

Response to reviewer #1 ACP 2026

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.

I would like to make a correction to the Major Comment in my review, and offer a suggestion for another analysis that could address the issue it raises. I meant to say that, if the trace of balloon zonal (or meridional) velocity versus altitude over one cycle forms a line, then the estimated shear should be correct. Again, if they trace an ellipse, the balloon and wind horizontal velocities would not be equal and the estimated shear would contain errors. My apologies for the incorrect wording in my original comment.

Another analysis that could address my comment would be to calculate the cross-spectrum of balloon altitude and zonal velocity, or of balloon altitude and meridional velocity. If the phase difference between the altitude and velocity is zero in the frequency range of the balloons' oscillations about their neutral density surfaces, then it is possible that the balloon and wind velocities are equal.

Thank you for this comment and for your suggestions. Indeed, this is a very relevant question, and providing a definitive answer is not an easy task.

As suggested by the reviewer, we extended the analysis of a ball's momentum proposed by Vincent and Hertzog (2014) to two dimensions, (x, z), that is to horizontal and vertical motions.

$$(M_b + \eta M_a) \frac{dw_b}{dt} = -g(M_b - M_a) - \frac{1}{2} \rho_a \cdot C_d \cdot A_b \cdot (w_b - w_a) |w_b - w_a| + (M_b + \eta M_a) \frac{dw_a}{dt}$$

We can rewrite equation (1) on the horizontal:

$$(M_b + \eta M_a) \frac{du_b}{dt} = -\frac{1}{2} \rho_a \cdot C_d \cdot A_b \cdot (u_b - u_a) |u_b - u_a| + (M_b + \eta M_a) \frac{du_a}{dt}$$

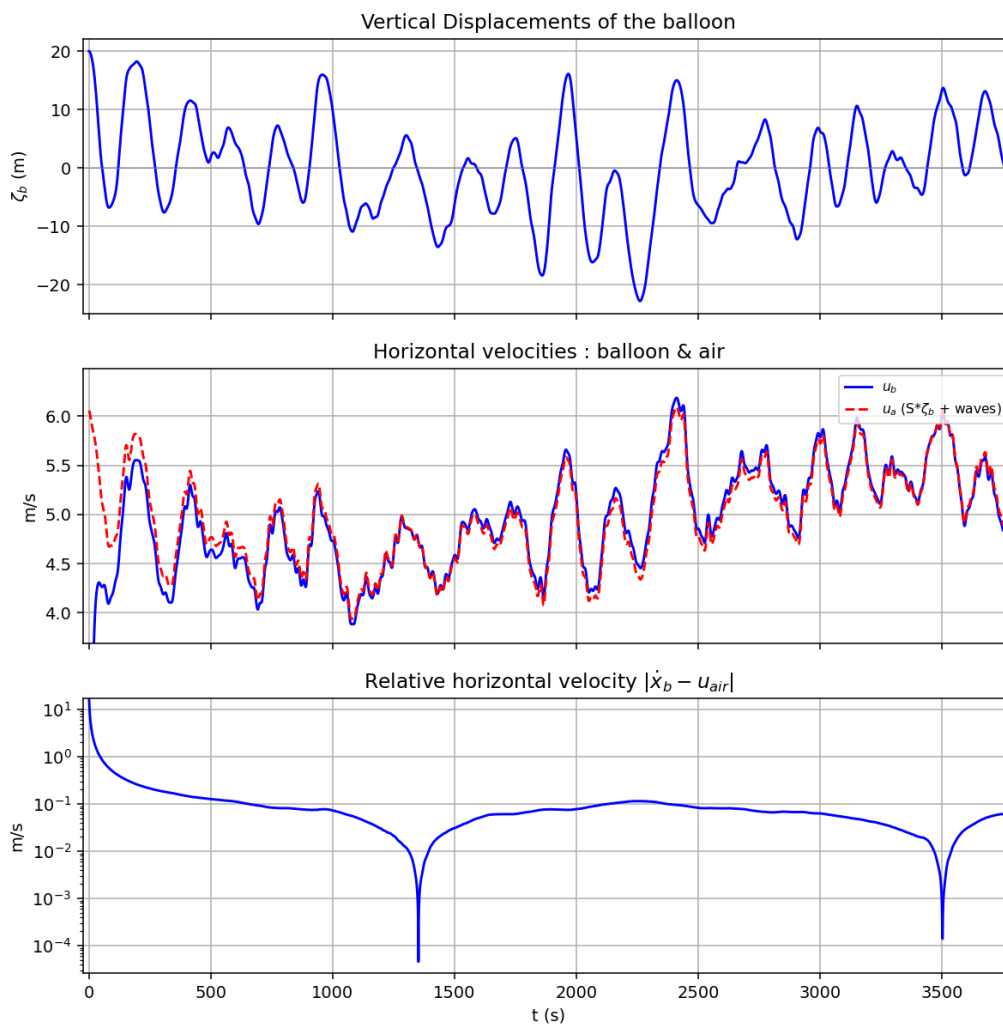
which is consistent with Maxey and Riley (1983), and Auton et al. (1988). For a sphere, $\eta=1/2$. At the equilibrium level $M_b = M_a = \rho_a V_b$. Assuming the relative wind to be $u_r = u_b - u_a$, Eq (2) describes an equilibrium between inertia and drag:

$$(M_b + \eta M_a) \frac{du_r}{dt} = -\frac{1}{2} \rho_a \cdot C_d \cdot A_b \cdot u_r |u_r|$$

