General comments

Overall, the preprint is of very high quality and is clearly and concisely written. The presented work constitutes a very relevant addition to the state of the art, particularly due to the thorough comparisons between the wake models, which represent a clear added value. The manuscript is generally well structured and easy to follow; however, minor improvements in readability could still be achieved. In particular, some tables and figures are occasionally placed before their first mention in the text, which slightly disrupts the flow. Addressing this and the specific comments (see below) would further enhance the clarity and accessibility of an otherwise excellent paper.

Specific comments

- I was not familiar with the concept of wake regeneration above conventional wind farms, so perhaps this could be explained a bit in more detail: How does this work? What is the advantage?
- Line 15: "*The solution of the original no-wake problem deviates with 145% compared to this reference solution."* Specify what solution you are looking at; is it power, trajectory? I only find out later that it is the force. Furthermore, what is the reference solution? The one that includes the wake? Please clarify.
- Introduction: The writer could refer to the following papers:
    - T. Haas, J. De Schutter, M. Diehl, and J. Meyers, "Large-eddy simulations of airborne wind energy farms", *Wind Energy Science*, 2022.
    - J.-B. Crismer, T. Haas, M. Duponcheel, and G. Winckelmans, "Large eddy simulation of airborne wind energy systems flying in turbulent wind using model predictive control", *Wind Energy Science Discussions*, 2026, Preprint discussion started: 09 Jan 2026; under review for Wind Energy Science (WES).
    - Pynaert N., Haas T., Wauters J., Crevecoeur G., Degroote J. (2025). Aero-servo simulations of an airborne wind energy system using geometry-resolved computational fluid dynamics. Wind Energy Science (WES). https://doi.org/10.5194/wes-10-2663-2025.

  In these studies, the wake is simulated by coupling AWEbox to a virtual wind environment (LES-RANS) that includes the wake implicitly. The trajectories that are flown are, however, based on simplified models that do not include the wake. The aforementioned studies could benefit from your work. It could also be interesting to compare the wake and induction with these studies (perhaps in future work).

- Line 55: "*In conclusion, the accurate performance assessment of utility-scale kite systems requires the development of optimization-friendly engineering wake models capable of capturing self-induction effects*." It could be beneficial to stress that the high controllability of airborne wind energy systems enables them to avoid wake and induction effects, potentially increasing power output, provided that this information is incorporated into the controller, further highlighting the significance of this study.

- Line 80 "*To limit the scope of this paper, we consider a kite system with fixed tether length, thereby excluding pumping-style AWE systems, which rely on tether reeling*." Also specify that no power generators are involved; e.g. fly-gen systems, but purely act as actuators.
- Line 94: You introduce *y* as a parameter for the wake state; is it possible to give a physical meaning to the "wake state" to help the reader? Also sometimes it is denoted as y(t,.), something y(t,tau); is there a (mathematical) difference? Please explain.
- Line 109: A figure would be helpful for the reader to visualize the states of the system.
- Line 131: "*Incorporating more realistic wind and density profiles as a function of flight altitude is considered straightforward and omitted here for simplicity*." I would argue that wind shear has a big influence on the wake development, so I am not so sure this is straightforward.
- Line 156: "*properly distributed*"; do you mean uniformly distributed?
- Line 171: "*For each wing, we ignore the induction of its own trailing wake less than a time T away, since this part is taken into account by the induced drag term.*" Can you discuss what would be the effect of not including the induced drag instead?
- I find "u^f" not so intuitive to indicate the induced velocity, but perhaps this is the standard notation in the literature.
- Figure 1: Indicate both methods using (a), (b), or (left),(right).
- Figure 3: The legends in panels (b) and (c) are difficult to read; using black instead of white would likely improve their readability. In addition, including the system trajectory in these figures would provide valuable additional context.
- Line 250: "*One problem is that the optimal orientation trajectory by construction assumes zero angle-of-attack*"; I would rather state that the angle of attack is not included or represented in the model, zero angle of attack would mean zero lift (or zero angle-of-attack lift).
- Figure 4: I had some difficulties understanding the figure. Clearly indicate in the figure what is part of the DUST simulation (I assume the thin black line) and the vortex loop wake (I assume the red dots, and blue arrow?).
- Line 323 *"For the atmospheric airborne actuator case, as well as for the ship-towing case, the tether pulling force is to be maximized."* Please provide additional explanation and justification for why this metric is the most appropriate or meaningful choice for the atmospheric actuator case.

- Figure 6: Indicating the system trajectory in the figure would improve clarity. Additionally, including an error contour plot could further enhance the informative value of the results.
- Combine figure 6,7,8 to enhance the readability.
- Table 1 describes the system variable bounds (both tether and wing properties), and Table 2 describes the kite system model parameters (both tether and wing properties). Why are they not presented in the same table? Furthermore, how are these system parameters determined?
- I wonder if it would be valuable to also compare the discretized wake with the DUST simulations.
- Line 418: "The transcription includes a time transformation approach to deal with the variable time grid." This appears to be an important step and would benefit from a more detailed explanation.
- The optimal flight trajectory obtained using the no-induction model appears physically unrealistic. For a flight speed of 190 m/s and a turning radius of 50 m, the required centripetal acceleration would be approximately 73 g, which is far beyond feasible operational limits. Please clarify this point and discuss the physical realism and practical implications of the resulting trajectory.
- Line 488: "*The model is also very well able to capture the induced velocity field around the wing trajectory flown, suggesting that it is well-suited for optimization purposes.*" I believe this sentence is a bit misformulated: The system model, including the wake model, increases the accuracy; on the other hand, the speed will determine if it is well-suited for optimization purposes. E.g., High-fidelity LES would not be well-suited for optimization purposes, but captures induced velocity very well.
- Line 493: Specify the original OCP (with or without wake model?)
- Line 495: Specify the reference solution (with or without wake model?)

Technical corrections

- At the end of the introduction, the conclusion is not included in the discussion of the structure of the paper.
- To be checked with the editor, but if I remember correctly, vectors should be defined in bold.
- Section 4.4 has almost the same title as Section 4.3.
- Figure 9: Please explain N, M, N, to make the figure self-explanatory.
- Figure 9: Punctuation mark forgotten.
- Line 418: "four Radaulla collocation points" → Typo?
- An introduction to the sections is missing in Chapter 6.
- Eq. 10: is "u_t,i" defined somewhere?