Dear Patel et al.,

Both reviewers recommend publication of the manuscript, subject to minor technical revisions. I also read the manuscript with interest, as the topic relates closely to my own research interests in wind loading on structures. In addition to the reviewers' comments, I have included several editorial and technical remarks intended to improve clarity and precision. Please revise the manuscript by addressing the reviewers' comments as well as the points listed below. In particular, please ensure that elements of the section "Methods" are not presented in section 3 "Results". I believe these adjustments will further strengthen an already interesting contribution. I look forward to reviewing the revised version

Introduction: Reference to Kaimal et al. (1972) may not be appropriate here. IEC recommends a spectral model that modifies the Kaimal spectrum and complements it with an exponential decay coherence function. The model presented in Kaimal et al. (1972) differs fundamentally from the IEC Kaimal model. In particular, Kaimal et al. (1972) do not discuss turbulence coherence. In addition, their formulation uses surface-layer scaling, whereas the IEC Kaimal formulation yields a spectrum that becomes independent of height beyond a certain altitude. It would therefore be more accurate to refer to the model as the "IEC Kaimal spectrum with exponential decay coherence" rather than simply "Kaimal et al. (1972)".

General scientific context

An interesting parallel can be drawn with the buffeting theory formulated by Alan Davenport and Robert H. Scanlan in the 1960s–1970s for towers and bridges. In its linear formulation, buffeting response is proportional to turbulence intensity. Consequently, a larger dynamic response is expected in unstable atmospheric conditions (higher turbulence intensity and stronger coherence) than in stable conditions (lower turbulence intensity and weaker coherence). Briefly mentioning this conceptual similarity may help place the present results within a broader wind-engineering framework.

Line 46: Regarding the reference to the model developed by Chougule et al., it would be appropriate to mention that the model becomes numerically unstable when applied to strongly and moderately convective conditions. This clarification could also explain why the model is not applied in the present study.

Writing and terminology

- The term "etcetera" (line 87) is generally avoided in academic writing. Consider explicitly listing the relevant variables or processes.

- When expressing wind speed in **m/s**, a space should be included between *m* and *s*. Without the space it reads as ms^{-1} (milliseconds^{-1}), which denotes a frequency.
- The phrase
  "w is the fluctuation in the vertical-wind component"
  could be revised to
  "w denotes the fluctuating vertical wind-velocity component."
- Power spectral densities are not strictly *measured* but *estimated*. It would be preferable to replace "measured PSD" with "estimated PSD" throughout the manuscript.

Data selection and spectral estimation

The phrase *"selecting the longest continuous period in each bin"* is somewhat unclear. Does this mean that at least **40 min of continuous data** are required for a sample to be considered valid? If so, it would help to state explicitly that the spectra are computed from 40-minute records divided into segments (e.g., ten bins or segments), similar to the procedure described in the PSD estimation method of Peter D. Welch (1967).

Related to this point: was the **Welch method** used for the PSD estimation? Welch's method allows overlapping segments and significantly improves spectral estimates compared with the classical periodogram approach. If four segments without overlap were used, this corresponds to a special case of Welch's method with 0 % overlap, still preferable to a raw periodogram, though overlap often improves variance reduction considerably. Clarifying the exact implementation would be helpful.

Boundary-layer and stability considerations: In the definition of the Obukhov length, the **virtual potential temperature (θ_v)** should formally be used rather than absolute temperature. If sonic temperature has been used instead, this is usually an acceptable approximation to θ_v, but it would be useful to mention this explicitly.

Table 3 is informative, but these values are typically interpreted within the atmospheric surface layer (ASL), where the Obukhov length is assumed constant with height. At 155 m, this assumption may no longer strictly hold. A short cautionary remark could help prevent over-interpretation of these parameters above the ASL. The friction velocity $u*$ generally decreases with height and approaches zero near the top of the atmospheric boundary layer. Since the Obukhov length L is proportional to $u*^3$, a similar tendency might be expected for L. There is also likely a height dependence in the turbulent heat flux, though I do not remember which one it is.

Interpretation of turbulence regimes

The sentence "At mean wind speeds above 14 m/s mechanically generated turbulence is stronger than the effects of buoyancy." is somewhat too categorical. This may often be the case near **10 m above ground**, but the balance between mechanical production and buoyancy can be different at **150 m height**. In engineering practice, a rule of thumb such as *10 m/s at 10 m height* is sometimes used, but this is only a rough approximation.

In Figure 4, for example, roughly 50 % of the data at wind speeds around 15 m/s appear to be non-neutral, which is significant. It may therefore be better to reformulate the statement more cautiously.

Spectral parameters

The parameter **Γ** likely depends on **surface roughness, atmospheric stability, and boundary-layer depth**, as these factors influence the ratio of the spectra (S_w/S_u), particularly in the low-frequency range. Stating that Γ is "around 3" may therefore be somewhat misleading given the expected variability. It might be preferable to emphasize that the variability of Γ deserves further investigation rather than presenting a single representative value.

Coordinate definitions

Line 235:

"Note that the fore–aft direction is parallel to the mean wind direction and side–side is perpendicular to it."

It may be useful to add that this statement assumes zero yaw angle (no yaw misalignment).

Fig. 7: It may improve the clarity of the figure to indicate the height at which the measurements were taken, since turbulence intensity generally decreases with altitude.

Mixing of Methods and Results sections: This is an important feedback from my side. Equations 16–19 and the associated explanatory text should be moved from the "Results" section to the "Methods" section. These equations describe the methodological framework used in the analysis rather than the outcomes of the study.