We resolved the coupled differential equations for the vertical and horizontal motions. For the vertical motions, we took the equation of Vincent & Hertzog (2014) with a forcing term w' (waves and turbulence). The equation (2) above describe the horizontal motion, with u_a (the velocity

of the air in which the balloon drift) including a forcing term u' related to w' for waves by the polarisation relationship. The coupling between the two equation is achieved by taking into account the vertical motion, i.e. the shear S in u_a : $u_a = u_0 + u' + S \cdot z_b$ (z_b is the vertical displacement of the balloon relative to the equilibrium position, u_0 a background velocity).

In this simulation the forcing terms w' and u' are built by inverting (inverse fft) a realistic spectrum of gravity waves (up to the BV frequency) and a turbulent spectrum for frequencies larger than N . The initial condition is far from equilibrium (in particular $u_r(t=0) = 10$ m/s). Some results of the simulation for about one hour of simulation are shown below.



Concerning the horizontal motion, there is no restoring force, the drag acts as a simple friction that brings the relative velocity u_r back to zero, without creating a spectral peak (no restoring force) and without phase shift between du_r/dt and u_r . We therefore conclude, and observe with the simulation, that the balloon is a very good tracer for horizontal winds (which is not the case for the vertical wind!)

- *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.*

The primary sensor we use to measure temperature during the balloon flights is a thermistor. Due to procurement issues, we fly thermistors of two different sizes, and the thermistor response time varies with its size: the larger the size, the larger the response time. For our larger thermistors (230- μm diameter), the response time is 0.5 s, hence much smaller than the typical timescale (110 s) we use to estimate the Richardson numbers. We have added the response time of the temperature sensor in the revised version of the paper.

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.*

One major outcome of our study is that the tropical stratosphere is actively turbulent a significant fraction of the time: by selecting a "conservative" threshold for Ri (0.25), we found a turbulent fraction of $\sim 20\%$ which is more than one order of magnitude than that of the recent study of Atlas et al. (2025). This might indeed suggest that turbulent processes could be more important than previously anticipated regarding vertical transport in the stratosphere. Though, our study is only a first step toward providing direct estimates of turbulence effects in vertical transport, and we are not able at this stage to state whether our estimate of the stratospheric turbulent fraction is consistent with state-of-the-art values of turbulent diffusivity.

Regarding wave-induced turbulence, current gravity wave parameterizations are to our knowledge too crude to predict the time fraction associated with wave breaking. Gravity-wave drag parameterizations at best describe waves as packets, which either propagate without interacting with the background flow, or fully dissipate at a given altitude. In the atmosphere that our balloons probe, the situation is likely far from this ideal world, and wave packets may

induce instabilities only during a fraction of the wave period. It is thus not easy to relate our balloon-borne results with outcomes of gravity-wave drag parameterizations. We have added a few sentences in the revised conclusion on these topics.

- *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.*

We thank the reviewer for this insightful comment. We agree that the occurrence of a negative vertical gradient of potential temperature (i.e., $Ri < 0$, corresponding to convective instability) provides a more conservative criterion for identifying regions that are unstable to convection. However, this criterion does not capture the full range of turbulent motions expected in stably stratified shear flows.

In particular, stratified shear flows may sustain turbulent mixing while remaining characterized by positive potential temperature gradients, as demonstrated in recent Lagrangian studies of mixing in stratified shear flows (Zhou et al., 2025). The classical Miles–Howard criterion indicates that a necessary condition for inviscid shear instability is a gradient Richardson number smaller than 0.25. Therefore, restricting the analysis to $Ri < 0$ would exclude turbulent regions generated by shear instabilities that do not become convectively unstable. In other words, the presence of turbulence does not necessarily imply that the mean potential temperature profile has been homogenized enough to produce $Ri < 0$ throughout the turbulent layer.

Following the reviewer’s suggestion, we nevertheless evaluated the sensitivity of our results to the more restrictive criterion based on convective instability ($Ri < 0$).

