The authors present a framework to analyze the effects of uncertain blade properties on aeroelastic stability of a turbine. The study is very relevant to wind turbine community since turbine stability is one of the hot topics as turbine sizes grow. The work includes results from different aeroelastic tools, surrogate model generation, damping and mode determination from time signals and uncertainty quantification test cases. This wide selection of tools and complex steps make it hard to understand the details for the reader.

I have some comments that I hope can improve the paper:

- The references for the aeroelastic tools especially for their theory and capabilities are not presented in the study. Although I am familiar with some tools, I spend some time to understand the theory behind the tools I am not familiar. It is very time consuming and not always a successful process. I think, authors should give references for each tool's theory and capabilities, so readers can find the correct source for the tools.
    - We will cite the theory manuals or equivalent papers.
- HAWC2 and HAWCStab2 are presented as same tool but in fact they are separate tools which use different formulations for beam solvers. In table 1. HAWCStab2 is presented as the linearization tool for HAWC2 results but in fact, it can compute equilibrium point and linearize its own solution.
    - We will clarify that HAWC2 and HAWCStab2 are two separate tools and correctly describe the beam solvers.
- Setting the operational points in each tool is not explained well. HAWC2 can be run in a constant rpm, constant pitch point but I don't think you can do the same for all tools. So, these differences also need to be explained.
    - This issue was also raised by Leonardo Bergami. Please see our answer on his comments for further information. This will be addressed by adding a discussion on the open-loop configuration (or lack thereof) in section 2.2 of the paper.
- The beam property conversion might be another source of error when different tools are used and it might help explaining the differences given in Figure 9. The shear center location is actually hidden in coupling terms for the tools which uses 6x6 stiffness terms directly (e.g. HAWC2), on the other hand SC location is generally a direct input for other tools. Besides, different tools use stiffness and inertia values which are computed at different locations. Although I don't expect to see all these details in the paper, I would like to see available references related to this.
    - All beam properties modifications are done on the reference 6x6 matrices (BECAS output). This includes the stiffness reductions to obtain the critical reference condition and the uncertain parameter modifications for the case studies. The initial comparison between the models (section 2.3) serves as verification that this derivation was done correctly in all tools. This was described on line 270-274.
    - Note that we initially indeed modified all parameters in the tools individually. This resulted in significant differences. This was fixed by applying all modifications on the reference matrices and deriving the inputs accordingly.
    - The modifications of the 6x6 matrices and the derivation of the beam properties is based on the work by Hodges (Hodges, 2006). The implementation can be found in the python modules preprocessor.py and libCrossSection.py in the software repository (Verdonck, 2023b)
- Although there is a blade frequency comparison given in Figure 1, mode shapes are not mentioned anywhere. I wonder if all tools give similar torsion and flapwise motions for the 1st edgewise frequency.
    - In preceding studies, mode shapes were satisfactorily compared, but this was not repeated for the modified model in this study. With previous experience of having a good match in mode shapes in addition, we concluded to reduce the verification to the blade eigenfrequencies, static blade deformation, and static aeroelastic test cases only.
- The complex mode shapes and phase differences are not mentioned in stability analysis part. It would be interesting to see if different tools give similar phase differences from their complex eigenvalue analysis.
    - We agree that a mode shape analysis for the stability analysis might reveal additional insight on the instability mechanisms. The detailed analysis of these mechanisms was not the focus of this study, but rather the comparison of the sensitivities between the tools. A

detailed analysis of the stability can be subject of further work. This remark will be added to line 356-357 in the conclusion.

I have more specific comments below:

