Response to reviewer#1's comments

Referee #1's comment:

Scientific comments：

This study employs a DJI M350 RTK multi-rotor UAV to investigate the reliability of atmospheric boundary layer (ABL) observations and flight safety over complex terrain. The authors conducted comparative observations against a 100-m gradient meteorological tower and vertical-profile surveys over the eastern Taihang piedmont. They first demonstrated that the UAV platform can accurately measure both basic meteorological variables (temperature, wind, humidity) and derived stability parameters (potential temperature and gradient Richardson number). Furthermore, the study quantitatively reveals the combined effects of vertical wind shear and stratification stability on UAV attitude disturbances, and proposes a possible vortex-airframe resonance-like mechanism, which is a novel contribution to UAV-based meteorological applications. Notably, the paper employs a sample-pairwise finite-difference method for Ri calculation, which preserves the distribution information of parameters between stratified layers and better captures turbulence details than conventional layer-averaging approaches.

The topic is well within the scope of AMT. The experimental design is rigorous, the dataset is substantial, and the analytical framework is logically structured. The findings provide valuable references for improving UAV-based ABL observations and low-altitude flight safety over complex terrain. The manuscript is well written and the figures are of high quality. Before formal acceptance, I recommend that the authors address the following specific issues.

Response: We sincerely thank the reviewer for the careful reading of our manuscript and for the positive and encouraging evaluation. We are grateful that the reviewer recognized the value of combining UAV–tower comparisons with vertical-profile observations, the quantitative analysis of how vertical wind shear and stratification stability

jointly affect UAV attitude disturbances, the possible resonance-like mechanism, and the sample-pairwise finite-difference method used to calculate Ri. We have carefully considered each of the specific comments, and our point-by-point responses are provided below. 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.

1. The logical chain from observational results to conclusions needs to be strengthened

The authors describe the time-height evolution of the large-scale wind shear field based on Figure 5c, and further analyze the shear characteristics during three flight periods (18:24-18:43, 18:55-19:22, and 19:43-19:55 BJT) in Section 3.2 (starting from page 12, line 328; page 13, line 365; and page 14, line 400, respectively). However, in the detailed discussion of these three cases, the authors do not explicitly link the temporal evolution of the large-scale shear field revealed in Figure 5c with the vertical distribution characteristics of UAV attitude responses during each period.

For example, during the first flight (18:24-18:43), which positive/negative large-scale shear zones did the ascending and descending segments (black solid and dashed lines in Figure 5c) traverse? Could the difference in this large-scale background shear be a significant cause of the asymmetry in flight disturbances between the ascending and descending profiles (Figure 7)? I suggest that the authors actively reference Figure 5c when discussing each case, and superimpose the flight trajectories onto the large-scale shear background for joint analysis. This would significantly strengthen the logical chain connecting the macro-scale background to the local disturbance responses.

Response: We thank the reviewer for this insightful comment. We have revised Section 3.2 to explicitly connect the large-scale wind shear evolution in Figure 5c with UAV attitude responses for all three flight periods. For each case, we describe the large-scale shear zones traversed by both ascending and descending trajectories and discuss how background shear differences shape the asymmetry of flight disturbances. The UAV flight paths are superimposed on the wind shear field in updated Figure 5c, and Figure 5c is cited throughout the case analysis to jointly interpret macro shear environments and corresponding UAV

perturbations, reinforcing the physical linkage between large-scale background conditions and local attitude responses.

2. The evidence supporting the "resonance-like amplification" hypothesis needs to be reinforced

On pages 16-17, lines 465-470, the authors propose a "resonance-like amplification" conjecture based on Figure 12. This is an interesting speculation, but it is currently supported only by anomalous data points in a specific regime (vertical wind shear approximately 0.007-0.010 s^{-1} combined with near-zero to negative Ri). The evidence appears somewhat thin. I recommend that the authors: (1) adopt a more cautious tone when presenting this viewpoint, clearly stating that this is a preliminary hypothesis requiring further verification; and (2) if possible, provide additional supportive analysis—for instance, by roughly estimating the characteristic scale of turbulent eddies under this specific shear range and discussing whether this scale is comparable in magnitude to the airframe dimensions or the natural frequencies of the MATRICE 350 RTK in pitch/roll directions. If such analysis cannot be provided, I suggest relocating this discussion to the end of the Discussion section as an outlook for future research directions.

Response: We are grateful for the insightful comments made by the reviewers. We acknowledge that the proposed resonant amplification hypothesis is based solely on limited observational results with a vertical wind shear of approximately 0.007–0.010 s^{-1} and Ri close to zero or negative, and the related evidence is relatively weak. We have explained this in the text in Section 3.2, using a more cautious tone and clearly stating that this hypothesis is only a preliminary speculation and requires further verification. We added cautious restrictive explanations to clarify that the resonant amplification concept is only a preliminary assumption and that the current data set has limitations that prevent the estimation of the vortex scale.

3. Inconsistency in the description of cloud-layer vertical extent

On page 15, line 405, the authors state "in the cloud layer (approximately 900-1200 m) and outside the cloud layer." However, on page 16, line 437, they mention "In the stable cloud-top layer above 1000 m." There is ambiguity and inconsistency between the description of the "cloud layer" (900-1200 m) and the "cloud-top layer" (above 1000 m). I suggest that the authors clarify and unify the vertical distribution and extent of the cloud layer, as this directly affects the accurate interpretation of the conclusion that "stable stratification suppresses disturbances."

Response: We thank the reviewer for pointing out the inconsistency in our description of the cloud's vertical extent. We have revised the relevant text. In the 19:43–19:55 flight case, clouds were detected between 800 and 1400 m. The cloud top was likely above 1400 m, which was the maximum altitude reached by the UAV. The term "cloud-top" was therefore incorrect, as the observed altitude range corresponded to an in-cloud region rather than the cloud-top layer. Accordingly, we have revised the relevant wording to "*In the stable cloud layer above 1000 m*".

Minor Comments:

1.The terms "flight bumpiness" (e.g., page 2, line 49; page 14, line 391) and "flight disturbance" (e.g., page 2, line 33; page 16, line 461) are used interchangeably throughout the manuscript. I recommend standardizing to "flight disturbance" or the more precise term "attitude disturbance" to maintain terminological consistency.

Response: We appreciate the reviewer's suggestion on terminology standardization. We have systematically replaced all occurrences of "*flight bumpiness*" with "*attitude disturbance*" throughout the manuscript to unify terminology, as "*attitude disturbance*" more precisely describes the UAV attitude perturbations analyzed in this work.

2. Figure formatting details：

1)In Figures 4, 6, 8, and 10, the variable names on the x-axes (e.g., theta, Ri ,etc) should be by mathematical variable formatting and italicized to comply with conventions, like: θ and Ri.

Response: We thank the reviewer for this careful check of figure formatting. We have revised the x-axis labels in Figures 4, 6, 8, and 10, applying proper mathematical italic formatting for variables such as θ and Ri to follow standard typesetting conventions.

2)In the captions of Figures 7, 9, and 11, the repeated use of "profile" could be simplified; for example, change to "Same as Figure 6, but for the vertical response of UAV attitude parameters during ...".

Response: We appreciate the reviewer's helpful suggestion for simplifying figure captions. We have streamlined the captions of Figures 7, 9, and 11 following the recommended format, adopting concise phrasing starting with "*Same as Figure 6, but for the vertical response of UAV attitude parameters during …*" to avoid redundant repetition of the term "*profile*".