The figure below is the same as Figure 6 in the paper, but using $Ri < 0$ as indicator of turbulence. As expected, we obtain smaller values of turbulent fractions than those inferred with $Ri < 0.25$ (11% during nighttime instead of 18%). The curves are slightly different than in the paper but essentially exhibit the same tendency: turbulent fractions decreasing with increasing $|\Delta z|$. We nevertheless note that a larger difference appears between daytime and nighttime turbulent fraction estimates: the higher daytime values are probably caused by the fact that, considering

only the vertical gradient of potential temperature, we give more weight to the temperature term in the uncertainty of the turbulent fraction estimate, and this term is more sensitive to daytime noise level.

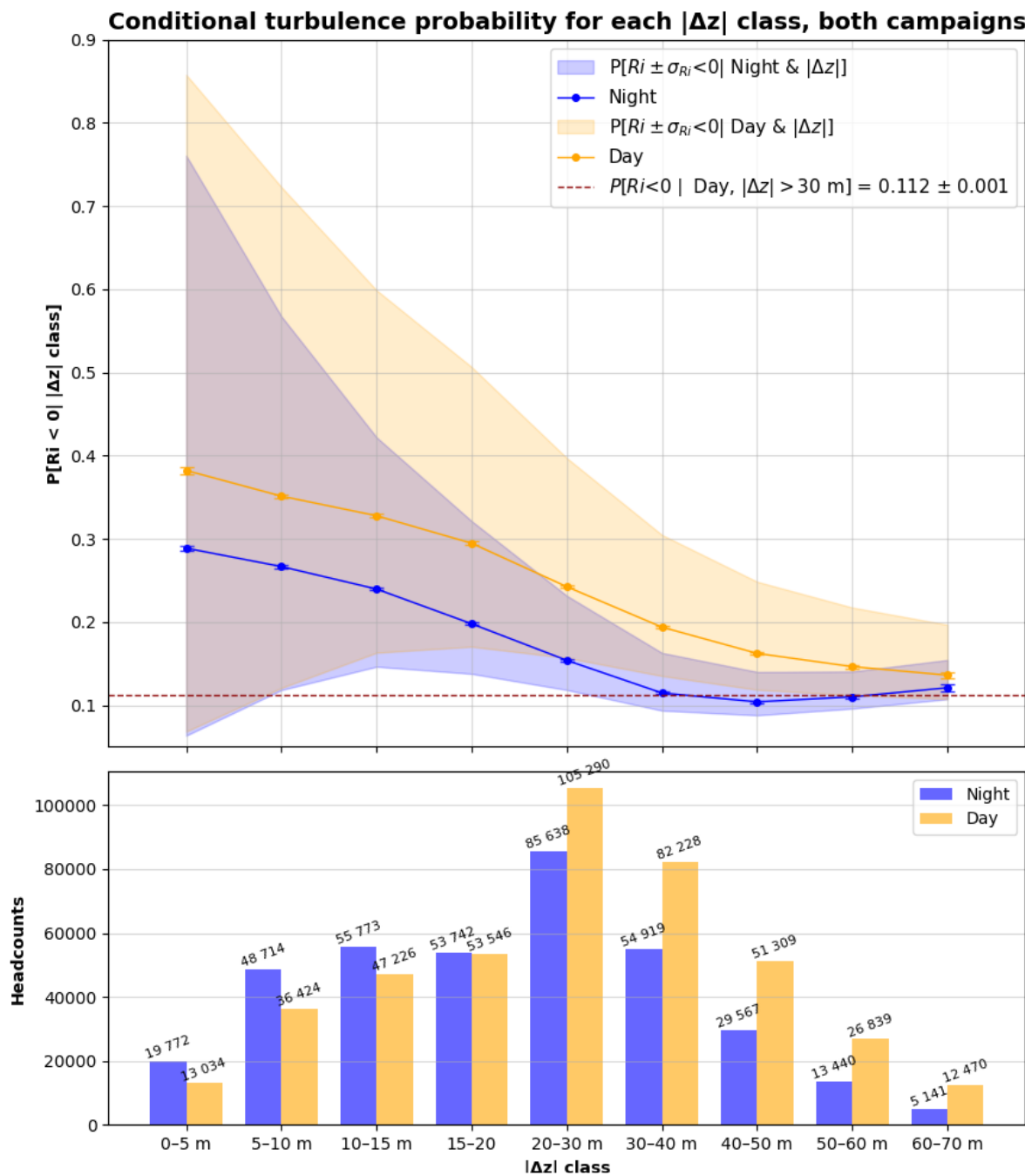
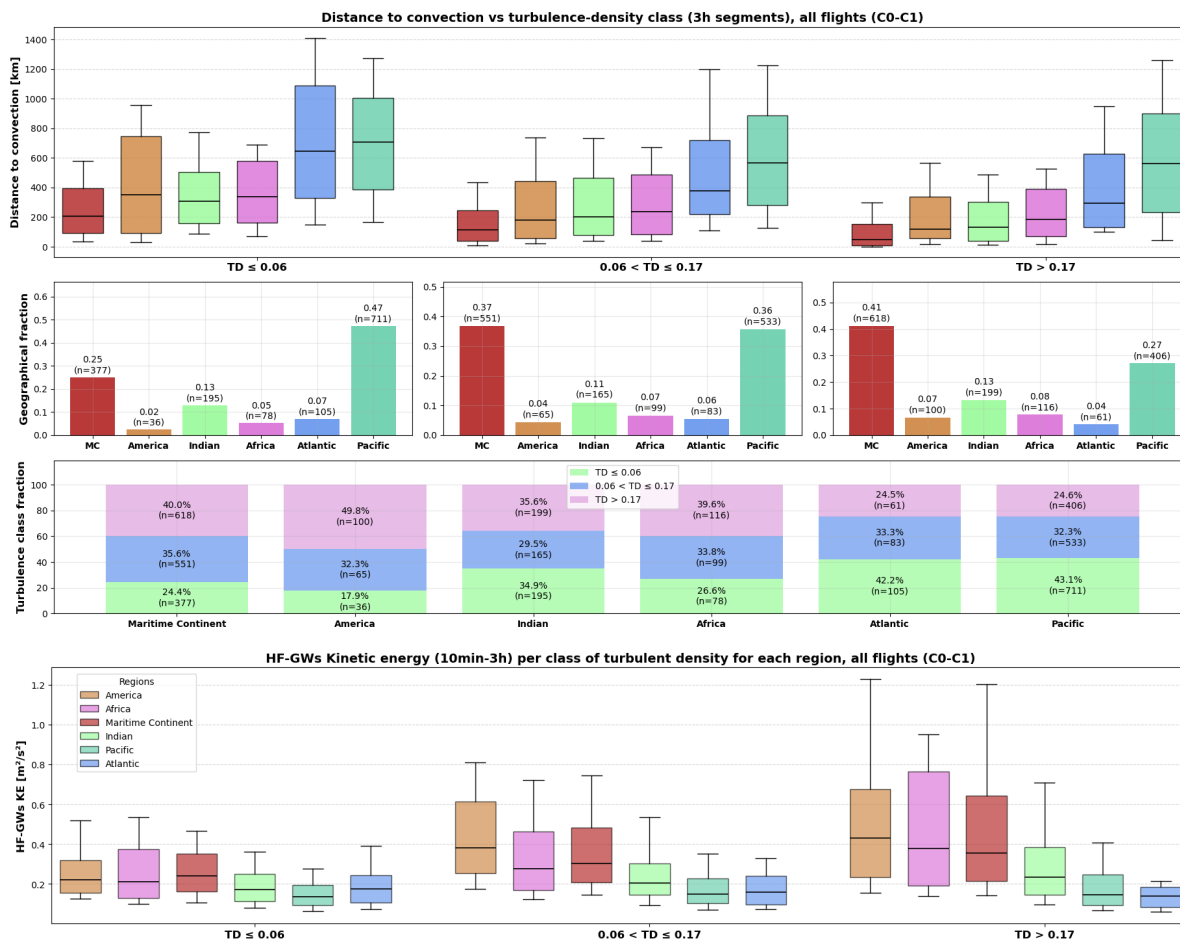


