

Response to Reviewer 2's Comments

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Overall, I find the manuscript and the resulting dataset highly innovative and valuable for both turbulence research and aviation applications.

We sincerely appreciate your thoughtful review and valuable comments on this manuscript. We seriously considered your comments and posted responses to your comments and suggestions. Below, we indicate the original comment of the respective reviewer in blue and our answer in black. In addition, we provide a tracked-changes version of the manuscript.

1. Please correct the apparent typo “Using 10 years (2015–2015)” to “2015–2024” in the Conclusions.

We thank the reviewer for pointing out this typo. The incorrect year range “2015–2015” in the Summary and conclusions has been corrected to “2015–2024” in the revised manuscript (Line 495).

2. You apply a 60 m-scale moving average (13-point for 1-s; 7-point for 2-s) to multiple variables. Please specify (i) whether the filter is centered, (ii) how end points are handled, and (iii) the effective vertical span after interpolation (since 13×5 m and 7×10 m are slightly >60 m).

We thank the reviewer for requesting clarification regarding the moving-average procedure.

(i) The moving average is applied as a centered scheme.

(ii) At the end points (i.e., uppermost and lowermost levels) where a full centered window cannot be constructed, boundary values are treated by assigning the nearest valid averaged value to those end points. This approach avoids artificial truncation of the profile while maintaining consistency near the boundaries.

(iii) Because the smoothing is applied after interpolation to uniform vertical spacing, the effective vertical span corresponds to 60 m in both 1-s and 2-s cases. The effective span is determined by the distance “between” the first and last points within the window: 12×5 m = 60 m for the 1-s data and 6×10 m = 60 m for the 2-s data. Thus, the applied filter consistently represents a 60-m vertical scale.

These are incorporated into the revised manuscript (Lines 218-222).

3. Because all reported layers must be ≥ 60 m, the method will systematically exclude thin shear layers. Please add a short sensitivity test (e.g., 40/60/80 m) or at least report the fraction of candidate layers removed by this constraint.

We thank the reviewer for this thoughtful suggestion. The minimum layer thickness of 60 m was not chosen arbitrarily. Rather, it is physically motivated by the observational characteristics of operational radiosonde systems, as stated in the original manuscript (Lines 188-195). Specifically, the balloon–payload length (~ 30 m) and the associated pendulum motion imply a characteristic oscillation period of approximately 11 s, corresponding to a vertical scale of about 60 m. Therefore, fluctuations at scales smaller than 60 m are strongly influenced by instrumental motion and cannot be reliably

distinguished from observational artifacts. For this reason, the 60 m threshold reflects an observational detectability limit rather than a tunable methodological parameter.

Because this threshold is tied to instrument physics and effective vertical resolution, conducting a sensitivity test using 40 m or 80 m would not provide physically meaningful guidance for this study. However, we acknowledge that thinner shear-driven turbulence layers may exist in the atmosphere but remain unresolved or undetectable given current operational radiosonde constraints. We have added a short discussion clarifying this observational limitation in the revised manuscript (Lines 221–222).

4. You interpolate 1- and 2-s data to fixed 5- and 10-m spacing. Please state explicitly which coordinate is used for interpolation (geopotential height vs geometric height), and whether pressure is interpolated or recomputed consistently.

We thank the reviewer for this clarification request. In the HVRRD used in this study, geometric height is not provided; only geopotential height is available. Therefore, all vertical interpolation to uniform spacing (5 m for 1-s data and 10 m for 2-s data) was performed with respect to geopotential height. Pressure was interpolated consistently onto the same geopotential-height grid. This has been clarified in the revised manuscript (Lines 209–211).

5. You note that manufacturer smoothing algorithms are proprietary and vary by instrument, and that processed temperature fluctuations may differ substantially from raw data. Please add (i) a brief statement of how this impacts Ri-based detection and ϵ , and (ii) if possible, the radiosonde types/IDs used (or at least a summary by instrument family) to help users assess heterogeneity.

We thank the reviewer for highlighting the importance of instrument-dependent preprocessing and potential heterogeneity.

