Non-proportionality analysis of multiaxial fatigue stress histories in trailing edge adhesive joints of wind turbine rotor blades

RESPONSE TO REVIEW

Dear Madam or Sir,

We would like to thank the reviewers and editors for the professional, detailed and yet fast review of our manuscript. We are very grateful for your work and the consideration of our manuscript for the Wind Energy Science journal. Besides generally revising the text to eliminate language errors and typos, we thoroughly addressed the comments raised by the reviewers and hope for the reviewers' approval.

Thank you again to the reviewers, as your comments helped to significantly improve the manuscript and to better present its content. We prepared responses to each of your concerns in the supplementary file and look forward to the upcoming steps.

The Authors:
Claudio Balzani
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1 Reviewer #1

Comment #1:

"Based on the non-proportionality factors of the three adhesive joints, the authors concluded that "each blade shows significant degrees of non-proportionality that should not be neglected in fatigue damage analyses". It is suggested to specify an upper limit so that when the non-proportionality factor exceeds this value, its influence on the fatigue analysis result should be considered."

> **Response:** Thank you very much for this comment. In fact, the previous formulation was not covered by the findings presented in the paper. At the current stage of research, we cannot derive an upper limit as proposed by the reviewer, as we did not look at the fatigue damage yet, but on the qualitative features of stress time series in a trailing edge adhesive to sensitise the reader that non-proportional fatigue may be something to look into for the design of adhesive joints. Further research is required to derive such threshold value of the non-proportionality factor. The evaluation of the fatigue damage is work in progress, and we hope to be able to publish results soon in a follow-up paper. We thus have rephrased the final sentence of the abstract and replaced it by the following formulation:
>
> > *"Each blade in this study shows significant degrees of non-proportionality that may be important to take into account in a fatigue analysis."*
>
> In this way, we do not claim that the non-proportionalities found in our study need to be accounted for, but only that it may be important (or may be not). We have also added a sentence that refers to necessary future work in the conclusions (line 467):
>
> > *"Further research is necessary to find a threshold above which non-proportionality needs to be considered."*
>
> It was already mentioned in the conclusions that "Future work will have to clarify in how far the non-proportionality factor can be utilized as a decision-making metric in choosing an appropriate fatigue analysis procedure [...]" (see last paragraph of the conclusions section, lines 467–470). The idea here is that, depending on the material, a threshold non-proportionality factor may be found under which a global equivalent stress approach is sufficient, while above that threshold the critical plane approach needs to be used. This matches the reviewer's comment on an upper limit and will be subject of future work.
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> In addition, we checked the text and weakened related statements throughout the manuscript. E. g., at the end of the second paragraph of section 5.1 (lines 335–336), we now state:
>
> > *"Hence, the non-proportionality may have a significant influence on the damage evolution, at least in the regions where higher degrees of non-proportionality are present."*
>
> For the reviewer's reference only, we would like to draw the attention to the fifth paragraph of the introduction (starting on line 54), where we discuss that using global equivalent stress criteria can be unconservative and refer to a recent work of Kuhn et al., 2023. Therein, a rotor blade adhesive was investigated under bi-axial loading (shear stress and normal stress). Non-proportionality was introduced by a phase shift between the

> two stress components. It was shown that the global equivalent stress approach significantly underestimated the fatigue damage already at quite small phase shifts, which corresponds to a small non-proportionality factor. Hence, it is very important to choose a valid fatigue analysis framework when non-proportionality is involved. However, as mentioned, more research is necessary to obtain enough evidence for a general conclusion on the threshold non-proportionality factor as described above.
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> For the time being, we propose to weaken the formulations in the abstract and throughout the manuscript as described above to align with the conclusions, and to add investigations on fatigue damage later. We kindly ask for the reviewer's approval.

Comment #2:

"Following the previous comment, I think it needs more evidence on to what extent will the non-proportionality impact the fatigue analysis result, especially for the blade trailing edge adhesive joint. If the fatigue life of the adhesive, with and without non-proportionality, can be compared, that would be great."