Figure 10 shows HF-GWs Kinetic Energy distributions of all regions for different classes of distance to convection, so it does not involve Richardson number estimates, and will remain exactly the same regardless of the threshold we use for determining the turbulent regime. On the other hand, we have redone Figures 9 and 11 using the $Ri < 0$ threshold. The shape and order of the distributions remain very similar to those shown in the paper. The only notable difference is in the boundaries of equiprobable bins of turbulent density: since we detect less turbulent events, those values are lower.



- 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.

We agree, and propose to rewrite the first paragraph of introduction as:

"A major feature of the middle-atmosphere large-scale dynamics is the Brewer-Dobson meridional circulation (BDC), which is associated with an ascending branch in the tropics, poleward transport at mid-latitudes and subsidence at polar latitudes (brewer, 1949; butchart, 2014). This residual circulation is fundamentally driven by wave–mean flow interaction: the breaking and dissipation of atmospheric waves induce a net deposition of momentum, which forces a slow meridional overturning circulation (Andrews, 1987; Holton, 2013; Butchart, 2014).

The BDC ascending branch is notably responsible for the upward transport of tropospheric species, such as water vapor (Plumb, 1996), and the tropical tropopause therefore constitutes the gateway to the whole middle atmosphere (Fueglistaler, 2009). Beyond its role in driving the mean circulation, planetary and smaller-scale waves generated by deep convection dynamically couple the troposphere and the stratosphere in the tropics through vertical transport of energy

and momentum (Andrews, 1987). Their upward propagation from convective sources and their breaking in the lower stratosphere eventually drive the Quasi-Biennial Oscillation (QBO) in tropical zonal winds (Fritts, 2003; Jensen, 2016; Baldwin, 2001). In the tropical lower stratosphere, wave breaking occurs due to, for example, the saturation of the wave amplitudes associated with the exponential decrease in atmospheric density or when waves encounter critical levels, i.e. when their ground-based phase speed approaches the background wind speed (Fritts, 2003). Wave breaking is associated with the development of shear-driven instabilities, such as Kelvin–Helmholtz instabilities (KHIs), or convective instabilities, which act as efficient generators of small-scale turbulence and irreversible mixing (Dornbrack, 1998; Fujiwara, 2003; Yamamoto, 2003; Mega, 2010)."

- *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.*

We agree, and have modified the article accordingly.

- *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).*

Actually, the flight number and the configuration of the instruments in the gondolas are already contained in the name of the flight, for example: C0_01_STR1 means campaign C0, flight 1, and configuration STR1, so level flight around 20.5km. We have added details in the legend of the table to clarify.

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

We agree, and have corrected the paper.

- *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).*

We agree and propose to add the following paragraph to the section 3.1:

"It is then possible to use these vertical oscillations of the balloons to infer some information about the flow regime. An initial attempt to analyse these data was carried out by Wilson et al. (2023). The authors developed two distinct methods for detecting turbulent events with a time resolution of one hour. The first method consisted of estimating a Spearman or Pearson correlation between the balloon's vertical displacements Δz_B and the resulting increments in potential temperature $\Delta\theta$ (30 s) over one-hour segments: a non-positive correlation indicating turbulent mixing. The second method involved estimating Richardson numbers from estimates of vertical gradients of potential temperature and horizontal winds, derived from the ratio of the covariance of the increments ΔX and Δz_B to the variance of Δz_B on 1h-segments. Turbulent events were defined as segments satisfying $Ri < 0.25$. In this study, we will also use these balloon oscillations around their equilibrium density level, but not in the same way. We will consider them as short vertical profiles in order to estimate the local vertical gradients of any measured variable X (u , v , T , or θ) with a much higher time resolution. More precisely, the time resolution is equal to half the oscillation period, that enables to detect shorter events than in the previous methods. "

- *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.*

We agree with the suggestion, and have corrected the article accordingly.

- *Section 3.2.1: what processes contribute to varying magnitudes of (Δz) ?*

We do not know precisely what processes contribute to the varying magnitudes of $|\Delta z|$, but we suspect that waves with frequencies close to the oscillation frequency of the balloons could affect these amplitudes. Some aerodynamic instabilities on the boundary layer of the balloon could also increase the amplitude of these oscillations during day time, associated with the solar heating of the balloon envelope. We have added a sentence in the paper on this topics.

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

We agree, and have added the averaged turbulent fractions for every regions in the table.

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

We display below p-values obtained from pairwise comparisons of turbulent density (resp. HF-GWs kinetic energy) distributions between geographical regions, for different classes of distance to convection (resp. turbulent density), using non parametric post-hoc statistical tests (Dunn test with Bonferroni correction). A p-value < 0.05 (red values in the table below) indicates a significant difference between two regions.

Most of the pairwise comparisons are statistically significant. And those which are not statistically significant seem to evidence a land/ocean contrast.

Dunn post-hoc test (Bonferroni corrected)
Pairwise comparisons between regions

	0-99 km							99-396 km							> 396 km					
	America	Africa	MC	Indian	Pacific	Atlantic		America	Africa	MC	Indian	Pacific	Atlantic		America	Africa	MC	Indian	Pacific	Atlantic
America								America							America					
Africa	8.06e-03							Africa	1.39e-01						Africa	1.00e+00				
MC	5.19e-09	1.00e+00						MC	5.78e-07	4.53e-02					MC	1.00e+00	1.00e+00			
Indian	1.54e-15	5.38e-04	8.15e-06					Indian	4.25e-19	3.54e-13	1.62e-13				Indian	4.61e-03	2.17e-04	3.06e-09		
Pacific	1.33e-19	2.20e-06	4.78e-11	1.00e+00				Pacific	4.94e-22	2.85e-16	6.72e-21	1.00e+00			Pacific	2.85e-09	8.10e-16	7.13e-36	4.04e-04	
Atlantic	2.63e-13	3.56e-06	1.16e-05	3.37e-02	3.50e-01			Atlantic	6.24e-28	4.79e-22	1.76e-21	6.19e-05	1.91e-04		Atlantic	2.61e-01	8.51e-02	1.64e-04	1.00e+00	8.69e-09

