The work described in “A semi-empirical model for near surface wind speed deficits downstream of offshore wind parks fitted to satellite synthetic aperture radar measurements” is of great interest as it relies on large-scale satellite observations to improve the accuracy of wake modelling in datasets from numerical atmospheric models. SAR imagery is indeed highly valuable information about the surface wind field in coastal and offshore locations and deserves to be better known in the offshore wind community.

While the scientific aspect and the innovative source of data are of great interest to the research community, the paper lacks consistency in the final application and the description of the methodology. Indeed, the authors emphasize that the final application is related to the improvement of surface wind speeds for the forcing of ocean models. However, the context of ocean modelling is rather poorly described, the focus being made by the authors on the wind speed in altitude and offshore wind energy in general.

Secondly, the description of the global methodology needs to be reviewed with particular care on the uniformisation of notations, precise mention of the considered height at each step of the process. Mixing parameterisations and concepts at different altitudes makes it complicated to follow the methodology and makes the whole process questionable with different extrapolations concepts in waked flows that are not properly discussed. The authors try to implement a rather simplistic approach based on some parameters to account for the change in altitude into their model. Indeed, the use of Fitch parameterisation and Ct requires a wind speed in altitude, the input of their model is a 10-m wind speed which is scaled up to address a wind field in altitude. But the output of the model is a surface wind field with inclusion of a wake. The model is trained/optimised on the difference between backscattered signals which are supposed to represent the surface wind field. A lot of uncertainties due to the change in altitude embarked in the whole methodology makes the conclusions of the author (i.e. an absence of turbulence modelling in their model to extrapolate properly insitu data which measurement heights are higher than 10m) hard to validate.

Thirdly, the authors use satellite imagery. The description of this remote sensing technique, as well as the resulting data, is very interesting and brings a new horizon to the offshore wind community. However, the offshore wind community is not familiar with this data, and it is important to demonstrate that the SAR-derived wind field does not bring additional uncertainties. I advise the authors to properly describe their methodology with the SNAP tool distributed by ESA and to explain why they do not use the L2 OCN products that include wind speed, NRCS and quality flags, that are provided and validated by the CALVAL of the Sentinel-1's Mission Performance Center.

I therefore recommend a **major revision** of this paper.

Detailed comments on the document:

A general comment on the position of the figures in the article: it will help to have the figures closer to the paragraph mentioning and explaining the figures.

Section 1: Introduction

Line 28: Please replace “industry” with “engineering”.

Line 54: You could emphasize that the addition of the wakes is done a-posteriori to existing surface datasets.

Line 56: The application of the tool is in contradiction with Lines 64-65. This sentence needs to be discussed and rephrased. The OWF context generally means wind speed or yield assessment in altitude, or structural analysis (from floaters/foundations up to the upper tip of the blades).

Figure 1: In general, color bars may be easier to understand with the title next to it.

- Figure 1c: Ct curve: wind speed at hub height or rotor-averaged wind speed. Is Ct extended for u_hub > 25m/s with a stall to a zero-Ct value?
- Figure 1d: I am not sure to understand the relevance of this graph. Would it be more interesting to locate the power of the turbines on the map as in a and b? Please describe or introduce dZ notation.

Line 59: Discuss the choice of the SAR scenes and how only 30 is relevant to cover all situations.

Line 61: The reference to the Fitch parameterisation needs to be added.

Line 61: Please uniformize the notation of the thrust coefficient, Ct or CT, throughout the paper.

Line 66: Explicit the height of the “near surface wind”. Is it 10m?

Section 2: Wake2Sea

In general, mathematical formulations need to be uniformized throughout the paper. Notations and parameters need to be introduced and clearly defined at each step.

When dealing with Ct and the wind speed U, it is not always clear at which height the equations are considered (10m or hub height). The applicability of Fitch parameterisation at 10m with Ct defined with U_10m needs to be strongly discussed.

For clarity, it would help to:

- Mention at the beginning of Section 2 that the eight constant parameters to be fitted will be called \alpha_i€(1-8) in the following equations;
- Recall the final system of equations in Line 130.

Line 74: Rephrase "bottom layer of the atm above the ocean surface".

Line 75: Is U at 10m? Within the surface layer? Within the whole MABL?

Line 80: Define \Chi = \nu_v/dZ^2 as the vertical diffusion parameter.

Line 81: Uniformize upper or lower case for u+/u-, and U in Lines 75 and 81.
Line 82: Please recall the Fitch parameterisation (its mathematical formulation) as it would make it easier to understand the following equations.