> **Response:** We absolutely agree. That would be great indeed and is exactly what the authors aim at in the near future. However, the aim of this paper was to evaluate the degree of non-proportionality in rotor blade adhesive joints first and see if somewhat relevant magnitudes can be observed. To the best knowledge of the authors, this was not done by other authors before and thus is valuable information by itself.
>
> The next step actually is to evaluate the fatigue lives for the adhesive joints with global equivalent stress criteria and critical plane approaches and to investigate the differences. Hopefully, we will then be able to find a threshold value or upper limit for the non-proportionality factor as a decision-making tool for choosing an appropriate fatigue analysis framework.
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> Anyways, as mentioned, this is work in progress and will be published in due time. We kindly ask for the reviewer's agreement to first publish the stress time series analysis presented in the manuscript and extend the findings by the fatigue lives later.

Comment #3:

"Fatigue performance is generally related to the material properties. Given that the input stress is from the finite element analysis, will the material properties (Elastic modulus) play a role in the non-proportionality?"

> **Response:** Generally speaking, yes, the material properties can have an influence on the non-proportionality, especially if the ratio between different stiffnesses change, e.g., the ratio between the Young's modulus and the shear modulus. This will influence the ratio between different stress components, which, by definition, will impact the non-proportionality factor. This is an interesting comment and motivates future work on the effect of elastic properties on the non-propotionality in adhesive joints and the resulting fatigue life. However, as we have not yet done the exercise, it feels like too much of a speculation to include that discussion in the manuscript already. We kindly ask for your agreement to first do the exercise and then come back to this point in a future publication.

The stiffness of the surrounding structure may play an even more important role in the non-proportionality within the adhesive. If the trailing edge structure is stiffened in longitudinal direction, e. g., by including uni-directional material, the longitudinal stress in the adhesive will decrease, increasing the relative importance of other stress components and thus increasing the non-proportionality factor. This is another aspect the authors try to look at in the near future. For the time being, we have included a discussion in this respect (also including thermal residual stresses) in the last paragraph of section 5.2 (starting on line 401).

2 Reviewer #2

Comment #1:

"Abstract line 2-3: Beside all the mentioned causes, blades are loaded in fatigue majorly from their revolution and the change direction of their dead weight."

> **Response:** This is true for lead-lag bending of the blade. However, fore-aft bending of the blade is governed by wind shear, turbulence, and fore-aft motion of the tower top. The term "aero-(hydro-)servo-elastic behaviour" is generic in nature and encompasses *all* types of loads that occur in the dynamics of a wind turbine, and thus seems to be an appropriate formulation for the introductory part of the abstract. It actually also includes the blade vibrations associated with the rotor revolution and the dead weight of the blade.
>
> In the introduction, we refer to the fact that "[...] lead-lag bending [...] is governed by the rotor blades' inertia" (see lines 33–34). Moreover, in the discussion of results, namely in section 5.2 "Correlation with stress time series", we mention that the oscillation of the longitudinal stress for the DTU blade "is due to a high edgewise bending moment originating from the blade mass in rotation" (lines 329–330). Similar information is included for the IWES blade (starting on line 373) and the SB2-DemoBlade (starting on line 383). Hence, the information you were asking for was already included in the manuscript.
>
> It is the core of the paper to analyze the stress time series as they are, including all stress components, whatever physical effect they are originating from (at least all physical effects that are usually accounted for in a loads analysis). Given the context of the manuscript, we prefer not to focus on one effect in the blades in the abstract already, but discuss the stress components in detail in the discussion of results, which we actually did.
>
> We thus kindly ask for your agreement to leave the abstract formulation as it was.

Comment #2:

"l 39 lead-leg"

> **Response:** We corrected the typo.

Comment #3:

"l 57 approach"

> **Response:** We corrected the typo.

Comment #4:

"l 79 The blades were chosen as the first two (improve language)"

> **Response:** The formulation was actually not wrong, but maybe a bit unusual. We hence rephrased the sentence to improve readability, which now reads (lines 173–174):
>
> > *"The blades were selected because the first two are similar in size and the third is significantly smaller."*

Comment #5:

"l 110 which product? There is a noun missing."

