Reviewer 1

This paper highlights the potential nonlinearities that can be observed in a tensegrity structure of a rotary airborne wind energy system. In particular, the hysteresis of the torsional stiffness and the discrepancy in the numerical vs experimental resonance frequencies are valuable additions to the literature and could be useful for further studies.

I only have two minor comments:

I understand the justification for only considering aerodynamic drag in equation 14. However, it would be useful to briefly comment on whether the lift generated by the spinning rotor is significant. I assume it's not.

Equation 14 is our model for the aerodynamic force acting on the helix (and not in the rotor). Only the aerodynamic drag is considered, as such a force component may dominate the lift. The spinning helix is made of rigid bars and tethers and its function is not to generate any lift but to transmit the torque from the rotor to the ground station.

The lift generated by the rotor is indeed important, but the rotor is not part of our model, which is focussed on the helix.

2. The hysteresis observed in fig. 5 is interesting. As the authors have noted in section 5.3, this could be a contributing factor to the resonance observed in experiments that were not predicted by numerical simulations. I think that the un-modelled hysteresis is the main reason for this discrepancy, so some discussion on how this can be studied further can be useful. There are some ODE-based methods that can model hysteresis in a dynamical system (e.g, the Goman-Khabrov model. This is more for unsteady aerodynamics, but the ODE-based approach can be extended to other applications). The authors can consider adding a brief discussion on how this hysteresis can be modelled in a future study.

We thank the Reviewer for providing this input. We added a few sentences about this important point.

Lastly, I have a few suggestions regarding grammar/spelling in the attached pdf.

We really thank the Reviewer for providing this revision. Most of his/her suggestions were incorporated to the manuscript.

Reviewer 2

This paper presents a model of a rotary AWE system. The structure model is 'decoupled' from the airborne rotor with the setting up of boundary conditions. Interesting new insights are obtained from the combined numerical and experimental studies. The investigation of the three scenarios following an increased complexity makes very good sense. The work includes clear novel contributions. The conclusion section is well written. The following minor comments are for the authors' reference.

We are glad to know that the Reviewer found that our work provides interesting insights and has novel contributions. We thank him/her for providing the minor comments that we address in the lines below.

In Figure 5, the initial point and the ending point are not identical for each tension applied. What causes this difference? Should it be a closed loop (in theory)? Also, when the tension is 233N (green colour), the pattern seems to be quite different from the rest. Is there a reason for this isolation?

In our opinion, the results of Fig. 5 are a consequence of the structure of the helix, which is made of bars under compression connected by tethers and knots. As the torsional torque M_t increases, in steps to give a torsional angle of 10º, the tension T_e also increases and exhibiting hysteresis. Clearly, the behaviour of the structure depends on the history. We think that it is because the exact position of the knots and the contact points between bars are not fixed but certain sliding occur and also friction plays a role. The exact positions of such a contacts points depend on the internal tension imposed for zero torsion (for this reason the curve for T_{e0} = 233N is so different) and also on the history. Due to the presence of knots and the sliding of the bars, each time the helix passes from being relaxed to be tensioned it acquire a slightly different configuration that translate into different mechanical properties.

In Section 5.3, it is said the frequency \$f_1\$ was verified. Is this forcing frequency of the eccentric arm introduced in the experimental study only, not covered in the modelling somewhere?

We are not sure to fully understand the question. In the experiments, frequency f_1 was imposed by the eccentric arm and it was measured and controlled.

In our model of the helix, frequency f_1 appears as a boundary condition in Eq. (76). In our numerical analysis such a frequency was varied to mimic the conditions of the experiment.

If yes to the previous comment, what is the model used for bifurcation analysis (Figure 8)?

The model of the helix used in the bifurcation analysis of Fig. 8 is the model explained in Sec. 2 together with the boundary condition of Eq. (76). In the bifurcation analysis, we fixed a value of the frequency f_1 and integrated the equations of motion numerically by using the initial condition explained in Sec. 5.1. The integration was long enough to let the system converge to a solution, which is periodic. We then computed the maximum value of the tension for a given oscillations.