Lines 94 and 95: Please discuss the simplifications with order of magnitude.

Line 109: Define \DeltaT: is it T_2m – SST?

Line 110: Introduce \alpha.

Line 121: Ct(U) with U at hub height. I am not sure that Ct(U downscaled to 10m) means anything.

Line 126: Please discuss this assumption, especially the applicability of the Fitch parametrization at 10m.

Line 136: dZ is related to the height of the MABL? Is it constant for all stability conditions? A recall of the standard Fitch parametrization would be easier to understand if recalled in Line 82 to introduce the concepts and notations, especially of dZ and the "bottom layer" which seems to be a modelling concept rather than a physical part of the MABL.

Table 1: The table could be placed near Eq 10. It would prevent thinking that these coefficients are part of the wake model parameters. You could maybe place Figure 1c here to avoid scrolling through the document.

Figure 2: Min/max limits of x- and y-axes could be set at the same values to make the visual comparison easier.

Line 150: Please discuss the applicability of the power law extrapolation within waked flows.

Line 154: Why not use directly U_100m or other heights that are generally provided by atmospheric models? You are introducing a lot of uncertainties with Eq 15.

Figure 3: Explicit the name of the color bar.

Section 3: Satellite observations and model data

Line 159: SAR signal can be impacted by heavy rain.

Figure 4: For clarity purposes, it will help a lot to add when an extrapolation step is needed between U_10 and U_altitude and vice versa.

Line 173: Can you describe the steps where you calibrate the SAR data with SNAP? Is the starting point the SAR-derived wind speed from the L2 SAR product you get from https://scihub.copernicus.eu (which does not exist anymore, please change the source of your SAR data in the "Data availability" paragraph), or do you work directly with the L1 SAR product with SNAP? Is SNAP-derived step depicted as the green box in your Figure 4? I am not sure to understand the purpose of using the SNAP tool to get NRCS. Can you quantify the uncertainty at each step of your procedure? Indeed, you could directly use L2-OCN products to have a validated NRCS and the associated *owiquality* flag, thanks to the CAL/VAL Sentinel-1 MPC.

Line 177: I understand that the 200-m resolution concerns the NRCS field and not the SAR-derived wind field. Please explain this.

Line 180: Please change NCRS into NRCS.

Line 181: Please rephrase the "radar signals from turbines". It is the interferences induced by operating turbines that are disturbing the SAR signal.

Line 182: Please define STDV.

Line 182: The result of this filtering step can be seen in Figure 8.a. Please discuss the results, especially in the left boundary of the domain.

Line 183: Where do the environmental conditions depicted in Figure 2a and Table A1 come from? The atmospheric model? The FINO1 platform? The SST may come from OSTIA, but T_2m? How is computed the average wind speed displayed in Table A1? Is it a neutral wind speed? Does ws come from CMOD5.N?

Line 191: In Figure 2b, you plot the wind speed from DWD: if you are not plotting a neutral component, how is it comparable to the neutral U_10m that comes from CMOD5.N?

Line 193: Please correct "a reasonable representations" with "a reasonable representation". Discuss the "reasonable" as it does not seem to be really the case.

Figure 5: Please define the symbols in the legend of the figure (with explanations in Line 275).

Section 4. Inverse Modelling

In general, it will help a lot the understanding of the methodology if you recall at each step the height of the wind speed and when you need to extrapolate between 10m and altitude. It seems that surface and altitude components of wind fields are mixed up in the following equations.

Line 199: GMF are derived from scatterometers/insitu collocations, and not SAR/insitu.

Line 201: Please discuss the impact of neutral atmospheric conditions on your work.

Line 213: Please introduce the BG notation. U and V are 10m components or related to the wind speed in altitude?

Line 225: Here, it seems that components at different heights are mixed up:

- From what I have understood in the previous parts, it is the wind speed in altitude (cf l155 "scaled according to Eq. 15") that is used as input of Wake2Sea. Hence, \alpha_wake is supposedly related to a wind speed in altitude.
- \beta_j,k is related to the height of (u,v) in Eq 16 and 17. I do not know if it is 10m or altitude
- NRCS must take as input a neutral wind speed at 10m as the CMOD5N GMF is used.

Could you check the consistency?

