General comments

Dear Authors,

I have carefully read the manuscript entitled *"Fixed Frame Control-Oriented Modeling and Characterization of Asymmetric Wind Disturbances for Advanced Individual Pitch Control Design"*. The paper addresses the control-oriented representation and characterization of asymmetric wind disturbances for wind turbine applications. Starting from a linearized aeroelastic model with spatially distributed wind inputs, the authors propose a sensitivity-based reduction of the inflow field into three equivalent disturbances in the fixed reference frame. The methodology is evaluated using the IEA 15 MW reference wind turbine and nonlinear OpenFAST simulations, both in the time and frequency domains.

Overall, I find the topic relevant to the wind turbine control community and the manuscript presents an interesting framework with potential value for disturbance-aware individual pitch control design. In particular, the reduction of a high-dimensional distributed wind input into a small set of control-oriented equivalent disturbances, followed by validation against nonlinear aeroelastic simulations, is an appealing idea. The manuscript is generally well structured and the numerical results show good agreement between the reduced linear-model responses and the OpenFAST simulations.

Nevertheless, I believe that several aspects of the proposed methodology require further clarification and justification to better establish the scope and validity of the proposed framework. In particular, further clarification is needed regarding the conditions under which the static sensitivity-based disturbance reduction can reproduce the dynamic response of the full distributed-input model, the assumption of a diagonal spectral density matrix for the equivalent disturbances, and the positioning of the claimed novelty with respect to previous effective-wind and disturbance-oriented IPC formulations. The dependence of the proposed disturbance representation on the selected operating point and inflow conditions should be discussed more explicitly.

Specific comments are provided below.

1. Equivalent disturbance formulation (Section 3.2, Eq. 13).

The equivalent disturbance in Eq. (13) is constructed using the static gains, while the reduced model in Eq. (12) retains the full dynamic transfer functions. The manuscript then states that this construction "reproduces the same system response as the full set of nodal wind disturbances when propagated through the linear model". This statement seems too strong in general.

The static weighting in Eq. (13) preserves the DC input-output mapping by construction, but exact dynamic equivalence would require additional conditions. The good agreement shown in Fig. 7 and Table 1 indicates that the proposed approximation works very well for the investigated case. However, I suggest that the authors clarify the approximate nature and validity conditions of Eq. (13). In particular, the sentence stating that the equivalent disturbance is "built so that it reproduces the same system response as the full set of nodal wind disturbances when propagated through the linear model" suggests exact equivalence. Unless the authors can demonstrate that the normalized nodal dynamics are practically identical over the relevant frequency range, I think this wording should be softened, for example by stating that the equivalent disturbance is constructed to approximate the dominant system response, or that it is based on static sensitivities to preserve the low-frequency input-output contribution.

2. Cross-spectral terms in the equivalent disturbance representation (Section 4.4, Eq. 19).

In Eq. (19), the disturbance spectral matrix is assumed to be diagonal, which implies that the equivalent collective, tilt and yaw disturbances are mutually uncorrelated in the spectral sense. I think this assumption requires further justification. Since the three equivalent disturbances are obtained from the same spatial turbulent wind field and transformed into the fixed frame, non-zero cross-spectral terms, particularly between the two asymmetric components, cannot be excluded a priori.

Previous stochastic multiblade formulations have explicitly shown that cross-spectral coupling can remain after transformation to the fixed frame. For example, Thomsen et al. derived stochastic wind models in multiblade coordinates including cross-spectral relationships between the transformed disturbance components (Thomsen et al., 2010), while Shan showed that the cross-spectral terms between the two asymmetric multiblade components may contain relevant information and should not generally be neglected without verification (Shan, 2017). In the latter work, the cross terms between the collective and asymmetric components were found to be small, but this was an observed property rather than a general consequence of the transformation.

I therefore suggest that the authors quantify the cross-spectral densities or coherences between the equivalent collective, tilt, and yaw wind disturbances using the turbulent simulations already available. If these terms are negligible over the relevant frequency range, the diagonal approximation in Eq. (19) would be properly justified. I would also be interested to see whether non-negligible cross-spectral terms appear, particularly between the two asymmetric components, and whether accounting for them could help explain part of the remaining discrepancies observed in the closed-loop spectral predictions.

3. Positioning of the novelty with respect to previous disturbance-oriented IPC formulations.

The novelty statement in the Introduction could, in my view, be positioned more precisely with respect to the existing disturbance-oriented IPC literature. The manuscript states that asymmetric disturbance characterization and control-oriented modeling have mainly been addressed separately, and that the disturbances responsible for exciting the loads are typically not explicitly incorporated into the IPC design process. However, previous studies have already combined several of these elements.

