Reviewer's comment to version #2 of

"A consistent Computational Fluid Dynamics surrogate model for wind turbine interaction including atmospheric stability" -

May 2026

Overall evaluation

Compared to the first version, this edited manuscript put additional emphasis on the description of the methodology. The authors made significant efforts to clarify all the steps involved in in the process of reconstruction the farm flow from the single-wake look up table (LUT) which is now thoroughly presented. Notably, Figure 2 is a great addition since it captures all the added information in a synthetic way. Additionally, section 2.3.2 provides the reader – who may not be familiar with pyWake – with a clear description of the farm flow reconstruction process. Finally, section 2.3.3 is probably the most valuable addition since it presents the development of the wind speed superposition model as one of the author's original contribution more explicitly (that part was a bit understated in the first version of the manuscript). The resulting section 2 is probably a bit dense (10 pages with mostly text and equations), although this is consistent with the objective of the paper which is to introduce the RANS-LUT methodology. Moreover, the extra information provided clarifies the main design choices and, thus, facilitates reproducibility of the results presented in the remainder of the paper.

The main comments that I raised in my previous review are clearly addressed in this edited version. The differences between power estimated by the RANS-AD and RANS-LUT methods are well justified by explaining the differences between the two power reconstruction algorithms. The choice of the superposition model is well motivated by the results presented in appendix B that clearly supports the author's claim about error compensation. One remaining point worth noting concerns the inclusion of the part of the speed-up region within the wake part of the LUT due to the upstream/downstream split. Since the wake and blockage parts use different superposition methods, this causes the speed-up region to accumulate differently depending on the model. It also implies that wake models will always carry over part of the speed-up region, even in "wake only" (PropagateDownwind) simulations. Solving this caveat falls outside the scope of this study, and its impact is expected to be limited given the small extent and magnitude of the speed-up included in the wake model. Nevertheless, a brief acknowledgment of this limitation in the manuscript would be valuable both for transparency and to orient future research.

In summary, this updated version of the manuscript addresses all the technical questions that were raised during the first round of review and requires only minor revisions before acceptance. For the final version, I would only recommend the authors to address minor points listed below. These remarks do not reflect on the quality of the work, which is already high, but are intended solely to improve readability.

Suggestion for clarification

Introduction

- I think would be beneficial adding a sentence describing what is the novelty of the superposition method the authors developed compared to that of Zong and Porte-Agel. As I understand this is an original contribution of the authors which is worth pointing out. Furthermore, making the distinction between the two models clear at this stage of the paper would facilitate the understanding of section 2.3.3 (see comment on section 2.3.3 below).

2.1 Test cases

- Could the authors detail what motivated the choice of the turbulence intensity levels for the stable, neutral and unstable cases. Especially since the TI levels for the neutral and unstable cases might seems a bit low. Is it design to be the most adverse case for the models, to meet wind conditions from a specific area or standard values often used in literature? A simple sentence would be enough.

2.3.1 Single turbine model

- Could the authors provide additional information on what is involved in the process of "collapsing all dimensions into a 1D array"? I think this points is worth detailing a bit more to ensure reproducibility. Alternatively a reference detailing the method could be included.

2.3.3 Superposition methods

- The section describing the wind speed superposition method may be a bit hard to follow. I think this is partly due to the fact that the authors' original contribution is not stated clearly until late in the paragraph. My understanding this that equations (16) and (17) are directly taken from Zong and Porte-Agel, while equation (19) is introduced by the authors to generalize the momentum-conserving superposition model to wind farm flows with multiple wakes. The original paper from Zong and Porte-Agel is paywalled. Therefore it makes sense to recall the basic principles since the reader may not have access to it. Nevertheless, I would recommend to put more emphasis on what is new – potentially by mentioning it earlier in the paragraph – and what motivated these developments.
- I would recommend adding, after the sentence detailing the choice of linear superposition for the blockage deficits on line 260, a brief discussion on the influence of the upstream/downstream splits on the reconstruction of the speed up region with different superposition model (see my above comment).

3 Results

- I would recommend defining the variables U_{LUT} and k_{LUT} at their first occurrence in the paper (i.e., intro of section 3). This point is important to clarify, notably, that the reconstructed flow field includes self-induction and can therefore be legitimately compared with the RANS-AD results. Citing equations (13) and (14) should probably be enough.

3.3 Wind turbine power

- Can the authors add a brief description of the method used for estimating the power coefficients from the local flow conditions for the reader who does not have time to read the reference. Moreover, the cited paper presents three distinct methods. I assume the one used in this study is the "AD Variable Scaling Method" (method II) but it would be good to clear any ambiguity. A simple sentence would suffice.

Minor corrections

- **Lines 43-45**: the sentence "Fuga and the RANS-LUT models of Schulte and Stoevesandt (2014), and Jacquet et al. (2022), do not take wake-added turbulence intensity (TI) into account." reads a bit awkward. It should be rephrased to "Neither Fuga nor the RANS-LUT models of Schulte and Stoevesandt (2014) and Jacquet et al. (2022) take the wake-added turbulence intensity (TI) into account."
- **Line 96-97**: the use of the semicolon ";" followed by "however" makes the flow of the sentence a bit odd. It could be "The same mesh topology and boundary conditions as those used by van der Laan et al. are applied, while the domain dimensions are defined on a case-by-case basis."
- **Lines 234**: A comma should be added after "generally" since it is placed at the beginning of the sentence. "Generally, the background flow [...]"
- **Equation (20)**: the term G_L appears to be undefined, based on equation (8), this should likely be D_L.
- **Line 362**: While grammatically correct, the sentence: "Hence, it is important to include wake-added TI LUTs in the RANS-LUT" ends the section a bit abruptly. I would recommend rephrasing to "The results presented in this section demonstrate the importance of including the dependency on the wake-added TI in the RANS-LUT model."