Response to reviewer#2's comments

Referee #2's comment:

This manuscript evaluates multi-rotor UAV measurements against a meteorological tower and examines how boundary-layer stratification and wind shear influence flight disturbances over the eastern Taihang piedmont. The study addresses an important observational need and provides valuable findings for atmospheric boundary-layer research and low-altitude meteorological applications. The combination of measurement validation and aircraft-response analysis is a strength, offering useful insights into environmental conditions associated with flight perturbations. The manuscript is generally well organized and relevant to the atmospheric observation community. Some clarification of measurement uncertainties, the Richardson-number calculation, and the interpretation of the turbulence index would further strengthen the study. Several mechanistic interpretations would also benefit from additional discussion or more cautious wording. Minor inconsistencies in dataset descriptions, figure annotations, and language could be corrected to improve clarity. Overall, I consider the manuscript suitable for publication after minor revisions.

Response: We sincerely thank the reviewer for the careful evaluation of our manuscript and for the positive assessment of its relevance to atmospheric boundary-layer research and low-altitude meteorological applications. We are glad that the reviewer found the combination of measurement validation and aircraft-response analysis to be a strength of the study. Following these suggestions, we have revised the relevant descriptions and corrected the inconsistencies in the dataset descriptions, figure annotations and language. For clarity purpose, here we have listed the reviewer's comments in bold font, followed by our response in blue plain font, and the modifications to the manuscript are in blue italics.

Specific comments:

1.The validation below 100 m is useful, although its applicability to the full 1500-m sounding range merits discussion. Please clarify sensor uncertainties, response delays,

and residual rotor-flow effects, and assess their influence on gradients and Ri. Height-dependent validation statistics would help establish confidence in measurements at higher altitudes and under cloudy conditions.

Response: We appreciate the reviewer's distinction between near-surface validation and the higher-altitude application. The benchmark comparison is limited to the tower levels (8–96 m AGL), whereas the attitude-response soundings extended to approximately 1500 m. We have revised Sections 2.1 and 3.1 to state explicitly that the reported biases, RMSEs and correlations establish performance only over the sampled tower range; they are not a validation at 1500 m or under cloud exposure. The sensors were laboratory-calibrated before deployment, and the meteorological package was located 0.6 m above the fuselage to reduce direct rotor-flow exposure. These precautions do not quantify residual downwash, ventilation dependence or the response delays of temperature and humidity sensors. We now explain that these effects may shift or smooth vertical gradients, and that the uncertainty can be amplified when those gradients are combined to form Ri. Since tower measurements end at 96 m, no independent height-dependent verification is available higher up.

2. The all-pairs Ri calculation combines sequential observations and produces dependent samples. The authors are encouraged to examine its sensitivity to temporal variability, vertical spacing, and small shear denominators through comparison with layer-mean estimates. Please also discuss effective sample size and the mathematical dependence between Ri and shear in Figure 12.

Response: Thank you for identifying an important interpretative limitation of the all-pairs approach. The computation remains exactly as described in Eqs. (1)–(4): observations at adjacent hovering levels are paired to retain a distribution of *Ri* values. In the revised Section 2.3, we now distinguish the number of pairwise combinations from the number of independent observations. Samples are reused across pairs and may be temporally autocorrelated; consequently, $N_i \times N_{i+1}$ must not be treated as the effective independent sample size. We also explain that sequential hovering can confound height and time, finite layer spacing leaves unresolved vertical structure, and a small squared vector-shear

denominator can amplify measurement and differencing errors. We attempted to calculate *Ri* using layer-mean values, but this approach substantially reduced the sample size. Given the small number of resulting estimates, we could not confidently assess the reliability of the results. Therefore, we did not conduct a comparison with layer-mean Ri in this revision; instead, we explicitly discuss the aforementioned limitations in Section 2.3. Finally, we make explicit that Ri and vector wind shear share the same gradients in Eq. (4), so their joint distribution in Figure 12 has a partly mathematical dependence and cannot, by itself, establish a physical coupling or causal threshold.

3. The turbulence index represents aircraft response and may also reflect flight-control behavior. Please clarify the consistency between Equation (5) and the magnitudes in Figures 7, 9, and 11, and describe filtering and averaging. Comparison with independent turbulence measurements, where available, would strengthen its interpretation.

Response**:** We thank the reviewer for this important comment and agree that the turbulence index represents the aircraft's attitude response rather than a direct measurement of atmospheric turbulence. In the revised Section 2.3, T is defined as the instantaneous Euclidean magnitude of the recorded pitch and roll angular velocities and is described as a platform-specific attitude-response proxy, to which the aircraft dynamics, stabilization commands and flight phase can also contribute. It therefore has the same units as these angular velocities, namely °/s. The pitch and roll angles were sampled at 1 Hz, and the corresponding angular velocities were calculated using a central difference based on the angle measurements taken 1 s before and 1 s after each time point. calibrating T against such measurements would be required for a physical interpretation, and we will address this in future experiments.