- Page 3, line 70 : "This effect can not be eliminated, but causes only a negligible periodic excitation" I think this is eliminated in HAWCStab2. Tower is an important element for system eigenvalues but can be assumed rigid for steady-state analysis and then real tower stiffness values can be included to eigenvalue and stability analysis. Of course this requires a lot of work for other tools but possible.
    - The proposed strategy is worth trying. However, as the reviewer already stated, it is a lot of work and can thus not be included in this paper. It may be subject of future investigations. We were not aware that this effect was eliminated in HAWCStab2. We will correct the statement in the paper.
- Page 3, line 85 : "A stiffness reduction of 70% in flapwise direction, 30% in edgewise direction, and 70% in torsional direction was required to accomplish the desired instability behavior." You could also alter the geometry such as prebend in flapwise direction, the swept in edgewise direction, aerodynamic center offset etc.
    - Yes, and it might have been better to include other parameters as well. However, as mentioned on lines 81-82, we established the critical reference condition in the beginning of this work, at a point where we did not have the automatic processes to modify all parameters. To keep the parameter space limited and manageable, we chose to limit ourselves to these three parameters. With the framework as it is implemented now, this could have been done better and faster. Nevertheless, we deem the critical reference condition to be suitable for the presented uncertainty quantification.
- Table 1: Although, HAWC2 uses MB, HAWCStab2 doesn't use multibody approach. It uses corotational formulation for blades. Interesting, Simpack-Aeordyn has 6x6 stiffness definition for tower but not for blades. Can you give some references? Also see my comments about references for tools theory/capabilities above.
    - We will correct the HAWCStab2 statement.
    - For Simpack-Aerodyn we use two different preprocessing tools for the tower and the blades. For the tower we use Ansys as preprocessor and import the tower as a single linear elastic body. Ansys uses the 6x6 properties directly. For the blades we use the Simpack internal beam description (SIMBEAM) in a multi-body implementation. At the moment of the work, SIMBEAM uses the engineering beam properties as input (6x6 input for SIMBEAM models is in development).
- Page 5, line 122: "only only" the same word typed twice.
    - Will be corrected.
- Page 6, Figure 2. Have you talked with FAST developers (e.g. Jason Jonkman) about tip deflection results? I haven't seen it in other studies such as IEA15MW turbine. You can check ORCAFlex IEA15MW report where OpenFAST is used for comparison.
    - This problem was raised as an issue and was worked on by Jason Jonkman and Andrew Platt. This issue is referenced in the paper (NREL, 2019).
    - Some commits have been added to the issue, but it is still open as of today (18.09.2023). The presented model was established before the latest commits by the OpenFAST developers.
- Page 7, line 148: "<todo>" missing reference.
    - Will be corrected, reference is (Verdonck et al., 2023a)
- Page 8, line 165: The difference for 1st EW BW seems the largest.
    - Will be corrected
- Page 9, line 192-194: I expect to see Bladed time domain and linearized results match much better. Is it related to DMD or Bladed time domain results?
    - This question concerns the comparison between the Bladed linearization and the Bladed DMD-postprocessed time domain simulation in figure 4c. This figure shows a good match in frequency and damping between the 2nd edgewise BW mode within its unstable range. The 1st edgewise BW mode matches well in frequency and shows a similar trend in damping over the wind speeds, but the magnitude of the damping of the time domain simulation is significantly higher for most of the domain. Note that the DMD markers for

the 1st edgewise BW are really small, indicating the small participation of this mode in the analyzed signals. We assume that the DMD postprocessing is sufficiently accurate in this case, and the main difference originates indeed from inherent differences between the linearization and time domain simulation. The results at operating points 9 and 14 m/s give the main reason for this opinion. The 1st edgewise BW mode is negatively damped in the linearization, but the time domain simulation is stable, i.e. there are no diverging signals. This is correctly identified by the DMD. The reason for this additional damping on the 1st edgewise mode in the time domain simulation is unknown.

- The sentence on line 193 missed a section, this will be corrected: "The 1st edgewise BW modal component is also identified, but its participation in the time signal **is significantly smaller** and damping ratio is higher compared to the linearization results and the other time domain simulations."

