match.”*

and in the conclusions accordingly.

*“Similarly, the inclusion of wakes in the DWD data significantly improved the agreement with independent airborne campaign data acquired during the WIPAFF and X-Wakes projects.”*

## Reviewer N#2

The authors have adequately addressed the comments raised during the initial review. The revisions have significantly improved the clarity and coherence of the manuscript. The objectives of the study are now more clearly stated, making it easier to understand the scope of the work and its contribution to the field.

In addition, the structure of the paper has been reorganized in a meaningful way, resulting in a smoother reading experience and a more logical flow of ideas. The description of the model, in particular, is now clearer: the assumptions, variables, and methodological choices are presented in a more detailed and rigorous manner, which strengthens the approach.

Overall, the revised version satisfactorily addresses the initial concerns, and the manuscript has gained in both scientific quality and readability.

A few remaining remarks:

- L 69: a closing parenthesis seems to be missing

*This has been corrected – thanks.*

- L 94: You could emphasize your point by recalling that even though this formula (the power law) is simple, it is still a very common assumption in the offshore wind industry, and it is even supported by the standard

*We thank the reviewer for this helpful suggestion. We agree that, although the power-law profile in Eq. (3) is a simple representation of vertical shear, it is widely used in offshore wind engineering practice and is also referenced in wind-energy standards. We have therefore revised the text around Eq. (3) to explicitly state this.*

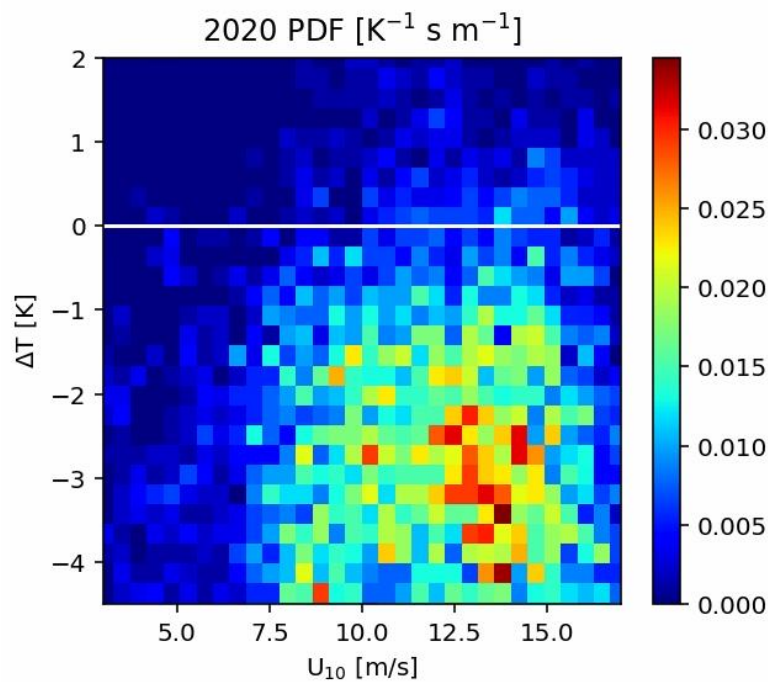
*“Although simple, the power-law wind profile in Eq. (3) is a widely used approximation in offshore wind engineering practice to represent near-surface shear.”*

- Table 2 could be moved to page 17 in Section 5 to make the reading easier

*We thank the reviewer for the helpful. The table 2 has been moved accordingly to improve readability.*

- In Section 5.1, it would be interesting to know if the 2020  $\Delta T$  and stability distributions match those from the training period.

*We thank the reviewer for the helpful suggestion. We agree this comparison would be informative. We have created the following PDF plot for 2020*



*It is qualitative very similar to Figure 2b, but more noisy because of the smaller amount of data. We have therefore decided against including it in the manuscript.*

- L 440: Mention “at sea surface” in the title.

*We thank the reviewer for the helpful suggestion. We have updated the title to explicitly include “at sea surface” accordingly.*

- In Section 6, numbering should be incremented for subsections.

*We thank the reviewer for pointing this out. It is corrected accordingly.*

- L 494: typo “??” to be replaced by the subsection number

*We thank the reviewer for pointing this out. Changes have been made accordingly.*

- L 516: This finding requires further elaboration, as its origin is not clear from the preceding sections.

*Sorry, this should have been better explained before. This comment refers to eq. 14 and we have added the following sentence in Section 5*

*“The values for  $\alpha_7$ ,  $\alpha_8$  indicate that the ratio of the deficit at 10 m height is about 36% of the deficit averaged over the first 200 m on average (see eq. 14 and eq. 11).”*

Some comments to extend the analysis in future work (not expect to be addressed in the current manuscript):

- It would be interesting to conduct a sensitivity study to quantify the impact of the number of SAR images used to train the wake model (Table 2)

*We thank the reviewer for this valuable suggestion. We agree that a sensitivity study on the number of SAR scenes used in the inversion (e.g., subsampling the training set and assessing parameter stability and performance) would provide useful guidance on data requirements and robustness. As this requires additional dedicated experiments beyond the scope of the present revision we have not been included here.*

- About the theoretical considerations in Section 6, do you think that your analytical analysis could be the same at hub height? Under which assumptions?

*Yes, the basic mechanisms, e.g. the wake expansion can be expected to be qualitatively the same, however the magnitude maybe different and this is really hard to quantify without resolving the boundary layer in more detail, which is challenging even if state-of-the art LES or mesoscale models are used (we added a reference to Ouro et al. [2025] following a comment from reviewer 1). We have added the following sentence in Section 6.0.2 to emphasize that the horizontal diffusion is an effect that can be expected to be relevant for the entire wake region including layers at hub height.*

*“The wake expansion has been addressed in previous studies, e.g. in Frandsen et al. [2006],, however this is the first time this effect is estimated for the near surface deficits using satellite observations.”*

- L 475: can this finding be checked on individual SAR images? Or compared to statistics retrieved from SAR images?

*Thanks for this comment. It is not easy to verify this from individual SAR scenes, because there are many factors (including the background variation of the wind field) interacting. However, there is no question that lateral diffusion is an important factor in wake dynamics (e.g. Frandsen et al. [2006]) and this statement is true for lateral diffusion processes in the most general sense.*