Review of "Turbulence occurrence in the Tropical Tropopause Layer from superpressure balloon observations: distribution and sources," *by Flore Juge, Richard Wilson, and Albert Hertzog*

Summary

This paper estimates the occurrence frequency and distribution of turbulence in the tropical upper troposphere and lower stratosphere in Strateole-2 super-pressure balloon data. The data lack the resolution necessary to directly detect turbulence, so the authors analyze $O(10\text{ m})$ vertical oscillations of the balloons about their neutral density levels to estimate the stability of their environment. When the environment is unstable to shear (gradient Richardson number less than 0.25), it is assumed that it is also turbulent. The authors find these conditions are more common in the vicinity of convection, near which short-period gravity waves are also more common, suggesting a mechanism whereby convection produces gravity waves which then generate regions of instability and turbulence. Away from convection, instances of instability appear to be related to the larger-scale atmospheric state; the Walker circulation and the Quasi-Biennial Oscillation can produce varying magnitudes of vertical wind shear which can affect the distribution of turbulence.

The results presented in this paper are new and present a benchmark against which other theories, observations, and models of atmospheric turbulence can be tested, for example by upcoming Strateole-2 balloons capable of directly detecting turbulence. The methods the authors develop are also novel (updated from those in Wilson et al., 2023) and could potentially be applied to other super-pressure balloon datasets. The contributions this paper makes to the study of atmospheric turbulence, gravity waves, convection, and stratospheric dynamics merit its eventual publication in *Atmospheric Chemistry and Physics*. I have one major comment about the assumptions the authors make when analyzing the balloon data, and several minor comments on various other topics, so my recommendation is the paper be returned for major revisions.

Major Comment

My main concern is whether variability in the GPS-derived horizontal velocities at the frequencies associated with non-isopycnal balloon motion can be described as the vertical shear of the background wind. The vertical balloon motion departs from that of a passive tracer at these frequencies, so I would expect the horizontal motion to depart from that behavior as well. This possibility does not appear to be considered here, in Wilson et al. (2023), or in any other super-pressure balloon papers that I am aware of. If the balloon's inertia is large enough relative to the drag it experiences while traveling through a region of vertical shear, my guess is the shear would be underestimated. The Richardson number would then be overestimated, affecting many results in this paper.

I can think of two ways the authors could address this issue (there may be others). The first would be to extend an analysis of a balloon's momentum (e.g. as in Vincent and Hertzog, 2014) to three dimensions and show that for typical parameter values the balloon's horizontal velocity would closely match the atmosphere's near the balloon's neutral buoyancy oscillation frequency, for an atmosphere with vertical shear of the horizontal wind. The second would be to trace a balloon's horizontal velocities in the observations over one vertical oscillation cycle, or create a composite over many cycles, like in a hodograph plot. If they trace a line in the plane of (zonal, meridional) velocity, the balloon motion is equal to the wind; if they trace an ellipse, it is not.

A related but comparatively minor request: it would be helpful if the response time of the temperature sensors is given. I could not find this information in Wilson et al. (2023), or in any other references for Strateole-2 data. It's probably too short to affect this analysis, but that would be good to know.

Minor comments

The average "turbulent fraction" is estimated to be 0.18 in these data; this number is compared to similar estimates made by Atlas et al. (2025) (0.005 to 0.01) and Alisse et al. (2000) (~0.18). I appreciate the comparison, but I am not sure if 0.18 is a higher or lower number than would be expected in this part of the atmosphere. Would a gravity wave parameterization, for example, predict wave breaking at these altitudes that would produce similar fractions? Is a turbulent fraction of 0.18 consistent with estimates of vertical tracer transport by mixing? Answering these questions is outside the scope of this paper, but I would appreciate some discussion about how these results can be used to improve our understanding of the topics covered in the first paragraph of the Introduction.

As the authors mention, values of the Richardson number less than 0.25 are no guarantee of the presence of turbulence. I understand why this approximation must be made for this analysis, but the results would be made more robust if the data are evaluated for instability to convection (vertical gradient of potential temperature equal to zero) as well. This would correspond to a Richardson number of zero, so it would be a more conservative estimate, but it would be encouraging if it produced results similar to, for example, those in the top panel of Figure 10. Regardless of whether the flow is unstable to convection or shear, once turbulence is present, I would expect potential temperature to be homogenized, potentially making this the more robust metric.

I do not ask the authors to redo all their calculations using this metric. I would be satisfied if an explanation is given as to why the criterion the authors use ($Ri < 0.25$) is better than this one, or if they could show that a few of their key results are unaffected if they use it. Since they already calculate the vertical gradient of potential temperature to estimate the buoyancy frequency, I hope this is not too much extra work.