> **Response:** The first factor in the product is the largest principal strain in each time instance denoted by ε_1. The second factor is the angular deviation between ε_1 and the maximum ε_1 observed in the entire time series. Although this was correctly expressed in the manuscript, the formulation was obviously unclear. We thus rephrased the sentence, see lines 81–83, which now reads:
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> > "Following a similar idea, Itoh et al. (1995) evaluated the temporal integral of the product of the largest principal strain in each time instance (denoted by ε_1) and the angular deviation between ε_1 and its time-related maximum in the time series."

Comment #6:

"The document does not adhere to a standard paper format, mixing different parts. Thus, segments of Chapter 3 should be included in the introduction, while the mathematical analysis should be placed under the Methodology section.

Moreover, the introductory paragraph seems misleading, as it does not accurately convey the paper's focus. The paper's essence is not about the importance of non-proportionality on the material level, which is majorly discussed in the intro; instead, it focuses on the non-proportionality of wind turbine blades and derives an equivalent value for their life span. Thus, the intro must focus on state-of-the-art research on the latest topic.

Additionally, it is used to add the motivation at the end of the introductory paragraph so the reader can follow what the research is about.

Thus, a major restructuring is recommended for chapters 1, 2, and 3."

> **Response:** We are grateful for your valuable feedback. In response, we have revised and restructured the introduction as demanded by the reviewer.
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> We acknowledge that we initially faced challenges in differentiating between the mathematical derivation and the historical evolution of non-proportionality factor formulations. In light of your insightful comment, we have integrated the historical development into the introduction and the mathematical derivation into section 3.
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> We regret to inform you that we were unable to incorporate your recommendation to integrate the mathematical elements into section 2, entitled "Methodology." The rationale

for this decision is that another reviewer requested a methodology section that explicitly delineates the study's objective, intended outcomes, materials, methods, and overall approach. This request, outlined in the fourth bullet point of the review included in the open discussion (see https://wes.copernicus.org/preprints/wes-2023-167/#RC2), ultimately influenced the inclusion of section 2. It should be noted that an outline does not cover a mathematical formulation. Accordingly, the mathematical formulation was retained in Chapter 3, but the section title was modified to "Mathematical formulation of the non-proportionality factor" to more accurately reflect the content of that section. This appears to be a reasonable compromise, and we respectfully request your approval.

The general section on fatigue analysis methods, which presents methods based on global equivalent stresses or the critical plane concept and offers a comparative analysis of these methods, has been retained in the introduction. This section is included for the following reason: it provides a rationale for addressing the quantification of non-proportionalities in stress time series, which appears to be a crucial aspect in the context of the research objectives.

As previously stated, we have provided an overview of formulations of non-proportionality factors. A section on fatigue damage in trailing edge bondings was subsequently included, which to our understanding was meant by the reviewer's comment. It should be noted that there is a paucity of previous work on non-proportional fatigue damage in trailing edge bondings in the literature, which is now thoroughly deduced from the state of the art on fatigue damage in rotor blade trailing edges. Furthermore, to the best of our knowledge, there are no contributions at all to the analysis and quantification of non-proportionalities in the stress time series of adhesives in rotor blade trailing edges. Consequently, the core of the work was not investigated before by other groups, which makes the paper and its outcome novel and original.

In conclusion, we derive the research objectives of the paper from the state of the art presented above and move on to the following chapter.

In order to enhance the readability of the text, we have introduced subsections in the introduction, which has become more extensive in the revised version and would lack of structure otherwise.

We kindly ask for the reviewer's approval.

Comment #7:

"Estimating the manufacturing-induced residual stresses is essential for the bond line stress analysis. Neglecting them will result in erroneous assumptions and conclusions, diverging from the material performance in the real structure and adding complexity which can be misleading."

Response: We concur. The introduction now includes an explanation of the significance of thermal residual stresses (lines 129–141). Nevertheless, accounting for thermal residual stresses would in fact result in an increase in the degree of non-proportionality, as the constant thermal residual stress in the longitudinal and chordwise directions would be superimposed by cyclic stress components other than those of a thermal nature. If a suitable blade design is employed, in which the trailing edge is stiffened to prevent the formation of transverse cracks, it may be anticipated that the non-proportionality factor will increase. Consequently, the exclusion of thermal residual stress is a conservative approach with respect to the non-proportionality degree in the stress time series. This is discussed in a paragraph at the end of section 5.2 starting on line 362. We thus conclude that neglecting thermal residual stresses in our study is not falsifying the outcome,

> misleading, or leading to erroneous assumptions and conclusions, but is rather a conservative estimate of non-proportionalities. However, it was definitely worth including a related discussion, to make clear that we are aware of the presence and importance of thermal residual stresses.

