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Interactive comment on "Aerodynamic characterization of a soft kite by in situ flow measurement" *by* Johannes Oehler and Roland Schmehl

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We respond to the referee comment by including our answers in a copy of the original comment:

The paper describes an experimental approach to estimate some basic aerodynamic and performance characteristics of a soft kite that is used for airborne wind energy generation. This is achieved by applying a novel setup for measuring airspeed, angle of attack and angle of sideslip in a position between the power lines of the kite in a short distance above the kite control unit. For the presented soft kite this setup seems to fulfill the premises to obtain meaningful information about the relative airflow

at the kite. From these measurements, the measured tether force and elevation angle, together with systems parameters from geometry and masses, the authors were able to estimate L/D, CL and the angle of attack at the chord. Thereby the could show that the variations of angle of attack and the angle of sideslip are not as large as indicated in the literature. They also could approximately reproduce the magnitude of L/D derived from aerodynamic models.

Although I am not very familiar with the literature and recent achievements in the AWE domain, it seems to me that their approach using the AWESOME measurement equipment offers new possibilities in obtaining valuable data for the characterization and modeling of AWE soft kite systems. The possibility of measuring the angle of sideslip for such a system is unique. I see a high potential for future use. The content of the paper is good and worth to be archived. Nevertheless there are a few aspects I would like to comment on.

Specific comments:

As the authors discuss many simplifications they seem to be aware of the limitations of their results. Many of their assumptions are subject of significant uncertainties. To name a few: They use a fixed geometry derived from a CAD model, although due to the flexibility and elasticity of the system, the assumed geometry of bridles, lines and canopy is deformed depending on the changing loads acting on these elements. Another significant simplification is the assumption of flying in quasi-steady equilibrium. From what I know, crosswind trajectories are highly dynamical maneouvres and accordingly not only the gravitational but also the inertial forces and moments have to be taken into account. They also consider unsteady airflow when discussing the oscillations observed. The assumption of a fixed center of pressure is a massive simplification too. On the other hand, it is comprehensible to simplify, because such effects are much more difficult to account for. Nevertheless, in my opinion, simplifications and neglected effects should be especially used in the discussion of the results, for example to explain the large dispersion of the derived L/D and CL. Obviously (see

fig 15 and 17) the applied filters alone were not able to reduce the dispersion very much. In my opinion the discussion and explanation can be improved here.
Response: Thank you for this input. Indeed the simplifications that we made in our modeling assumptions should be given more space in the discussion of results. We will consider this in the final publication.

Concerning the results (fig 15) I did not understand the trend of L/D vs alpha for the depowered flight. As noted, it contradicts the trend in the aerodynamic models. Although it is said that the angle of attack does not have strong effect in this flight regime as the wing is largely deformed, it does not explain the clear trend of L/D being reduced with increasing alpha. If possible, an explanation for the opposite trend should be provided.
Response: The mentioned effect that L/D is reduced for a higher angle of attack is only visible when looking at one certain power setting (e.g. the lowest one in dark blue in fig. 12). Since the measured inflow angle itself is used for the calculation of L/D a change in the measured flow angle causes this change in L/D. A steering command or change in the heading of the kite can be the root cause of this. L/D is actually not reduced with increasing alpha for depowered flight. This can best be seen in fig. 12. The main trend is a higher L/D value for a higher power setting at higher angles of attack.

Further comments and technical corrections:

1) Right at the beginning it is said that wind tunnel testing of large deformable kites is practically not feasible. It is possible, but of course it is a question of money and available facilities. In US large gliding parachutes have been tested in the wind tunnel (see Geiger/Wailes: Advanced Recovery System Wind Tunnel test Report, NASA TM CR 177563, 1990). In Europe a scaled model of the FASTWing parachute was tested in the DNW-LLF wind tunnel (see Willemsen et al: The FASTWing project: Wind Tunnel

Tests, Realization and Results, AIAA 2005-1641).
Response: We agree and have adjusted the manuscript accordingly, a.o. by referring to the experimental campaigns at NASA Ames and DNW-LLF.

2) On page 3 the power setting "up" is introduced but not clearly defined. An implicit definition is later provided in equation 5. On page 3 a reference to eq. 5 should be included.
Response: We write after the introduction of u_p “A high value of u_p means that the wing is powered, while a low value of up means that the wing is depowered.”. We agree that refering to Eq. 5 where u_p is linked to the geometric lengthening of the rear bridles is better. We will include this reference.

3) On page 9 "cref" is defined perpendicular to the power line, but in fig. 5 "cref" seems to be defined as horizontal distance. Please update fig. 5.
Response: The reference chord length c_{ref} is a geometric parameter of the wing which we use to relate the actuation of the steering lines Δl and the pitch rotation of the wing, quantified by the depower angle α_d. We have slightly reworked the definition of c_{ref}. It measures the distance from the trailing edge of the wing, where the steering lines are attached, to the (virtual) point of rotation of the wing. Please check the revised manuscript for more details. The state displayed in Fig. 5 is a rotated (depowered) state with $\alpha_d > 0$. We intentionally chose a perspective at which the reference chord is horizontal relative to the paper.

4) On page 10 the authors refer to a "mechanistic model". Does this mean a rigid body model? Please explain the meaning.
Response: With "mechanistic model" we actually mean the geometric model discussed in Fig. 5. We have adjusted this in the text.

5) The calculation of "lambda0" is discussed on pages 12 and 13. Here the corresponding equations should be given.
Response: Since we use a numerical solver for the bridle line angles the whole algo-

rithm would be rather long. We updated the text already to say that we use a numerical shooting method upon the request of one reviewer. We added a description of the algorithm.

6) "Beta" is usually used for the angle of sideslip. If possible use a different symbol for the elevation angle.
Response: We chose β mainly to be consistent with the publications of our own group where β is used for elevation and β_s for the sideslip angle.

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