The use of mathematical notations is confusing in places. Better give a table to list key variables, parameters, reference frames, operators, etc. It would be helpful to specify the use of bold letters and plain letters especially when the same letters are recycled, e.g., the two v_A terms in Equation (14), the time t and the bold letter of t. If possible, keep a consistency in partial derivative representations in PDEs and derivatives in ODEs.

We revised all the equations carefully and we did not find any error. However, we agree that some key aspects can be highlighted to help the reader to understand the notation. We added several sentences in the revised manuscript to help its readability and clarity. We thank the Reviewer for this comment.

In several places, the term 'dynamic' should be 'dynamics' for dynamic systems.

We made a search to the word "dynamic" and corrected it.

In Line 71, it should be 'r(L_0,t)' not 'r(L,t)', isn't it?

Yes, the typo was corrected.

In Lines 266-267, 'Fig. 1' should be 'Fig. 2.'

The Reviewer is right. We corrected it.

What is the i_E used in the boundary conditions (72) and (76)? It was introduced after Equation (14) but not explained there.

Vectors $\mathbf{i}_E$, $\mathbf{j}_E$ and $\mathbf{K}_E$ are the unit vectors of frame S_E. We added a sentence in Sec 2.1 and another in Sec. 5.1 to remind their meaning to the readers.

In Figure 6, add the text to the y axis of the bottom figure.

We cannot add a text to the y axis because Fig. 6 presents two different quantities, which are explained in the legend.

Some materials in Section 3 can perhaps be moved to appendix to keep a smooth reading.

We agree that such idea improves the readability of the manuscript. Part of the material of Sec. 3 was moved to a new Appendix.

There are some typos and gramma mistakes in writing which can be easily removed.

We revised the manuscript a fix some typos.

Reviewer 3

Reviewer 3. The manuscript under review describes a numerical model of a rotary airborne wind energy convertor developed following the. approach of Cosserat theory, laboratory tests to determine mechanical properties and its dynamics under forced motions, and a comparison between the two models. The rotary airborne wind energy convertor seems to consist of a helical rotary system to transmit the torque from the airborne rotor to the ground station, but unfortunately no detailed drawings or information about the design is given.

We agree that the information provided in the original manuscript was not enough to fully understand the structure of the helix. We added a sentence in the first paragraph of Sec. 4 to make clear that the manuscript studies the rotor of SomeAWE Lab and a new citation where the reader can find data, drawing and videos of the helix.

Reviewer 3. The numerical approach is based on modelling the full helix (with complex internal structure) as a one-dimensional flexible, nonlinear Cosserat rod. The equation of motion is developed using vector analysis and discretized using a -Galerkin method. Unfortunately, even after trying to match the material properties with laboratory tests, the results show large discrepancies between the numerical model and the experimental tests. The authors' argue that the numerical model can qualitatively reproduce the experimentally observed resonance (leading to destruction of the model in the laboratory), but it remains unclear what features of the actual system the model can actually reproduce and what its limitations are.

This comment of the Reviewer is based on the mismatch between the resonance frequency predicted by our model (about 14 Hz) and the break-up frequency in the experiment (5 Hz). Both frequencies can be different because the collapse of the structure can happen before the resonance frequency is reached. We would like to emphasize that such a result is only one of the many contributions of our work (see our reply to the next comment).

Nevertheless, there are two aspects that should be emphasized and reinforce the usefulness of the new numerical tool presented in the manuscript. Firstly, the helix collapsed in the experiment at a frequency of 5 Hz, which corresponds to the frequency of the first lateral and torsional mode predicted by our numerical tool (a period of 0.2 s in Table 3). This is an example of why having access to numerical tools is helpful. The code provides the characteristic frequencies and, as it is well-known, external excitation of structures with frequencies equal or close to the natural frequencies of the structure should be avoided. Secondly, the fact that the structure collapsed at 5 Hz in the experiment does not mean that the maximum tension versus the forcing frequency occurs at 5 Hz in the experiment. Such a result just indicates that the helix was not able to sustain loads in the range of 170 N – 200 N under such a periodic driving condition. In other words, it may happen that Fig. 8 is mainly correct, but slightly shifted in frequency, and in the experiment we only observed the initial rise of the force with the frequency because the helix collapsed at a low load level.