Comment #8:

"Figure 7. The results of the finite element (FE) analysis should be interpreted with caution. Using the boundary elements of the bond line for stress analysis might affect the conclusions, as the stresses in these regions do not converge with a finer mesh. To implement the results accurately, a few rows of elements should be discarded. Similar cases can be found in the literature."

> **Response:** We concur. It is widely acknowledged that the stresses do not converge with the mesh at singularities such as the corners of the adhesive (sharp corner and/or discrete transition to another material). The aim of Figure 7, however, is to show the qualitative distribution within the cross-section and to relate it to the loads acting on the blade.
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> In the subsequent analysis based on Fig. 8 (and also before in Figure 5), the outermost elements were excluded from the data to ensure the validity of the drawn conclusion. We have added a short comment on lines 360–361 that reads:
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> *"Note that we neglected the outermost row of elements in order to ensure not to consider stresses close to singularities associatedwith geometric corners or discrete jumps of material properties."*
>
> We have also added an explanatory paragraph in lines 439–444, which reads:
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> *"It is important to exercise caution when interpreting the stresses in the corners of the adhesive, as it is widely acknowledged that stresses do not converge with the mesh density in such singularities (geometric sharp corner and/or sudden jump in material properties in the interface between adhesive and shell laminates). Given that the non-proportionality factor is a direct consequence of the stress state, it is similarly prudent to exercise caution when considering the non-proportionality factor in the corners. However, in the subsequent analysis, the non-proportionality factor in the outermost elements of the adhesive were excluded from the results, ensuring that the conclusions are valid and not affected by spurious stress components."*
>
> The authors are generally aware of this issue and apologize for not addressing it properly. We hope that it is now sufficiently included in the manuscript and kindly ask for the reviewer's approval.

Comment #9:

"To the reviewer's knowledge, there is no literature evidence that the trailing edge adhesive cracks develop in an inclined plane other than the one transverse to the blade length. It is recommended to include photographic evidence or references that demonstrate a maximum stress plane different from the transverse plane."

> **Response:** We agree. As previously stated in the introduction, cracks in trailing edges may manifest as either longitudinal cracks (also known as debondings) or transverse cracks on a plane perpendicular to the trailing edge.

The underlying cause of the transverse cracks is the inadequate design of the trailing edges. These are typically designed for shear stress transfer in the adhesive joints. Consequently, transverse fatigue cracks emerge due to the high longitudinal strain in the trailing edge and the resulting high longitudinal normal stress in the adhesive. Given the dominance of the longitudinal normal stress, the critical plane is evidently the plane perpendicular to the longitudinal direction. This results in transverse cracks, as documented in the literature.

Integrating additional unidirectional material in the vicinity of the trailing edge could serve to stiffen it, thereby reducing the longitudinal strain and the associated longitudinal normal stress. This would help to prevent the formation of transverse cracks. However, it is important to exercise caution to ensure that the trailing edge is not excessively stiff, as this could lead to complications related to thermal residual stresses.

A reduction in the dominant longitudinal normal stress would result in an increase in the relative contribution of other stress components, leading to an increase in the non-proportionality factor and, consequently, an inclination of the crack surface, as the critical plane would undergo a change in its orientation. This phenomenon cannot be demonstrated at present (neither by potographs of field damages nor by referencing respective literature, as they do not exist), because blades are currently not designed in this manner. However, a future study dedicated to this topic could elucidate the impact of blade design on the non-proportionality and the resulting critical plane orientation.

A discussion on this can be found in a paragraph at the end of section 5.2 starting on lines 362. We also revised the conclusions on the degree of non-proportionality accordingly, see lines 485–492.

We believe that we have now addressed this point sufficiently, apologize for not doing so before, and kindly ask for the reviewer's approval.