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.

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.

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.

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.

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).

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).

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 ??'.

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.