Line 225: If we put the previous mixing on heights aside, when implementing the difference between the NRCS derived from the simulation and the original SAR NRCS, how do you:

- Account for the time difference between the model and the SAR acquisition time? (-20 minutes to +17 minutes in Table A1)
- Retrieve the "neutral" component of the DWD wind speed?

By doing so, be aware that at this 10m altitude (if I assume the height correctly), you are qualifying the surface parameterisation model of the atmosphere model, and the quality of forcing data, rather than the quality of the atmosphere modelling itself. You will need to discuss whether the conclusions you find at 10m can directly be applied to hub heights.

Line 255: Please define TVD.

Figure 8:

- Explain MIN value in the legend.
- In 8b and 8c, some artefacts are visible in the middle of the domain due to some effects at the boundaries of the descending pass. Could you comment on this?

- In 8d, the red value in the standard deviation on the left side of the domain seems to be due to the noise we also find in the number of images in 8a. Is it due to the filters applied to the NRCS?

Line 279: Introduce notations ‘ and “

Section 5: Inversion results

Line 310: Discuss the robustness of 30 samples in terms of statistics. Does Figure 8 take into account the filtered NRCS?

Line 318: Discuss the situations which required stronger corrections. Is it related to the pixels in the vicinity of the boundary of the swath which were evicted with the NRCS filtering? Is it maybe related to some bias in your GMF implementation?

Figure 11: what is the value of DT in Figure c and d ?

Line 325: Is the analysis done without the correction of the background wind fields?

In general in section 5.1, you show aggregated results for one entire year. How does the model individually compare to SAR images outside of the “training” dataset?

Line 332: Please put “situation” in the plural form.

Line 340: It would be interesting to add FINO1 location in all figures.

Line 348: Why are you using a neutral log law to extrapolate your insitu data? Figure 2a shows that unstable conditions are more common in this site. Moreover, you used a power law extrapolation in your wake modelling. You should show some consistency.

Line 355: Please rephrase the sentence with the missing OWF wakes. Is a one-m/s bias in wind speed a standard value between unwaked and waked flows? Wake deficits can be much stronger. Please add references to support this order of magnitude.

Line 356: Mean absolute error would be an interesting metric. You could also investigate the comparison by wind speed ranges corresponding to wind turbine regimes, also related to the behaviour of the Ct curve.

Line 358-360: Please give some references supporting this theory.

Line 361: Discuss the 3%-deficit threshold

In general, other major reasons can also be discussed:

- The uncertainties related to the different uses of extrapolation laws, with log law in neutral conditions rather than unstable versus power law earlier in the methodology.

Dedicated methodologies exist to compare wind speed at different heights in the surface boundary layer (for instance a very recent work in https://wes.copernicus.org/articles/9/1727/2024/);

- The applicability of the Fitch parameterisation at 10m;
- Uncertainties related to the inversion model between wind speed and NRCS, and the assumption of neutral wind speed derived from CMOD5N;
- Uncertainties related to the optimisation methodology to derive the wake model;
- The representativity of the SAR dataset.

Section 6: Theoretical considerations about Wake2Sea

Line 370: Please rephrase “the shape of the wakes” into “the horizontal shape of the wakes”.

Line 372: Recall the reference of the equation describing D.

Line 394: Please give references for the Gaussian shape profile.

Line 415: Recall that Figure 1.c shows that Ct=0.8 for a wind speed of 8 m/s. Here again, the thrust curve as the power curve of a wind turbine is a function of the wind speed encountered by the rotor in altitude. It can be the wind speed at hub height, or a rotor-averaged wind speed. The vertical profile of the wind speed in a wake is not well understood yet, but some studies also mention a Gaussian shape in vertical direction, whose lower part is more or less disturbed depending the nature of the ground. I am not sure that Eq 11 and 12 are able to account for this “Gaussian”-like vertical profile and the resulting behaviour between 10m and hub height.

Line 421: Please replace “suggest” with “suggests”.

Section 7: Conclusions and Outlook

Line 432: Your explanation about the turbulence impact on the vertical extrapolation in waked flows is interesting but lacks scientific references. And may be of secondary importance compared to other reasons listed in my previous comments.

Line 436: Please discuss the marginal deviation from the standard formulations from the literature. Are you thinking about engineering wake models? If so, please add comparisons with your model, and add references to benchmarking studies with engineering wake models.

Line 442: Please explain how you obtained the 36% value. Discuss how you can derive your results in surface to altitude considering the limitations that have been highlighted, especially due to the major uncertainties in the vertical profiles in waked flows inside a wind farm and within a cluster of wind farms.