Kinetic Energy of HF-GWs VS Distance to convection

Dunn post-hoc test (Bonferroni corrected)
Pairwise comparisons between regions

	(Tdens ≤ 0.11)							(0.11 < Tdens ≤ 0.25)							(Tdens > 0.25)					
	America	Africa	MC	Indian	Pacific	Atlantic		America	Africa	MC	Indian	Pacific	Atlantic		America	Africa	MC	Indian	Pacific	Atlantic
America								America							America					
Africa	8.1e-01							Africa	1.0e+00						Africa	1.0e+00				
MC	1.0e+00	8.3e-02						MC	1.0e+00	1.0e+00					MC	1.0e+00	1.0e+00			
Indian	1.3e-01	1.0e+00	6.6e-06					Indian	3.4e-04	1.5e-02	2.2e-10				Indian	2.3e-07	1.8e-04	2.6e-08		
Pacific	1.9e-06	5.7e-03	1.5e-40	8.2e-06				Pacific	1.1e-12	7.0e-13	1.6e-57	4.3e-05			Pacific	2.9e-25	1.1e-20	2.4e-49	9.0e-07	
Atlantic	8.5e-02	1.0e+00	1.2e-04	1.0e+00	2.7e-02			Atlantic	1.3e-07	4.5e-06	1.9e-14	1.5e-01	1.0e+00		Atlantic	1.5e-19	5.0e-16	1.0e-21	7.7e-07	4.2e-01

Kinetic Energy of HF-GWs VS Turbulent density

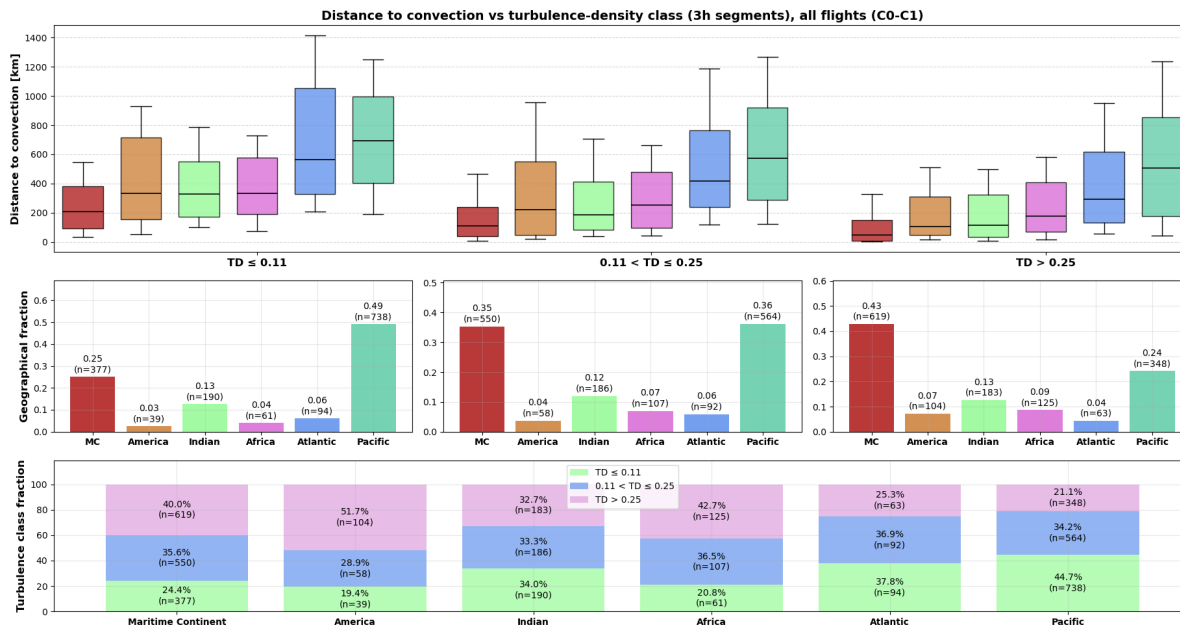
Dunn post-hoc test (Bonferroni corrected)
Pairwise comparisons between regions
Distance to convection

	(Tdens ≤ 0.11)							(0.11 < Tdens ≤ 0.25)							(Tdens > 0.25)					
	America	Africa	MC	Indian	Pacific	Atlantic		America	Africa	MC	Indian	Pacific	Atlantic		America	Africa	MC	Indian	Pacific	Atlantic
America								America							America					
Africa	1.0e+00							Africa	1.0e+00						Africa	1.0e+00				
MC	3.9e-02	7.7e-02						MC	4.5e-03	5.8e-05					MC	4.0e-04	3.5e-10			
Indian	1.0e+00	1.0e+00	1.0e-04					Indian	1.0e+00	1.0e+00	5.5e-09				Indian	1.0e+00	8.7e-01	3.4e-06		
Pacific	3.1e-04	1.1e-08	7.9e-80	3.4e-22				Pacific	1.7e-06	2.7e-11	1.2e-92	5.2e-22			Pacific	3.3e-12	6.1e-08	5.3e-78	1.8e-18	
Atlantic	1.7e-02	1.0e-04	2.3e-21	1.3e-07	1.0e+00			Atlantic	1.2e-02	8.5e-04	7.3e-20	2.5e-06	1.0e+00		Atlantic	8.4e-04	7.7e-02	2.0e-15	1.2e-04	1.0e+00

