Review of “A semi-empirical model for near sea surface wind speed deficits downstream of offshore wind parks fitted to satellite synthetic aperture radar measurements” submitted for publication in Wind Energy Science.

The presented manuscript introduces a two-dimensional model for the estimation of near-surface wind speed due to the impacts of wind-farm wakes. The model applies data assimilation to a set of empirical relations coupled with a physics-based model of the near-surface atmosphere (hence the name semi-empirical) using Satellite Aperture Radar (SAR) images as reference. Validations of the optimized model are presented against, for instance, recordings from the FINO-1 met mast. The purpose of the model is to have a computationally less expensive tool than three-dimensional atmospheric models to estimate near-surface conditions impacted by wind-farm wakes, which has direct application in oceanography.

The manuscript clearly consists of two main parts: the physics model of the atmosphere, named "Wake2Sea", and the data assimilation technique. This review will mostly focus on the first part: the physics model, but suggestions to other parts will be included.

First, some general notes about the manuscript are presented. The arrangement of the figures and their corresponding text needs to be revised. Figures were typically a few pages ahead of their corresponding text, which makes it very hard for the reader not to lose focus while going back and forth. For instance, Fig. 5 is in page 10 and is first mentioned six pages after (page 16). Additionally, the mathematical variables used in both main parts of the manuscript are not well defined, which adds further difficulties to understand. The authors should consider a thorough description of all used variables. Additionally, the manuscript can benefit from polishing the text for better readability.

I have some concerns about the assumptions made to derive the physics model of the near-surface atmosphere. These concerns are detailed below. However, I wanted here to emphasize a critical point regarding models that fit to either high fidelity simulations or to measurements. When comparing fitted models to reference data, matching the reference data is not an indication of the solidness of the adopted physical model. A flawed physics model, if fitted properly, can match the reference data potentially leading to an overestimation of the model’s robustness. Applying the fitted model to conditions that were not included in its optimization process is typically a better test for the model’s accuracy. The Wake2Sea model was fitted to and tested against SAR images of the North Sea only, which does not actually measure the accuracy of the underlying physics assumptions. The same analogy holds for the comparison against the recordings of the FINO-1 mast, which is situated in the North Sea. This indicates that the near-surface wind conditions at the FINO-1 site were already included in the optimization of the Wake2Sea model. Hence, such comparison mainly examines the accuracy of the expression used to relate the wind speed at 10 m above the surface (used in the optimization process) to the wind speed at the recording height, rather than testing the physics accuracy of the Wake2Sea model.

Based on this introduction and on the comments listed below, I recommend **rejection** in its current form, as the manuscript does not yet meet the publication standards of Wind Energy Science.

The Wake2Sea model

Momentum extraction by the turbines was modelled using Fitch's parameterization. However, Fitch's parameterization represents the turbines as not only sinks of momentum, but also sources of turbulence. The role of turbine-induced turbulence remains unclear, given that its impact on the flow, particularly near-surface fluxes, is not negligible. If turbine-induced turbulence was not considered, it was not discussed what implications this assumption may have on the model's accuracy.

The derivation of Eq. 2 is unclear and would benefit from further clarification. The first assumption (setting $U_{-} \approx 0$) can be to some extent understood if the authors intended to say that the wind speed at the bottom of the considered atmospheric layer is much smaller than wind speeds at higher altitudes. However, setting $U = U_{+}$ needs further elaboration. While not mentioned, can I assume here that the authors intended to say that the vertical profile of wind speed becomes uniform starting from the middle of the considered layer, and hence does not change with higher altitude? If this is the case, which can roughly be pictured to represent an atmospheric boundary layer where the wind profile reaches geostrophic values, then the height of the considered layer should be at least twice the atmospheric boundary layer thickness. However, later in the article, the authors mention that the thickness of this layer is taken to be 200 m, which is much less than a typical atmospheric boundary layer height. The authors should elaborate more on the consistency of their assumptions regarding this part, particularly the consistency of the assumptions in Eq. 2 and the selection of the atmospheric layer thickness.

Line 94: “Changes in the pressure field introduced by OWFs are not considered”. Can this be supported from the literature by showing the typical distances downstream of a wind farm after which the farm’s impact on the pressure field becomes negligible?

Line 95: “The advection term for the deficit includes higher order terms, which were omitted to keep the numerical treatment simple”. It would be helpful if the authors first presented the higher-order terms and then provided justification for their omission. Terms can be neglected when they are considerably smaller than other dominant terms in an equation. This is typically done analytically using an order of magnitude analysis or numerically using a budget analysis of the considered equation. None of these approaches was included.

The authors assumed that the considered turbines in the North Sea area have the same thrust coefficient. I understand that finding such data for individual turbine types is not easy. However, neglecting such variations may impact the accuracy and generalizability of the Wake2Sea model.

Line 141: Setting the CFL number to unity is not a universal stability condition. While it holds for simple partial differential equations (e.g. a heat equation), for more complex PDEs this condition does not necessarily guarantee numerical stability. Can the authors comment on their choice of setting the CFL number to one? Some support from the literature would benefit.

Line 394: “If we assume that the across wake profile has a Gaussian shape at distance x = 0 km from the wind farm”. This assumption needs further justification. Wind-farm wakes do not become self-similar and follow a Gaussian profile just behind the wind farm.

Line 438: “The sink term also showed a slight dependency on the deficit itself with lower diffusion at higher deficits”. It is unclear how the sink term, which represents the momentum extracted by the wind farm would be dependent on one of its results which is the wind-speed deficit. Can the authors elaborate more?

Minor comments

The following are minor comments to be considered by the authors if they please.

- Fig 1a, b: Consider using a discrete colour bar instead, as it may enhance readability.
- The introduction section frequently refers to things that are yet to be introduced later in the manuscript. This may reduce the clarity for readers unfamiliar with the material.
- Lines 158-172: Can this be summarized? This part is very detailed, and not all these details are relevant to the main purpose of the manuscript.
- Lines 178-182: Presenting this section as bullet points may improve readability.
- In the caption of table A1, please briefly mention what is ΔT and U_{10}. Are they spatial averaged values or are they taken at the FINO-1 site as in Fig. 2?

Suggestions

I have a few suggestions to the authors to consider if they please.

- The underlying physics assumptions of the presented model needs to be thoroughly revised.
- A clear mention of the limitations of the proposed model would help the reader understand when it is suitable to use the model.
- Consider validating the model with SAR images for other sites than the North Sea.
- The impact of turbine-induced turbulence should be accounted for.