We appreciate Referee 2 careful reading of our manuscript and your candid feedback. We have taken your concerns seriously and revised the manuscript accordingly.

S.No. 1: Referee 2 comment: Lack of physical interpretation and consequences of BTC

The manuscript does not clearly explain the direct consequences of bend–twist coupling in the context of ultra-long wind turbine blades. The potential advantages and disadvantages of BTC (e.g., load alleviation, aeroelastic stability, flutter margins, controllability) are neither discussed nor quantified. As a result, the reader is left without understanding why the reported coupling trends are important or how they affect blade performance.

AC Response: We acknowledge this concern. In the revised manuscript, we expanded Sections 2.2–2.3 to better connect stiffness-matrix coupling terms to aeroelastic tailoring, damping, and stability relevance based on prior literature, while clarifying that direct quantification of these effects for the present blade set would require dedicated aeroelastic simulations beyond the scope of this revision.

Changes in manuscript: Section 2.2–2.3.

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S.No. 2: Referee 2 comment: Incorrect and confusing presentation of the stiffness matrix coupling terms in Figures 4, 8, and 11.

For a symmetric 6×6 stiffness matrix, there are only 15 independent off-diagonal coupling terms, yet Figures 4, 8, and 11 display 30 terms, effectively duplicating symmetric entries. This duplication is not explained and significantly complicates interpretation. This represents a serious presentation flaw, and it took considerable effort to realize that the results were repeated rather than distinct.

AC Response: We agree with the reviewer that the symmetry-related duplication in the normalized coupling matrix plots should be clearly explained. In the revised manuscript, we clarified that the 6×6 stiffness matrix is symmetric, so the full visual matrix includes mirrored off-diagonal entries for completeness of visualization, whereas the RMS coupling metric is computed using unique off-diagonal pairs only (i.e., without double counting). We have revised the methodology text/captions accordingly to avoid ambiguity.

Changes in manuscript: Section 5.0.

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S.No. 3: Referee 2 comment: **Unclear link between stiffness coupling and upscaling.**

The manuscript itself states that each sectional stiffness matrix results from composite layups, fiber orientations, and thickness distributions, which are design choices, not purely scaling effects. Consequently, it is unclear whether the observed differences between the three reference turbines reflect upscaling trends or merely different design philosophies. Comparing only three reference turbines is insufficient to disentangle these effects.

AC Response: We agree that differences across the three reference turbines cannot, by themselves, establish general scaling laws or fully disentangle scaling-driven effects from design-driven choices (layup, fiber orientation, thickness, etc.). In the revised manuscript, we clarified that the sectional matrices are analyzed as published reference inputs and interpreted as a comparative matrix-level mapping across three public reference designs, rather than as universal scaling rules. We also revised the scope language to emphasize this limitation.

**Change in manuscript:** Section **4.1**.

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S.No. 4: Referee 2 comment: Speculative and unsupported interpretive statements.

Several key claims are made without evidence or analysis, for example:
(i): "This redistribution signifies a maturation in structural tailoring.”
(ii): "Designers have successfully spread the coupling effects to achieve load alleviation benefits."
(III): "Smoothing of the coupling landscape is a critical evolutionary step for ultra-long blades."
These statements are speculative and are not supported by aeroelastic analysis, sensitivity studies, or design optimization results. Moreover, it is not demonstrated that “spreading the coupling” is beneficial, nor what trade-offs it introduces.

AC Response: We agree that matrix trends alone do not demonstrate aeroelastic benefits. We did not perform new aeroelastic simulations in this revision (due to scope/time constraints), and therefore revised the manuscript to remove unsupported interpretive claims and limit conclusions to matrix-based coupling descriptors.

**Change in manuscript:** Abstract , Section 5.0 , and conclusion.

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S.No. 5: Referee 2 comment: Weak and casual cross-scale comparison using the RMS coupling metric.

The comparisons based on the “overall coupling score, K_RMS (s)" presented in Figures 7, 10, and 13, are not convincing. Several of the claimed differences are not evident from the plots, and the physical meaning of the RMS metric is not established. Without further analysis linking this metric to aeroelastic response or design outcomes, the comparison remains superficial

AC Response: We thank the reviewer for this important comment and agree that the RMS coupling metric should not be interpreted as direct evidence of aeroelastic performance or design benefit. In the revised manuscript, we strengthened the theoretical basis of the normalized coupling coefficient and explicitly define the RMS coupling quantity as a dimensionless screening indicator for comparative structural-coupling intensity. We also clarify that linking this metric to modal damping, flutter margins, or design outcomes requires dedicated aeroelastic simulations, which are outside the scope of the present revision. Accordingly, we have revised the discussion/conclusions to avoid over-interpretation.

**Change in manuscript:** Section 4.2.1 , 4.2.2 , 5.0, and conclusion.

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S.No. 6: Referee 2 comment: **Code availability claim is incorrect.**

The manuscript states that the analysis code is available. However, no code is provided or accessible. This contradicts the claims made under "code availability" and undermines the reproducibility of the study.

In its current form, the manuscript primarily reports stiffness matrix data of three reference turbines with minimal analysis. This level of reporting is insufficient for wind Energy Science, which typically requires mechanistic insight, sensitivity analysis, or demonstrated consequences for aeroelastic performance or design.

AC Response: We acknowledge this issue. In the current revised manuscript, it is updated.

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S.No. 7: Referee 2 comment: The manuscript contains serious citation errors. Four of the seven references are incorrectly cited. These issues substantially undermine the credibility of the work and its engagement with the existing literature.

AC Response: Updated.

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S.No. 8: Referee 2 comment: In my humble opinion, to make the manuscript suitable for future publication, the author should, at minimum achieve one of the points listed below:

- Conduct comparative or sensitivity studies similar to those in Refs. [1], [4], or Shakya et al. (2019, see below).
- Quantify the impact of coupling “hot spots” versus smooth coupling distributions on aeroelastic stability, flutter, or load response.

- Analyze a larger dataset of real turbine blades (at least ten) to support claims of scaling trends rather than relying on three reference designs that have never been realized.
- Clearly distinguish design-driven effects from scaling-driven effects.

AC Response: We thank the referee for these constructive recommendations and agree that comparative aeroelastic simulations/sensitivity studies and a larger blade dataset would be the appropriate next step to quantitatively link coupling distributions to stability margins and load response. Accordingly, in the revised manuscript we refocused the motivation and scope toward a matrix-level comparative analysis of the three public reference inputs, and we explicitly identify aeroelastic sensitivity/simulation studies and larger-dataset validation as important future work.

**Change in revised manuscript:** 2.4 , future work

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We thank the referee again for highlighting key issues. The revisions substantially improve the manuscript's positioning, rigor, and compliance with journal requirements.

Regards

Abhishek Sharma

AC