Following this comment of the reviewer, we added more text in Sec. 5.3 and Sec. 6 to explain better the results and how they should be interpreted.

Reviewer 3. The manuscript is- mostly well written, but is difficult-to read since there is little motivation given for the presented developments.

Several sentences were added to the third paragraph of the Introduction to motivate better the work and highlight two important contributions: (i) a new physical model and numerical code capturing longitudinal, lateral and torsional dynamics and (ii) experimental test to characterize the axial, bending and torsional stiffness.

Reviewer 3. It is also surprising that the authors did not spend more time trying to match the experimental results. See below for more detailed comments on this. All in all, it seems that this paper is somewhat premature and I recommend that the authors test and investigate the numerical model more.

This manuscript is a summary of the work that we did in the three-year project GreenKite-2, which is now expired. We disagree with characterizing our work as "premature" and suggesting that insufficient time or effort was dedicated to preparing the best possible manuscript. Since RAWE machines are practically an unspoiled field of research (there is only a handful number of papers on the topic), we had to innovate extraordinarily in both the theoretical and experimental sides. For convenience, we summarize below the main contribution of the work.

A novel model for the helix of RAWE machines was developed from scratch by using Cosserat theory. It is the first time that such a theory is used in AWE. The model, which captures longitudinal, lateral and torsional waves and considers gravitational and aerodynamic forces, presents clear advantages as compared with previous dynamic models of RAWE machines. In particular, it is the first time that a dynamic model capturing the three effects is presented. The proposed numerical approach, i.e. finite elements, is also adequate because it yields a robust and efficient simulator. The correct implementation of the code was verified by conducting different tests. The simulator was used to investigate stationary solutions and their linear stability. The natural frequencies of the longitudinal, lateral, and torsion modes were determined and compared with basic results from beam theory. Afterwards, the simulator was used to study the nominal operation of a RAWE machine and analyse the performance of a new proposed controller. Tension level, angular velocities, response times and torques are aligned with the experience of SomeAWE Lab. Finally, the behaviour of the helix when the upper end is forced at a driving frequency was studied and an interesting resonance was found. The simulator was prepared to be published in open-source and it now belongs the LAKSA.

In our view, the set of result described in the above paragraph is adequate for a good theoretical paper. Nonetheless, we decided to expand the scope of the work, conduct experimental activities, and benchmark theoretical models with experimental data. The axial, bending and torsional stiffness of a RAWE helix were characterized by conducting tests in the laboratory. To the best of our knowledge, such activity was never performed for RAWE machines. They require to prepare and develop specific experimental and sensor setups for each of the tests. Interestingly, it was found that the torsional stiffness depends on the axial force and a novel hysteric behaviour for RAWE helix was found. Finally, a new experimental setup was prepared to mimic the upper-end forcing scenario studied with the simulator. It required to develop dedicated equipment with several motors and a controller as well as sensors to measure the relevant variables. Interestingly, it was found that the structure collapsed when the frequency matched the natural lateral and torsional frequencies predicted by the simulator (5 Hz) and the collapse occurred at a lower frequency than the one that produces the largest force in the simulator (about 14 Hz).

As pointed out by the Reviewer, our work does not provide answers to all the questions that it raised. However, in our view, it contributes to the field. Moreover, since we made the simulator open-source, other researchers will be able to use it for filling the gaps that any work, like ours, inevitably leaves.

Detailed comments by Reviewer 3.

1. Cosserat theory is mentioned, among others in the abstract, as the basis for developing the numerical model, but in the section on modelling no reference is made to it. I suggest the authors explain first, to the benefit of readers unfamiliar with this approach, what Cosserat theory actually is why it is needed here, and where the authors make use of it. In particular, it should be mentioned that the helix is modelled as a 1D Cosserat rod.

We agree. Some sentences were included in the first paragraph of Sec. 2.1.

2. The rotary wind energy machine for which this model is developed is never really shown or explained. Please include more details about it.