The logic of the first paragraph of the Introduction section is a little confusing. It starts with the Brewer-Dobson circulation and vertical transport in the tropical atmosphere, then moves on to a general discussion of waves and turbulence. Turbulence is important for maintaining the Brewer-Dobson circulation, but that connection is never made clear. This could be improved by expanding on what "dynamically couple" means on line 24.

Line 27: I think it would be more accurate if "either occurs due to" is replaced with "occurs due to, for example," since other pathways to turbulence are possible (e.g. Achatz, 2005).

Figure 1: the sinusoidal curve is not described in the figure caption.

Table 1: it might be helpful to have a column of flight numbers, since these are mentioned elsewhere (line 113, Figure 3 and 4 captions).

Lines 90, 91: Looking at the dates in Table 1, I think C0 began in November 2019 and C1 began in October 2021.

Section 3.1: if I hadn't read Wilson et al. (2023), I would have thought that this is the first time that the buoyancy frequency and Richardson number had been estimated from balloon oscillations about their neutral density surface. The method described here is different than the one used in that paper, but it is clearly inspired by it. Please mention the analysis of Wilson et al. before Equation 2 (equivalent to Equation 3 in Wilson et al.), and at some point discuss the difference between your method and the "correlation" and "Richardson" methods in that paper (their Sections 3.2.2 and 3.2.4).

Lines 164-166: I appreciate that the emphasis in this paper is on the Richardson number, but since the lower panels show the vertical gradient of potential temperature, a simpler discussion would focus on instability to convection rather than shear.

Section 3.2.1: what processes contribute to varying magnitudes of (delta)z?

Table 2: it might be helpful to include a column of the value of the average turbulence probability for each region.

Lines 317, 355-356, 364-365: are the tendencies in each region statistically significant?

Figure 9, lines 325-328: the relative sizes of the bars each of the lower panels appears to mostly reflect the amount of data in each region. It might be more insightful to divide the number of data points in each region in each range of turbulence fractions by the total number of data points in the region.

Figure 10, lower panels: see above comment on lower panels of Figure 9.

Section 5: I find the analysis in this section convincing, but the results in Figure 11 offer an alternative interpretation: the difference between convection over land and over ocean. Convection over land is more vigorous, possibly producing higher energy high-frequency gravity waves and more turbulence. The trend in Figure 11 for Africa might support this, since I would expect the Walker circulation to have less of an influence there. Do you think this mechanism is plausible?

Wording comments, typos, etc.
Line 3: replace "under" with "by."

Line 48: "turbulence is involved" is vague, and "even though turbulence is involved in the global tracer and heat budgets" can be deleted.

Line 56: the wording here is confusing to me, because I do not think you mean to imply that the stratosphere is a boundary layer.

Line 59: move “Strateole-2” to before “stratospheric,” and replace “fly” with “flew.”

Line 77: replace “providing” with “provide.”

Lines 80, 86, 93, 94, and maybe elsewhere: inconsistent abbreviation of “Table” and “Figure.”

Line 99: delete “actually.”

Line 100, and in the region names: it is unusual to me to see both South and North America contained in the term “America.” Normally, I would expect them to be combined as “the Americas” (the region name would just be “Americas”), but different journals have different conventions for geographic names, so I may be wrong. Another option would be to use “South America,” even though one balloon appears to fly over Panama.

Lines 104-105: the way this sentence is written, the “which” refers to the instabilities, not to the gravity waves.

Line 112: delete “along their trajectories.”

Line 115: delete “at the same time,” or rephrase (vague, unclear).

Line 119: replace “. ERA-5” with “and.”

Line 120: delete “consistent” (vague).

Lines 121-123: combine these two sentences and delete “covering the whole planet.”

Lines 137: replace “panel” with “panels.”

Line 160: replace “panel” with “panels” and correspondingly change “shows” to “show.” Delete “for instance.”

Line 172: “LITOS” is undefined.

Figure 4 caption: replace “up” with “upper panels” and “down” with “bottom.”

Figure 7 caption: replace “panels” with “rows.”

Line 258: I suggest replacing “exhibits” with “presents,” “shows,” or “describes.” I also suggest replacing “differences in the distribution” with “distributions.”

Line 265: add “for each region” after “nearly constant.”

Figure 8 title: “Ri_env” is undefined.

Figure 9 caption: replace “lower panel” with “lower panels.”

Lines 427-428: looking at the time axis on Figure 14, I think this is the wrong date range.

I suggest switching to the past tense when discussing results in the Conclusion section and replacing, for example, “is” with “was” on line 431 and “are” with “were.”

Line 437: I think there’s a typo where the numbers with dashes through them should have been deleted but weren’t.