GENERAL ASSESSMENT AND SUMMARY

The manuscript presents an engineering wake model for crosswind kite systems. Compared to previous works, the proposed vortex-based continuous-time wake model is capable of capturing the unsteady, non-axisymmetric flight conditions. The continuous wake formulation is thoroughly validated against the higher-fidelity DUST solver, demonstrating good agreement in induced velocities and aerodynamic loads under the considered conditions. The paper is generally of good scientific quality, well structured, and clearly written.

The subsequent simplifications introduced to render the wake formulation computationally tractable within the optimal control framework are technically interesting and relevant. However, certain modelling assumptions and case study choices would benefit from further clarification and discussion in order to fully assess the generality and robustness of the conclusions.

Overall, the manuscript makes a valuable contribution to wake-aware modelling and optimisation of airborne wind energy systems.

Major Comments

1. **Validation of the simplified wake model within the optimal control framework**

   The continuous vortex-loop wake model is convincingly validated against DUST. However, the optimal control problem employs a simplified hybrid wake model with reduced window-of-influence assumptions and finite wake history.

   While such simplifications are understandable for computational tractability, it remains unclear to what extent the reduced model preserves the quantitative accuracy demonstrated for the full formulation. Although Figure 12 compares the discretised wake against an analytical vortex-loop solution, it is not evident how the reduced model compares to the DUST-validated formulation.

   Since the dual-kite optimisation results rely on the simplified wake model, it would strengthen the manuscript to clarify this validation chain, for example through a direct comparison with DUST for the reference solution or by quantifying the modelling error introduced by the OCP simplifications in the reference solution.

2. **Representativeness of the case study and interpretation of wake effects**

   The numerical case study considers a configuration with fixed tether length and without reeling dynamics and with a very optimistic mass scaling (see https://doi.org/10.5194/wes-7-1847-2022). This appears to represent either a fly-gen system without explicitly accounting for the additional mass and drag of onboard turbines, or a ground-gen system without accounting for its reel-out operation.

   In both cases, the resulting flight speed and loading would be reduced in real operation, which directly affects period duration and wake strength. The analysed dual-kite configuration therefore seems close to a worst-case scenario in terms of wake sensitivity.

While this does not invalidate the results, it affects how broadly they can be generalised. A more explicit discussion of the operational interpretation and limitations of the chosen case study would help readers assess the scope and transferability of the conclusions to other systems.

3. Half-periodicity assumption for the dual-kite problem

The manuscript states that, because the two airfoils follow nearly identical trajectories over a full orbit, the dynamics can be integrated over half an orbit by enforcing half-periodicity (line 411), following Zanon et al. (2013).

This reduction appears insufficiently justified for the present setting. For a helical trajectory, the “up-loop” and “down-loop” segments typically experience different apparent wind conditions, speeds, and load distributions due to gravity, so the orbit need not exhibit a strict half-period symmetry. This makes the choice of the two half-orbit boundary points (where the state matching is imposed) critical, and it is not clear from the text where along the orbit the half-horizon starts/ends, nor under which assumptions half-periodicity is expected to hold.

Related optimal-control studies (eg. https://doi.org/10.3390/en16041900) explore dual-aircraft configurations without taking only half-period and the resulting trajectories are not generally symmetric. This suggests that enforcing half-periodicity may exclude physically relevant asymmetric solutions unless the symmetry assumptions are carefully justified.

I recommend that the authors (i) clearly define the half-orbit start/end points along the trajectory, (ii) explain the conditions under which the half-periodicity constraint is valid for their chosen path class and (iii) ideally demonstrate that solving the full-orbit problem does not yield materially different (asymmetric) optimal solutions for at least one representative case.

Minor Comments

- Line 14-15: Specify what metric are you using for the accuracy and comparison with reference solution. It should be clear by only reading the abstract.
- 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 mention that no onboard power generators are included, such that they purely act as actuators
- Line 81: “However, the developed methods in this paper can be directly applied to these type of systems as well.” The present method assumes a fully developed wake, whilst in pumping systems wake development becomes increasingly important, particularly if the amount of flown loops is small. Please justify further how these methods could be applied to pumping systems or remove this phrase.
- Figure 1. Add a label to the methods (a,b). There are some symbols that are only introduced later in the text so the reference should indicate a) or b) as well.
- Line 228: “Recall that for each wing we ignore the self-induced velocity for the first half-rotation interval with length T.” It says recall while it is the first time a half-rotation is mentioned.
- Line 250: additional clarification of the DUST simulation setup would improve transparency. It is not entirely clear whether the wing kinematics (positions and velocities) are prescribed and the pitch angle is chosen to match a target C_L without accounting for induction effects,

or whether the induced velocities computed by DUST are already incorporated in a fully coupled manner such that the local angle of attack and resulting aerodynamic loads are determined self-consistently.

- Figure 4 is somewhat difficult to interpret in its current form. The readability would improve if the flown trajectories of the kites were explicitly included in the visualisation. In addition, it would be helpful to clearly distinguish in the figure or caption which results correspond to the DUST simulation and which to the proposed vortex wake model.
- Lines 275-280: It would be helpful for the reader to understand how to qualitatively asses over or underestimation of convection speed in Figure 4 (eg. number of tip vortices seen in DUST vs vortex method)
- Figures 6,7: it would be helpful to mark the positions where the wing crosses this plane
- Figure 9. The time intervals N are difficult to see.
- Subsection 6.2. the discussion of discretisation sensitivity is not entirely clear. In particular, the relationship between the number of collocation intervals N(global time discretisation) and the influence window parameter N_f would benefit from clarification. Please explicitly state the value of Nused and clarify how it relates to the window size $2N_f + 1$.