

Second review of “Uncertainty in Offshore Wind Power Forecasts: A Regional Climate Modeling Approach for the North Sea” by Alberto Elizalde, Naveed Akhtar, Beate Geyer, and Corinna Schrum, submitted to Wind Energy Science

I will address the authors’ responses first, and then discuss a couple of additional issues.

8. *Original issue:* L. 164-167: please explain how the large (15 MW) and small (3.6 MW) turbines were arranged over the same wind farm areas for the 3.6 MW and 15 MW cases. What spacing (8Dx8D? 10Dx10D?) was assumed?

Original reply: Since the model resolution in our case is 2 km x 2 km, smaller-scale processes cannot be explicitly resolved. The parametrization of Fitch et al. (2012) assumes that all wind turbines within a grid box are effectively located at its center in superposition, without interacting with one another. In other words, if the turbine density for a specific wind farm (N_T in our formulation) is sufficiently high that multiple turbines fall within a single grid box, their mutual wake effects are neglected. For clarification, line 157 has been revised to read:

“ N_T is the turbine density, calculated as the number of turbines per wind farm area, assumed to be located at the center of the grid box in superposition, with no mutual interaction, ...”

New: I did not explain myself well. I know that the Fitch parameterization is insensitive to the actual turbine location. I was asking how you determined how many turbines to place in each grid cell. For example, if you assume 8Dx8D spacing with the 3.6 MW turbine ($D=120$ m), then each grid cell of 2 km x 2 km would contain: $2000 \times 2000 / (8 \times 8 \times 120 \times 120) = 4.3$ turbines, let’s say 4. But if you use a larger spacing, say 10D x 10D, then you’d place 2.7 ~ 3 turbines only. You must have assumed a value for spacing, please add some details about that.

12. *“For clarity, Figure 5 caption and related text have been revised to read: “Wind speed anomalies, calculated as deviations from the respective dataset mean ...”*

Thank you for clarifying what anomaly means here. But then it is not true that “The model simulation is able to capture the wake effects”, because the comparison is not good at all and it appears that the model does not reproduce the anomalies well and it misses entirely the positive anomalies to the west. Please add a comment/discussion about this.

New issues

- a) After reading Reviewer #2’s comments, I must agree with them that this study is not an “uncertainty” study, but rather a “sensitivity” study. Uncertainty deals with the fact that we do not know something exactly (e.g., because there are errors in measuring or simulating it), but we are close in order of magnitude. For example, with respect to the wind turbine capacity, uncertainty would be whether it is exactly 3.6 MW or, say, 3.5 MW or 3.7 MW. But in this study the authors considered 3.6 MW versus 15 MW. This is not an uncertainty. Same for the IC/BC: using two different datasets is not an analysis of uncertainty, it is a sensitivity to which reanalysis is chosen. All the authors must do is replace the term “uncertainty” with “sensitivity” and perhaps rephrase a few sentences to make sure that the scope is clear.

- b) Chronological runs: the more I think about this, the less convinced I am that the Chronological runs should give better results than the NH runs. The atmosphere is a system that does not depend much on the IC; the state variables (T, p, u, v, etc.), by definition of a state variable, do not “remember” the path that it took to get there. In other words, whether the wind farms were installed all together or chronologically, ultimately the atmosphere should reach a final state that is the same regardless of how we got there. Obviously in the short term there could be deviations, but after a while the results should converge, or close to it. However, at times the atmosphere is chaotic, meaning that it can reach very different final states given different initial conditions. But this is uncommon. So the question is: is this an example of a chaotic behavior of the atmosphere, or was the statistical performance analysis just conducted incorrectly? In my original review, I raised questions about this in issue #2 about Table 4 and the authors basically said that they did not want to limit the analysis to 2018 (the only year with no farm development) for fear of losing statistical significance. In light of this, I must request that:
1. The analysis be redone and limited to 2018 only in Table 4;
 2. Table 5: add the results from the NH runs at FINO1;
 3. Table 5: add the results for FINO3.

If the atmosphere is normal, then in Phase 4 (Table 5) and in Table 4 the results should be very similar between the Chronological runs and the NH runs. If the atmosphere is chaotic, then the results might be significantly different, and this would be worth discussing further.

- c) Table 4: missing statistics for ChrNH for FINO3.