For example, stochastic wind disturbance models formulated in multiblade coordinates have been explicitly incorporated into model-based IPC design (Thomsen et al., 2010). Tang et al. (2022), already cited in the manuscript, also introduce blade-specific wind disturbances and transform them into fixed-frame average, d-axis, and q-axis wind components for disturbance-aware IPC design. In addition, Shan (2017) demonstrates controller evaluation and tuning based on disturbance spectra and linear models. These works do not implement the same methodology proposed here, but they suggest that the general link between asymmetric disturbances, fixed-frame representations, and control-oriented design has been explored previously.

In my view, the specific contribution of the present work is more clearly associated with the reduction of a high-dimensional distributed wind input into three equivalent fixed-frame disturbances using whole-system input-output sensitivities, together with the validation of this reduced representation against nonlinear OpenFAST simulations. I therefore suggest that the authors reconsider the wording of the research gap and clarify more explicitly how this specific contribution differs from the previous disturbance-oriented IPC literature.

4. Dependence on the operating point and inflow conditions.

The proposed equivalent disturbance representation is derived and validated at a single above-rated operating point, corresponding to a mean wind speed of 19 m/s. Since both the linearized input-output sensitivities used to construct the equivalent disturbances and the disturbance spectra may depend on the operating condition, the manuscript should discuss more explicitly how the proposed framework is expected to behave away from this operating point.

In particular, it would be useful to clarify whether the equivalent-disturbance weights are expected to remain approximately valid over a range of above-rated wind speeds, or whether they should be recomputed as the turbine operating condition changes. A similar question applies to changes in turbulence intensity or vertical shear, since these conditions may modify the spatial and spectral characteristics of the inflow.

A full multi-operating-point validation does not seem necessary for the present manuscript, but a discussion of this dependence and of a possible extension to gain-scheduled or operating-point-dependent disturbance models would help define the practical applicability and limitations of the framework.

5. Practical estimation of the equivalent disturbances for real-time control applications.

The proposed framework provides a compact and potentially useful representation of the distributed wind field through the equivalent collective, tilt, and yaw disturbances. In the present study, however, these signals are obtained from the undisturbed nodal wind velocities available from the OpenFAST simulations.

Could the authors comment on how these equivalent disturbances might be obtained in a practical wind turbine implementation, particularly for future real-time disturbance-aware control applications, including possible feedforward or preview-based architectures? A brief discussion of the main practical difficulties would help clarify the pathway from the proposed disturbance representation toward such applications.

6. Clarification of reconstruction, prediction, and computational savings.

The manuscript highlights that the proposed framework can reduce the need for computationally expensive aeroelastic time-domain simulations during controller design. I agree that the spectral propagation through the reduced linear model has clear potential in this regard. However, I think the distinction between reconstruction and prediction could be clarified more explicitly.

In the time-domain validation of Fig. 7, the equivalent disturbances are constructed from the nodal wind velocities extracted from the OpenFAST turbulent simulations and are then propagated through the reduced linear model. Therefore, this test primarily demonstrates the ability of the reduced disturbance representation and the linear model to reconstruct the corresponding nonlinear turbine response for a known inflow realization.

I would suggest clarifying which quantities need to be obtained from high-fidelity simulations before the proposed framework can subsequently be reused for controller evaluation. In particular, are the equivalent disturbance spectra intended to be characterized once for a given operating and inflow condition and then reused to assess multiple controller designs without additional aeroelastic simulations? If so, stating this workflow more explicitly would help quantify where the computational savings are obtained and avoid possible ambiguity between reconstruction of a known turbulent realization and statistical prediction using previously characterized disturbance spectra.

References

Thomsen, S. C., Niemann, H., and Poulsen, N. K.: *Stochastic wind turbine control in multiblade coordinates*, American Control Conference, 2010.

Shan, M.: *Load Reducing Control for Wind Turbines: Load Estimation and Higher Level Controller Tuning based on Disturbance Spectra and Linear Models*, PhD thesis, 2017.

Tang, S., Tian, D., Wu, X., Huang, M., and Deng, Y.: *Wind turbine load reduction based on 2DoF robust individual pitch control*, Renewable Energy, 183, 28–40, 2022.

Technical corrections and minor editorial comments

The manuscript is generally well written, but I noticed several minor typographical and grammatical errors that should be corrected:

- Line 86: “correspondng” → “corresponding”.
- Line 121: “an specific pitch variation” → “a specific pitch variation”.
- Line 129: “respresented” → “represented”.
- Line 271: “rotor frecuency” → “rotor frequency”.
- Line 354: “demostrating” → “demonstrating”.
- Line 361: “rotor spped variability” → “rotor speed variability”.
- Acknowledgements: “grategully appreciate” → “gratefully appreciate”.

I also recommend a final proofreading of the manuscript to identify similar minor typographical and grammatical issues.