A new sentence and a new citation, where reader can find all the details about the design and the manufacturing of the helix, was added in the first paragraph of Sec. 4.

3. What is the motivation for modelling the complex internal structure of the helical transmission system with a 1D elastic rod, instead of a higher fidelity model? Why do the authors think that such a simple model is good enough to reproduce the important features (what are they?)-of the system?

To understand our decision, it is necessary to explain first previous work. The two alternatives found in the literature are the simple spring-disc model and the multi-spring/multi punctual mass model by Tulloch (see citations in the Introduction). The spring-disc model does not include axial and bending dynamics (just torsion) and the multi-spring/multi punctual mass model does not include bending. Therefore, one of our goal was to propose a higher fidelity model that would include the three relevant dynamics (axial, bending and torsion). Moreover, the Cosserat theory offers a more compact and understandable model (two coupled partial differential equations), where the characteristic wave velocities are evident. Additionally, the Cosserat theory naturally involve the axial, bending, and torsional stiffness that can be determined experimentally.

We would not characterize our model as “simple” or low-fidelity, because it already captured the three relevant motions of the helix. Increasing the fidelity of the simulator of our work irremediably needs to model the internal dynamics of every cross-section of the helix, which is assumed to be unshearable in this manuscript. In our view, such a modelling is not essential to simulate the nominal operation of RAWE machines. However, if one would like to predict the collapse of the helix in the experiment, then it would be indispensable to increase the fidelity of the simulator and consider shearable cross-sections.

Following this interesting comment by the Reviewer, additional explanations were added in the third paragraph of Sec. 5.3.

4. Why did the authors not use a standard flexible multi-body approach where individual elements of the helical structure are modelled directly, and in much more detail than with their approach? Or, why not use Cosserat rods for each member in the helix, instead of the full helix?

The approach proposed by the Reviewer is feasible, but the computational cost would be much higher. In this work we looked for a model with higher fidelity to the existing models for RAWE machines, but keeping moderate the computational cost.

5. It should be mentioned where the slenderness assumption is used (e.g. Eq. 16?)

We added some clarifications in the first paragraph of Sec. 2.1.

6. line 124- Why is it natural to make this assumption?

For a RAWE, the angular velocity of the Frenet frame with respect to the inertial frame is small as compared with the angular velocity of the helix (local frame) with respect to the inertial frame (or the angular velocity of the Frenet frame with respect to the local frame).

7. For the benefit of the reader, when citing books, such as Villaggio and Press et al, please indicate which chapter of the book is relevant here.

We added the pages of the relevant chapter.

8. Eq. 49: Is it correct that alpha_2,..., alpha_N-2 (mapped by the C_alpha matrix) are all supposed to be zero?

The typo was corrected. We thank the Reviewer for detecting it.

9. Figure 2: What is N for the experimental system shown in Panel d?

We are not sure to understand the comment. We do not find any N in panel (d) of Fig. 2.

10. line 313: If eigenvalues are pure imaginary, the system is not asymptotically stable and can exhibit bounded oscillations. Is this physically realistic?

The eigenvalues with zero real part correspond to free rotation of the helix as a rigid body. In a real RAWE machine there are always dissipative forces, like for instance friction at the attachment of the lower end, that damps the oscillations.

11. What assumptions were made about the damping in the numerical model?

The aerodynamic drag included in the model introduces some damping. As explained in the last paragraph of Sec. 5, internal friction also produces damping but it is smaller and it was ignored in this manuscript.

12. line 344: Why is the coupling with the rotor avoided? Why not couple the numerical model with a simple blade element momentum model for the rotor? The work of Wacker et al (https://doi.org/ 10.1088/1742-6596/2626/1/012011) suggests that this is feasible. In fact, this work also shows an analysis of a helical system by someAWE that the authors might want to consider and comment on.

We thank the Reviewer for letting us know about the interesting and recent work by Wacker et al. We added a sentence and a citation in the Introduction.

