Review of

“Load application in wind turbine blades modelled as reduced-order multibody structures in the floating frame of reference formulation”

The manuscript presents a formulation for beam-like structures, as those used in wind turbine blades, based on substructuring from 3D finite element models and the assembly of macro-structural components using the Floating Frame of Reference Formulation (FFRF) to account for moderate displacements and geometric nonlinearities.

The manuscript focuses in particular on the introduction of distributed loads (e.g., of aerodynamic nature) based on low-fidelity formulations, such as Blade Element (BE) methods (often associated with Momentum Theory, although this aspect is not directly relevant here). These approaches provide beam-oriented, spanwise load distributions in the form of forces and moments per unit span (typically lift, drag, and aerodynamic moment, i.e., two force components and one moment, defined at the aerofoil section). As a consequence, such loads must be re-distributed onto a more detailed structural mesh, which includes chordwise (and thickness-wise) geometric resolution.

Overall, the manuscript is well written, lies within the scope of the journal, and appropriately references previous work. A significant amount of results is presented and generally well discussed, supporting the characteristics and features of the proposed formulation and its claimed improvements with respect to the state of the art.

My main criticism concerns the consistency of modeling fidelity across different aspects of the analysis. Increasing the level of fidelity of one part of the model should be accompanied by a corresponding increase in fidelity in other, closely related aspects. In particular, increasing the structural detail should ideally be paired with a commensurate increase in load modeling fidelity. Otherwise, a beam model with accurate section characterization and strain/stress recovery may yield comparable-quality results at significantly lower computational and modeling cost. This issue is partly discussed in the manuscript and is central to the topic of the work; however, it might benefit from being more clearly framed in the introduction, as it already underpins much of the subsequent discussion of the results.

Detailed comments follow:

- Line 5: “.. constituting a higher-fidelity alternative to beam elements, ...”
  The objective is certainly admirable; however, it would be useful to explain and justify more explicitly why and in which sense this higher fidelity is beneficial, possibly providing quantitative or qualitative measures of improvement.
- Lines 7-8: “… of calculating equivalent concentrated loads and applying them to the model using interpolation multipoint constraints (RBE3).”
  Again, increasing the fidelity of a part of the analysis may be of limited value if related aspects are not enhanced accordingly. In this case, adding section-wise structural detail may be of limited benefit if the distributed loads do not provide comparable spatial resolution. This would not be the case, for example, if higher-fidelity aerodynamic models such as full 3D CFD, or even mid-fidelity methods (lifting-surface, vortex-lattice, or panel methods), were employed.
- Line 12: “… minimum strain energy interfaces increase the stiffness of the model.”
  This statement appears to contradict, at least in part, the subsequent discussion. Please clarify this point and reconcile it with the later arguments.

- Lines 25, 32-33, and 40: the authors first state that HAWC2 departs from GEBF in that it uses the FFRF (line 25); then they state that HAWC2 uses (Timoshenko) beam elements (line 32); subsequently, by contrast, thay state that the proposed model (following Antunes et al.) is based on "solid finite elements" (line 33), but conclude that "[s]uch a model is able to compute accurate nonlinear static responses when compared to standard solid finite element models" (line 40).
  This sentence leaves some ambiguity as to whether and how the proposed approach differs from "standard solid finite element models." I believe this entire section would benefit from a revision aimed at more clearly describing the characteristics of each method and how they compare to and improve upon one another.
- Lines 73-78: some clarifications along the lines of the previous point are provided here. It may be beneficial to anticipate and integrate this discussion with that presented earlier, in order to improve coherence and avoid repetition.
- Lines 144-147: The need for compatible connecting interfaces is clear; however, how this compatibility is achieved using a singular value decomposition (SVD) is not. Please clarify this point, at least conceptually, without necessarily introducing all implementation details.
- Lines 155-157: "… partitioning a structure into multiple sub-bodies (multibody approach) is required to be able to model nonlinear deflections with the FFRF, as it computes a linear response for each sub-body."
  This is not only a matter of large deflections, but also of preload effects and geometric stiffness contributions. In wind turbine blades, the stiffening effect due to tension induced by centrifugal loads can be quite significant (and even more so in helicopter rotor blades).
- Lines 257-267: I suggest decoupling the description of the Exudyn implementation from the theoretical formulation. The formulation should be presented first, possibly followed by a brief discussion of implementation aspects and specific details.
- Line 351: "The cross-sectional properties of the beam in HAWC2 are computed using BECAS v4.0…" If the approach described in

  > Masarati et al., "Substructuring-Based Accurate Beam Section Characterization from Finite Element Analysis," Computers & Structures, https://doi.org/10.1016/j.compstruc.2025.107720

  (based on Masarati, 2020), is adopted, the same mesh and material properties used for the 3D model could also be used to characterize the beam section. This would avoid the need for an external tool or a separate finite element analysis.