Reply to reviewer 2

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The authors would like to thank the reviewer for the thorough review and many good points made, which will improve the paper significantly. In the following, the reviewer's comments will be given in black font, while the answers from authors are given in blue.

The paper presents a field data-based validation of an aero-servo-elastic solver coupled with LES simulations. The topic is very interesting and relevant for the readers of Wind Energy Science. The article is well-written. The authors provided detailed explanations regarding how to couple the LES tool YALES2 with the aeroelastic tool BHawC.

I only have some minor comments as follows:

1. page 2. Please define 'CFD'

   The definition was added.

2. page 2. '… HawC2…" should be HAWC2

   This has been corrected.

3. "To the authors' knowledge, (Gremmo et al., 2022) is the only reference where a code with these capabilities has been reported." Can the authors elaborate on what capabilities they have? As far as I know, there are also examples of coupling between EllipSys and HAWC2, which can be easily found in the literature.

   This statement was not referring to the Elastic-Actuator-Line kind of coupling, which indeed led to several publications in the litterature already. The authors were rather trying to emphasize that YALES2-BHawC can additionally embed the actual logic of a SGRE wind turbine controller, through the dynamic loading of a specific library. This part of the introduction was slightly reworded to clarify it. An additional reference has also been added in the introduction section of the manuscript, regarding an existing coupling between EllipSys 3D and HAWC2, which was indeed missing.

4. page 8. "First, it cannot work without the structural solver also having at least the blade element theory implemented within. In other words, a standalone structural solver is not suited for this strategy." Can the authors elaborate more? Why wouldn't the standalone structural solver without the blade element work?

   The use of a coupling approach, where the flow velocities are fed back to the structural solver, implies that the aerodynamic loads need to be calculated internally using these velocities. As a consequence, the structural solver needs to integrate a BEM module to convert the velocity into an aerodynamic force, and so "... a standalone structural solver is not suitable for this strategy". The sentence has been edited to improve readability

5. Figure 8. I don't really understand this figure. It seems the mean error is so small? Is it true?

   The error is indeed expected to be so small. Here two configurations are compared and the difference lies in the structural model:

   (a) The YALES2 configuration has a fully rigid structure, which is infinitely rigid.

   (b) YALES2-BHawC has a very high structural stiffness (cannot be set to infinity for numerical reasons).

   The point of this graph is to emphasise that, despite this difference, a meaningful comparison can be made because the residual flexibility of the structure is very small, resulting in a very small difference (i.e. nondimensional mean error) in particle position between the two configurations. The caption of Figure 8 is modified to explicitly mention the two configurations.

Once again, the authors would like to thank the reviewer for providing very constructive comments.