Review of the manuscript wes-2025-77, “Wind Tunnel Load Measurements of a Leading-Edge Inflatable Kite Rigid Scale Model” by J.A.W. Poland, J.M. Spronsen, M. Gaunaa, and R. Schmehl.

The study presents the wind tunnel measurements of the aerodynamic loads of a downscaled rigid model of a 25 square meter TU Delft V3 Leading-Edge Inflatable (LEI) kite. The experimental measurements are further compared with the results from numerical methods: particularly RANS results from the literature and VSM method. Authors claim to have observed similar trends and values in the load data between the experimental data and the results from numerical methods under nominal operational conditions. Confidence intervals are presented for the measured data, and the discrepancies are attributed to the experimental setup in the wind tunnel. Correction of the experimental measurements are performed using standard methods considering blockage, downwash, and streamline curvature. The measured data and used codes are made publicly available through Zenodo.

Introduction along with the literature review is presented in the first part of the study, which is then followed by the description of the experimental setup and downscaled model. Mean and standard deviation of the measured aerodynamic loads as a function of angle of attack and side slip angle, considering uncertainty analysis, boundary layer transition and Reynolds number effects are presented.

The manuscript is well written and organized. The study provides novel experimental data sets on steady aerodynamic coefficients for the downscaled models of LEI kite. However, the authors do not discuss/acknowledge about the unsteady loads or aerodynamic coefficients, which are responsible for the realistic aerodynamic performance of LEI kite. In addition, the following comments should be addressed for improving the quality of manuscript.

1. Page 8, Line 134: A measurement period of 10 s is used. Is the time sufficient for the convergence of statistics? Discussions on the convergence analysis should be made.
2. Page 8, Figure 6: Please check the x-label of the figure, $Re \times 10^{-5}$ or $Re \times 10^{5}$?
3. Page 9, Line 155: where U_k is the local velocity at the roughness height, which may be approximated by U_∞. Does the presence of boundary layer near the leading edge affects the approximation of U_k as U_∞?
4. Page 10, Line 184: x_{cg} is repeated twice and needs to be corrected with z_{cg}. Does the change of angle of attack, slide slip angle as well as deformation of the model at high test speed affects these values? If it does, how are they corrected?
5. Page 12, Table 3: What is the angle of attack and slide slip angle for these measurements?
6. Page 14, Line 269: Please discuss why $\alpha = 7.4^\circ$ was selected.
7. Page 16, Line 293: ‘.. we applied the area ratio of 3.7 as a correction factor to the Cs reported in Vire et al. What's the reason for using this specific ratio? It should be clarified.

8. Page 16, Line 301: '.. contained a 1.02 degree offset in the angle of attack". How has this been decided? Is it done for the better match of the experimental results with the results from numerical simulation?
9. Page 17, Line 324: In figure 10, the legend does not indicate with and without strut CFD studies.
10. Page 17, Line 329: 'All numerical models predict a higher maximum L/D…' Is this because of the difference in Reynolds number between the experimental setups and numerical simulations?
11. Page 21, Line 377, 'the observed agreement suggests there is an aerodynamic potential within the presented numerical models..'. However, the experimental results show that there is a disagreement after a certain range of angle of attack and slide slip angle. The statement should be justified with details.
12. Page 28, Line 523: Comments on the sources of vibrations (resonance, vortex shedding) will make the manuscript more robust.