(i) As noted in the original manuscript, the manufacturer-specific smoothing algorithms applied to radiosonde data remain proprietary and are not publicly disclosed (Wang and Geller, 2025). Consequently, the exact quantitative impact of instrument-dependent preprocessing on Ri and ϵ cannot be explicitly evaluated at present. However, Wang and Geller (2025) reported that processed radiosonde temperature data generally exhibit reduced small-scale fluctuations compared to raw data. Because Ri depends on vertical gradients of temperature and wind, reduced temperature variability may lead to larger estimated Ri values (i.e., more stable stratification) and, consequently, lower ϵ estimates relative to what might be obtained from unsmoothed raw observations. Further assessment using raw radiosonde datasets would be valuable in future work. We have added this discussion in the revised manuscript (Lines 198–201).

(ii) Following the reviewer’s suggestion, we have also summarized the radiosonde instrument types used during the study period (2015–2024). A total of 25 radiosonde types were identified, and these are now listed in the revised manuscript (Lines 201–206).

6. The choice of $Ri_{min} < 0.25$ is motivated as “stricter” given 5–10 m data. Please add one or two sentences clarifying what changes if a more common threshold (e.g., $Ri < 1$) were used, or provide a brief sensitivity.

We thank the reviewer for this comment. We respectfully note that $Ri < 1$ is not generally regarded as the critical threshold for shear instability. The theoretical critical value $Ri = 0.25$ originates from the Miles-Howard stability theorem (Miles, 1961; Howard, 1961), which establishes $Ri > 0.25$ as a sufficient condition for linear stability in shear flow. This threshold has also been adopted in subsequent theoretical studies (e.g., Lindzen, 1981; Palmer et al., 1986) as a physically grounded criterion for Kelvin–Helmholtz instability.

If $Ri < 1$ was adopted in this study, the number and vertical extent of detected turbulence layers would substantially increase, particularly in statically stable but weak-shear conditions. This would lead to broader turbulence occurrence frequencies and systematically larger estimates of turbulence metrics. We added this discussion in the revised manuscript (Lines 128-130).

- Miles, J. W. (1961). On the stability of heterogeneous shear flows. *Journal of fluid Mechanics*, 10(4), 496-508.
- Howard, L. N. (1961). Note on a paper of John W. Miles. *Journal of Fluid Mechanics*, 10(4), 509-512.

7. You define a turbulence layer as a vertical segment where Ri consistently remains below 0.25 and take its thickness as L . Please clarify how “consistently” is implemented in practice (e.g., do you allow 1–2-point gaps above 0.25; minimum number of consecutive levels).

We thank the reviewer for requesting clarification. We apologize for the ambiguity introduced by the use of the term “consistently” in the original manuscript (Line 143), which may have caused confusion regarding its practical implementation. In this study, the term “consistently” refers to consecutive vertical levels satisfying $Ri < 0.25$. A turbulence layer is identified when Ri remains below 0.25 over contiguous levels without allowing gaps above the threshold. We have modified the statement with respect to the definition of L in the revised manuscript (Lines 145–146).

8. Since EE has physical units and you present $\log_{10} EE$, please clarify the convention (e.g., $\log_{10}(EE / 1 \text{ m}^2 \text{ s}^{-3})$) to avoid ambiguity about taking logs of unitful quantities. Also, please restate Z clearly when switching between figures (troposphere/stratosphere depth vs fixed 1 km).

We thank the reviewer for pointing out this clarification. We have now explicitly clarified $\log_{10} EE$ to avoid ambiguity regarding the logarithm of a unitful quantity. Specifically, we added the following sentence: “Here, $\log_{10} EE$ denotes $\log_{10}(EE / 1 \text{ m}^2 \text{ s}^{-3})$, ensuring that the logarithm is taken of a dimensionless quantity.” in the revised manuscript (Lines 404–405).

In addition, we have restated Z clearly in the revised manuscript. Specifically, we added “(i.e., $Z = 1 \text{ km}$)” in Line 430 in the revised manuscript to indicate that the vertical distribution in Fig. 9 is computed using fixed 1-km vertical bins.

9. Please ensure consistent notation and units formatting throughout (Ri_{\min} , ε , EE ; superscripts; “UTLS” definition on first use; consistent use of “turbulence layer” vs “turbulent layer”).

We thank the reviewer for this careful observation. We have thoroughly reviewed the entire manuscript to ensure consistency in notation and units formatting. In particular, we standardized the notation of Ri_{\min} , ε , and EE throughout the text, equations, and figures. The term “UTLS” is defined at its first occurrence in the original manuscript (Lines 280–281). In addition, we ensured consistent use of the term “turbulence layer” across the revised manuscript.