4.Ascent–descent differences may also reflect temporal evolution, horizontal displacement, and control effects. A discussion of these influences would strengthen the interpretation of atmospheric heterogeneity. Statements concerning cloud-base entrainment, moisture effects, resonance, and operational thresholds could be supported further or expressed more cautiously as hypotheses or case-specific findings.

Response**:** We thank the reviewer for these helpful suggestions. The three case discussions place the ascent and descent tracks in the changing signed-shear background of Figure 5c. We now also state explicitly in Section 3.2 that the ascent and descent passes were separated by several minutes and need not have sampled identical air. Temporal evolution, possible horizontal displacement and differences in autopilot correction may all contribute to the asymmetric response, and the available observations do not separate their effects; the statements linking background shear to the asymmetry have been revised accordingly. The lower turbulence index during UAV ascent compared with descent is likely attributable to the control actions of the flight-control software. The cloud-base entrainment explanation has been changed from a demonstrated process to a plausible interpretation, and the humidity-related statements in Section 3.2 and the Concluding remarks no longer exclude moisture or cloud effects. For Figure 12, the localized bin of elevated index is presented as an observation specific to this dataset rather than as evidence of eddy–airframe resonance, for which spectral or airframe-frequency evidence is not available. References to quantitative operational thresholds in the research objectives and the Concluding remarks have been replaced with case-specific conditions associated with increased aircraft response.

5.Section 2.2 reports 39 hovering datasets and 46 vertical profiles, whereas Figure 3 identifies 46 comparison profiles. Please reconcile these numbers and specify the datasets used in each analysis.

Response: We thank the reviewer for pointing out this inconsistency. The campaign comprises 46 vertical profiles in total: 39 obtained from the tower–UAV comparison experiments at the Xingtai base (July 26–30) and 7 obtained from the attitude disturbance experiments in Qianxian Township (July 31). Each dataset corresponds to one vertical profile, i.e. one ascent or descent together with the hovering measurements at each level; the original text described the same datasets as both “hovering datasets” and “vertical profile datasets”, which caused the confusion. The label “Profile number = 46” in the originally submitted Figure 3 was an error, because Figure 3 is based only on the 39 comparison profiles; it has been corrected to 39, and the statistics in the figure are unchanged. Section 2.2 now uses one term throughout, states the composition of the 46 profiles, and specifies

that the tower–UAV comparisons in Figures 3 and 4 use the 39 Xingtai profiles and that the three evening cases analysed in Section 3.2 were selected from the 7 Qianxian profiles.

6.Figure 5c describes signed shear as a "magnitude." Please distinguish the signed wind-speed gradient from vector shear magnitude and ensure consistency among the equations, text, figure labels, and captions.

Response: We agree. Figure 5c contains positive and negative values and therefore cannot be described as a shear magnitude. Its shaded field is now explicitly called the signed vertical gradient of horizontal wind speed (signed shear): positive where wind speed increases with height and negative where it decreases. This differs from the non-negative magnitude of the vertical gradient of the horizontal wind vector, whose square forms the denominator of Eq. (4) and which is used in the shear–Ri analysis of Figure 12. We have revised the Figure 5 and Figure 12 captions, the description of Figure 5c in Section 3.2 and the discussion of Figure 12 in Section 3.3 accordingly. In Section 2.3, the text introducing Eqs. (1)–(3) now refers to the vertical gradients of the two horizontal wind components rather than to wind shear. The Figure 5 caption also states that the colour bar labelled "Wind shear" shows this signed shear. The unit printed on that colour bar ($m \cdot s^{-1}$) is a typographical error; the correct unit is s^{-1}, as given in the caption.

7."top and right marginal distributions ……" in Lines 450 to 451 is inconsistent with Figure 12.

Response: Thank you for catching this description error. In Figure 12, the marginal strip showing the mean TI for each Ri bin is at the bottom, and the strip showing the mean TI for each wind-shear bin is on the left. The sentence in Section 3.3 has been corrected to refer to the "bottom and left" marginal distributions, and it now states explicitly which quantity each strip shows. The later reference to the "top" marginal distribution has been removed, together with the specific numerical TI values (see our response to Comment 3).

8.Correct "yileds" (line 244), "July 31th" (line 222), and duplicated punctuation (lines 364 and 368).

Response: Thank you. All the typographical errors and duplicated punctuation have been corrected as suggested.