Response to Reviewer #2 (anonymous referee)

General Remarks

"The analysis presented in this paper is well worth investigation and adds new knowledge for wind turbine blade designers. However, a major revision to the paper is required, due to the structure of the paper and some investigations that are missing. The specific changes requested are:"

Statement #1:

"The results in this paper are based on a finite element analysis of a wind turbine blade. However, there are very little details of this model and no validation. Both of these should be added to demonstrate that the authors are starting with an accurate model."

> **Response:** The modeling strategy and the modeling tool were validated in detail using the SB2-DemoBlade in the following reference that was also referenced in the manuscript:
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> > Noever Castelos, P., Haller, B., and Balzani, C.: Validation of a modeling methodology for wind turbine rotor blades based on a full-scale410 blade test, Wind Energy Science, 7, 105–127, 2022.
>
> The particular parameterization of the blade models were thoroughly verified using models that were internally provided by the blade designers. This information exchange is not citable, since it is based on unpublished data. We ensured that the natural frequencies, mode shapes, the blade masses and the positions of the center of gravity match well. Mesh convergence studies were carried out to guarantee an accurate representation of the global blade response. The mesh was further refined, especially in the evaluation region and at the trailing edge to obtain mesh convergence not only with respect to the aforementioned outputs and additionally deflections, but also with respect to stresses, especially in the trailing edge adhesive. The models used were thus as accurate as possible, and there are no doubts that the results are reliable. We would like to emphasize that we are aware that good models are essential when analyzing model output data. Hence, the procedure described above is regularly exercised whenever we do finite element modeling, at least when we have reference data available, which is not always the case.
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> We have added some more details on activities related to model quality in the text, which reads:
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> > "The modeling strategy was validated in detail using the SB2-DemoBlade (Noever Castelos et al., 2022). Mesh convergence studies were carried out with respect to static deflections, natural frequencies, mode shapes, blade mass, and location of the center of gravity for all blades to ensure converged and reliable solutions. Figure 3 shows coarse versions of the blade meshes (the used FE meshes were too fine for visualization) of all three blades, highlighting the size differences."

> Detailed validation is normally not documented in similar publications throughout literature, unless the core of the paper is the finite element modeling itself. In our manuscript, the finite element method is utilized as a state-of-the art tool to calculate stresses in complex structures. The FE modeling itself is not the focus of this paper, but the degree of non-proportionality in the trailing edge adhesive. In our opinion, documenting the blade models in more detail than is done in the revised version of the manuscript, including validation and verificiation, does not provide significant added value, especially given the fact that this would result in several additonal pages of text and figures. Since the paper already consisted of 20 pages in the submitted version (and has even more pages in the revised version), adding this content is – in our opinion – not reasonable. We hope that this finds the reviewer's agreement.

Statement #2:

"From the outset of the paper, the authors acknowledge that "The results do not reveal any correlation between the degree of non-proportionality and the blade size. General conclusions are hard to draw, as the blade response does not only depend on the turbine size, but also on the blade design philosophy." However, a very limited selection of blade (3) are used, two of which are of similar length. Therefore, I would suggest adding more blades to the analysis in order to draw better conclusions for the trailing edge design consideration that is presented in the paper."

> **Response:** We are indeed a bit surprised. We analyzed three different blade designs, which are already two more than usually analyzed in similar research papers. Of course, it would be interesting to see if other blades show similar or dissimilar trends, especially industrial blades (that we don't have access to). We created detailed finite element models, did the analyses, postprocessed the data and documented everything in a detailed and comprehensive fashion. For this paper, three blades are more than enough in our humble opinion.
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> Moreover, there are multiple factors influencing the stress response in the blade: The choice of the fatigue analysis methodology (global equivalent stress vs. critical plane approach), the choice of the equivalent stress criteria, the quality of the material database, the general design policy of the blade designer, the design targets with respect to aeroelastic performance and cost, the risks the designer is willing to accept, the overall turbine concept, the dynamic behavior not only of the blades but also of the turbine, etc. Hence, stating that a general conclusion is not possible is reasonable and honest. Even if we analyzed three additional blades (that we don't have the data of, by the way), there is a high probability that general conclusions are still not possible. And that is one of the outcomes that is also important to accept: The non-proportionality in a trailing edge adhesive joint (and other adhesive joints and sub-components of the blade) has to be analyzed for every blade and turbine individually.
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> To clarify the dependency of the outcome on a variety of influencing factors, we have included the following text in the conclusions:
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>> "A correlation between the degree of non-proportionality and the blade size was not substantiated. The reason is that the stress response in a blade depends on a large variety of influencing factors, such as the choice of the fatigue analysis methodology (global equivalent stress vs. critical plane approach), the choice of the underlying equivalent stress criteria, the quality of the material database, the general design policy of the blade designer, the design targets with respect to aero-elastic performance and cost, the risks the designer is willing to accept, the overall turbine concept, the dynamic response not only of the blades but also of

> > the entire turbine, different inflow conditions of small and large rotor diameters, etc. Hence, the degree of non-proportionality in a rotor blade has to be analyzed for each turbine individually. "
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> We believe that three blades are enough for a journal paper and kindly ask for your agreement. Additional blades can certainly be subject of follow-up research, and we are looking forward to extend the study presented in this paper in the future.

Statement #3:

"The conclusion largely summarises the work carried out in the paper. A paragraph on the impact of the study should be added to Section 4."

> **Response:** The paper sensitises blade designers that there is always at least some degree of non-proportionality in trailing edge adhesive joints. It is also exlained in the paper by referring to literature with experimental evidence that using a global equivalent stress approach can result to highly non-conservative estimates of the fatigue life of adhesive joints. Hence, the paper can have an impact on the overall structural reliability of wind turbine rotor blades, as the non-proportionality factor can be used as a metric for the choice of an appropriate and reliable fatigue analysis framework, meaning that time-consuming methods such as the critical plane approach needs to be applied only in case the degree of non-proportionality implies that.
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> We have added the following paragraph to section 4 for clarification:
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> > "The non-proportionality factor as proposed in this paper can be utilized to quantify the degree of non-proportionality in rotor blade adhesive joints, and it can be adapted for other materials and sub-structures as well. The blade designer is then able to select an appropriate fatigue analysis methodology, e. g., the critical plane approach only for spots with a high degree of non-proportionality and the global equivalent stress approach elsewhere. As the wrong choice of the fatigue analysis method can be non-conservative, the non-proportionality factor can thus serve to improve the overall structural reliability of wind turbine rotor blades. "

Statement #4:

"Although much of these details are included throughout the paper, please add a methodology section that clearly outlines the study aim, objectives, materials, methods and overall methodology."

> **Response:** A methodology section has been introduced after the introduction.

Statement #5:

"Section 3 is 8 pages long – please divide it into subsections to make it easier for the reader."

Response: Thank you very much for your comment. We were in fact also struggling with the length of the former section 3 and have divided it into different sections and subsections.

The former section 3 has been re-structured to improve the reading experience. It has been divided into two sections, which are *4 Application examples* and *5 Results and discussion* (number is higher due to the inclusion of the Methodology section). Hence, the *Conclusion* section has become section 6.

The results and discussion section has been divided into the following subsections: *4.1 Non-proportionality in trailing edge adhesive joints*, *4.2 Correlation with stress time series*, and *4.3 Weighted mean non-proportionality*.

In our opinion, the subdivision section 3 improved the readability significantly and thank the reviewer again for the comment.