Distance to convection VS Turbulent density

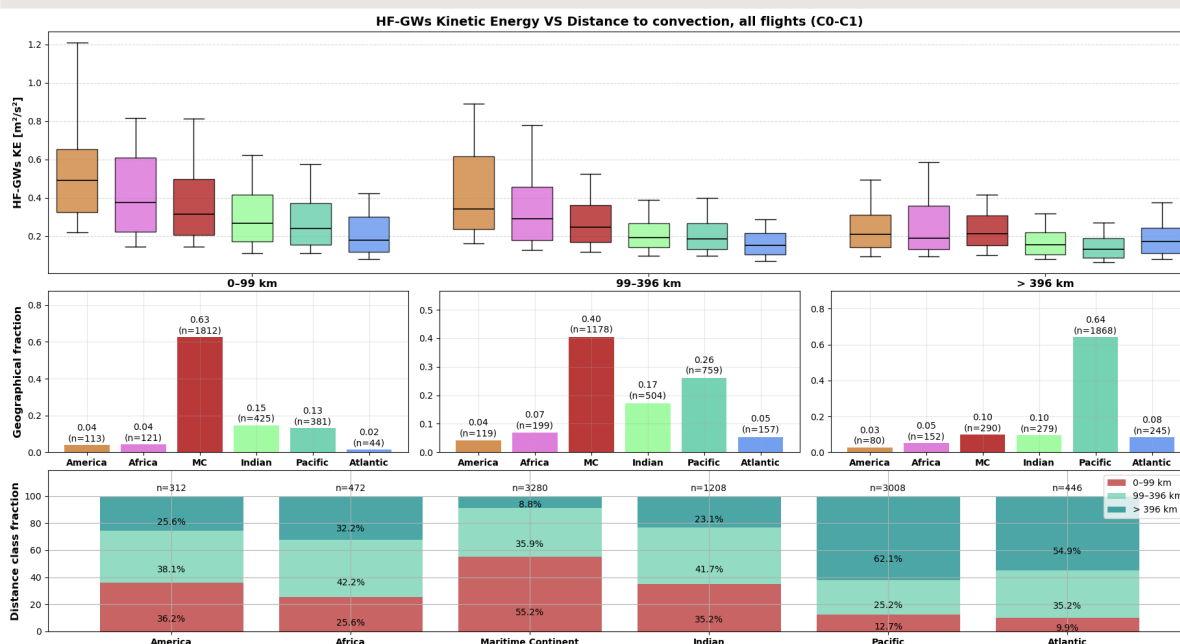
- Figure 9, lines 325-328: the relative sizes of the bars each of the lower panels appears to mostly reflect the amount of data in each region. It might be more insightful to divide the number of data points in each region in each range of turbulence fractions by the total number of data points in the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

We agree with your suggestion, and have added a third panel to the 9th and 10th figures.



The third panels of Figure 9 present the share of each turbulent density (TD) class in the total data points associated with each region. We can notice that the majority of data collected over the Atlantic and Pacific Oceans correspond to the lower TD-class, 37.8% to 44.5%, respectively, while only 25.5% to 21.1% of the data associated with the higher TD-class. Over South America, Africa, and the Maritime Continent, we can observe the opposite trend: higher shares of data corresponding to the higher TD-class (51.7%, 42.7% and 40%, respectively), indicating that the turbulence is more frequent and concentrated over the continents than over the oceans.

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



The third panel of Figure 10 shows that the share of data points associated with proximity to convection is very low for the Atlantic and Pacific oceans, indicating again that these regions are less convective. For the Maritime Continent, we observe the opposite trend: a majority of data

points associated with proximity to convective systems, confirming that it is a very convective region.

- *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?*

You are absolutely right: our results in Section 4 indeed suggest that waves generated by convection that are breaking in the stratosphere are the main source of turbulence in the tropical lower stratosphere. Over continents, we were actually able to show a positive correlation between the balloon proximity to convective systems/the activity of high-frequency gravity waves/and the stratospheric turbulence fraction. We therefore believe that, as you suggest, the contrast between continental and oceanic convection is the primary driver of turbulence variability in the lower stratosphere.