- Page 9, line 200-201: Any root cause of low damping values of SimPack? It is not particularly away from other tools in steady state analysis results. Any difference in unsteady aerodynamic part?
  - The root cause for the low damping values and the deviating frequencies in Simpack still has to be investigated. Simpack-AeroDyn uses a Beddoes-Leishman-like unsteady aerodynamic model, similar to the other tools. Unlike the other tools, we did not enable the dynamic wake model in Simpack. We verified that this is not the reason for the damping differences with a Bladed simulation without dynamic wake model.
- Page 10, Figure 4.: Is there any given small disturbance in time domain simulation to excite the modes, so that you can observe them clearly in the signals?
  - For the present work we were focused on the identification of the lowly damped modes in unstable operating points. For this use case we did not have to disturb the simulation, the interesting modes could be identified successfully with the DMD process. If the same approach is to be applied for stable time domain simulations, it might indeed be necessary to specifically disturb modes for a clear identification. This was not attempted in this work. A comment on the suitability of the DMD process is given on lines 351-353.
- Page 12, Figure 250-257: I think you explained the Sobol indices very well. Can you just elaborate the interactions? How should I interpret them?
  - The uncertainty quantification is based on a Sobol decomposition of the output quantity of interest, i.e. the function for the quantity of interest which depends on a certain number of uncertain parameters can be decomposed in a sum with terms depending on 1) none of the uncertain parameters, 2) only one of the uncertain parameters, or 3) combinations of two or more uncertain parameters. The variance of the quantity of interest can be decomposed by looking how each of these terms contributes to the total variance.
  - In the case of a variance-based uncertainty analysis with a PCE surrogate model, this becomes a lot easier to grasp. Assume we want to approximate a Quantity of Interest (QoI) of an unknown model with two uncertain parameters (X1, X2). Take following three basis functions for the polynomial expansion (X1, X2, X1*X2). This is just exemplary, depending on the uncertain parameters, different basis functions should be used. The PCE model will look like this: QoI(X1, X2) = C1*X1 + C2*X2 + C3*X1*X2. The unknown coefficients (C1, C2, C3) are determined by a least-squares regression (or something equivalent) based on the samples of the model. The variance of a polynomial is the sum of its squared coefficients. The total variance of the PCE model would therefore be (C1**2 + C2**2 + C3**2). To compute the first order Sobol index of uncertain parameter X1, we gather the polynomials which **solely** depend on X1 and compute the ratio of their variance with the total variance. This would be computed as C1**2 / (C1**2 + C2**2 + C3**2). Similarly, the first order Sobol index of uncertain parameter X2 would be C2**2 / (C1**2 + C2**2 + C3**2). For the total Sobol index of uncertain parameter X1 we gather all polynomials which depend on X1 and compute the ratio of their variance with the total variance. This would in this case be (C1**2 + C3**2) / (C1**2 + C2**2 + C3**2). Similarly, for X2 the total Sobol index would be (C2**2 + C3**2) / (C1**2 + C2**2 + C3**2).
- Page 15, Figure 6: HAWC2 total is more than 1 for total Sobol indices. Is it correct?
  - Yes, the sum of the first order Sobol indices can be maximum 1, the sum of the total Sobol indices will be at least 1.

- Continuing the example from the last question. The sum of the first order Sobol indices would be (C1**2 + C2**2) / (C1**2 + C2**2 + C3**2). This will always be smaller than 1, or exactly 1, if there is no interaction between the uncertain parameters (C3 = 0). The sum of the total Sobol indices would be (C1**2 + C2**2 + 2 * C3**2) / (C1**2 + C2**2 + C3**2). This will in the very least be equal to 1. The variance contribution of polynomial term X1*X2 counts double, as it contributes to the variance contribution of both X1 and X2.

- Page 17, figure 9: The SC location is very critical for aeroelastic stability and Damage Equivalent loads in flapwise direction. I expect, it should also have substantial effect on EW direction stability. I don't understand why you don't observe it in the tools other than HAWC2 and HAWCStab2. Can it be related to my comment above about stiffness conversion? I might be wrong, but it would be great if you can add some physical explanations about the differences and observed results?
  - As described above, the stiffness modification and the uncertain parameter modifications are done directly on the 6x6 reference matrices. We believe that our verification proves sufficiently that the derivation of the input for the different tools is implemented correctly. We therefore believe that any difference in the uncertainty quantification study originate from the fully aeroelastically coupled simulation models themselves and are not caused by differences in the input generation.
- Page 18, figure 10: I expect HAWC2 and HAWCStab2 damping curves with opposite slope, so that the damping decreases as the SC moves towards TE. Again, I might be wrong, but it would be great if you can add some physical explanations about the differences and observed results?
  - The focus of this work was the identification of differences in the sensitivity between different tools. Investigating the underlying physical explanations for these differences was beyond the study's scope. Nevertheless, it would be great to answer this question in combination with a study on the detailed instability mechanism in future studies.