It is perfectly possible to couple our dynamic model of the helix with an aerodynamic model for the rotor. However, it was clear for us from the beginning of our project that such a coupling should not be done in this work. In our experience, the best practice to tackle difficult dynamic problems is to divide them in simpler pieces and analyse first each part separately. The scope of the manuscript is to understand the dynamic behaviour of the helix and prepare a simulator and experimental data to tackle more challenging problems. Since the code is open-source, we think that such a coupling will be implemented soon.

13. Figure 7: Please include a similar time domain plot for the numerical model, to allow comparison.

Trying to perfectly match the experimental results of Fig. 7 with the simulator is a fruitless exercise. Our simulator is a model that does not capture all the complexity of the experimental setup. Figure 7 is the result of a specific history for the driving frequency and, for the large oscillations before the collapse, the nonlinear response of the helix induces irregular (chaotic) oscillations. In case we would feed the simulator with the history of the driving frequency, we would observe a different signal for the tension that would corresponds to the transient behaviour from one forcing frequency to the other.

14. line 380: Why is Fig. 8 showing a bifurcation (and not simply a resonance)? This is mathematical concept with, a very precise meaning, are you sure that this what is happening here? Why?

In line 380 we said that Fig. 8 is a bifurcation diagram because we were plotting a parameter in the horizontal axis (the driving frequency) versus the maximum of a variables related to the state vector (the tension). It is a bifurcation diagram but we did not claim about the existence of any bifurcation point.

Nonetheless, we take the opportunity to comment an interesting point of Fig. 8, which is how the lower and upper branches of the resonance curve are connected. They could be connected by a stable branch not plotted in the figure because it is almost vertical, or they could be connected by an unstable branch involving bifurcation points. The study of such a potential bifurcation requires dedicated analysis and numerical tools that is clearly beyond the scope of this work.

15. Please discuss the possibilities to better match the numerical model with the experimental tests. Which parameters-are still available, or is the numerical model fully specified?

As explained in Sec. 5.3, the simulator relies on a set of simplifying hypotheses and also needs to be fed with inputs about the stiffness of the helix that has uncertainty. The model could be improved by using a torsional stiffness that depends on the instantaneous value of the tension. However, it is not evident how to do it because the torsional stiffness exhibit hysteresis.

In a future work, and if abundant experimental data is collected, it would be possible to tune the parameters of the simulator by using machine learning techniques and improve its predictive capabilities.

16 Figure 8: Please also show the standard deviations and the phases of the simulations for the different forcing frequencies.

We are not sure to understand the comment. For each forcing frequency, there is only one simulation, and it was carried out as explained in the paragraph below Fig. 7. After fixing the forcing frequency, we integrated the equations of motion forward in time, and plotted the maximum values of the tension in the long term (after the transient dies out). For the next value of the forcing frequency, we used as initial condition the final value of the state vector in the previous integration. For the full forcing frequency range studied in the analysis, the tension exhibited regular oscillations as shown in the inset of Fig. 8. Therefore, it is not possible to show any standard deviation or phase because the solution is unique.

A relevant question is whether, for each forcing frequency, it exists several attractors/stable solutions. Such a problem can be studied by using different initial conditions and integrating the equations forward in time for each forcing frequency. To explore it, and for some specific values of the forcing frequency, we used as initial condition the steady state that it exist for a steady upper end. We found the same solution shown in Fig. 8, which corresponds to initial conditions equal to the states of the helix for the previous values of the forcing frequency.

17 The manuscript needs a proper conclusion that sums up not only what has been done, but also what readers can learn from this work.

Following the suggestion. we expanded the Conclusions in the revised manuscript.

18 Reference Beaupoil (2017) is only an abstract. This is-discouraged and should not be done according to the uniform requirements for scientific manuscripts. Please reconsider.

The reference is a citation to the Book of Abstract of AWE Conference 2017. Unfortunately, there is not a paper or a conference proceeding. Since the literature on RAWE machines is scarce, we prefer to keep it because we think that it enriches the Introduction. Nonetheless, if the policy of the journal does not allow it, there is no problem on eliminate it.

We would like to thank the Reviewer of his/her careful revision of the manuscript and the provided comments. The changes implemented in the manuscript to address them certainly improve the quality of our work.