Yet, the question we address in Section 5 is different: since we still observe turbulence patches over oceans, and those seem to be unrelated to convection or to high-frequency gravity waves (as illustrated in Figure 11), we investigate in this Section other possible sources of stratospheric turbulence. We focus in particular on the Pacific Ocean, where we detect concentrated turbulent events (though not as numerous as over continents). Section 5 shows that at least some of them, during the C1-campaign, may be associated with the Walker circulation, which produces favorable large-scale shear conditions for the generation of turbulence in the lower stratosphere.

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.

We have corrected all the typos in the next version. Thank you.

Response to reviewer #2 ACP 2026

Summary

Measurements from superpressure balloon (SPB) campaigns are used to deduce turbulence occurrence near the tropical tropopause. Specifically, the naturally occurring vertical oscillations of the balloon position are exploited to estimate vertical derivatives of temperature and horizontal wind components, which in turn can be used to estimate the gradient Richardson (Ri) number. The common threshold of $Ri < 0.25$ is used to designate flight segments to turbulence occurrence. The campaign data used are found to include an inferred mean turbulent fraction of 18%. Most turbulence is found in close proximity to deep convective activity and seems to be associated with enhanced high-frequency gravity wave activity. Enhanced turbulence rate

is also found in large-scale shear zones associated with the QBO and upper branches of the Walker circulation.

Estimates of turbulence occurrence near the tropical tropopause are important for improved understanding of tracer transport and its potential consequences for climate feedbacks. But unfortunately these estimates are rare, primarily because of the lack of sufficiently high-resolution observational data. So the present analysis based on unique SPB measurements is undoubtedly beneficial and helps to fill this gap. The paper is well written and mostly easy to follow.

I have three general concerns that I think are important to be addressed in the paper and which may require extra work, which is why I selected "major revisions". I also have a few minor comments (see below), but otherwise support publication of this paper.

General concerns:

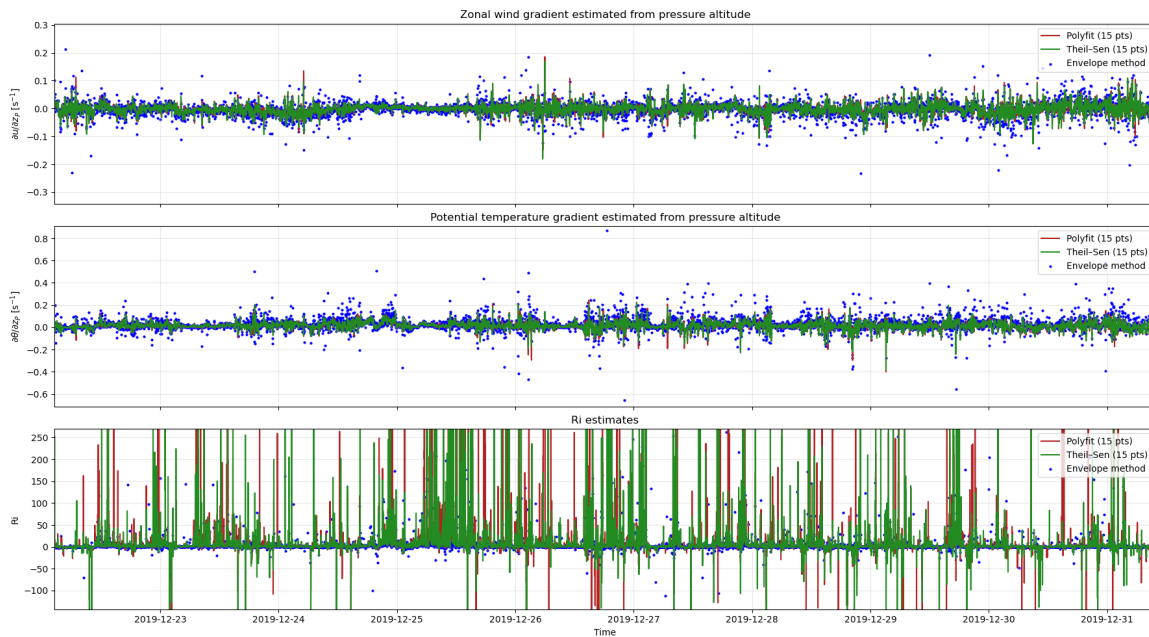
- *The estimated vertical gradients are not instantaneous and therefore don't constitute true vertical gradients because they are affected by the horizontal drift of the balloon. This means that the inferred vertical gradient includes contributions from changes in time and horizontal distance of the field of interest (T , u , v). This potential issue needs to be discussed somewhere in the paper. Perhaps such a discussion is included in previous published work by the authors, in which case such a discussion could be brief. I did have a look at Wilson et al. (2023), where the effect of changes in time are discussed for the case of temperature gradients and the authors concluded such effects to be negligible. But I still wonder about the respective effect on the wind and more importantly the effect due to the horizontal drift of the balloon. For a 10 m/s horizontal velocity the balloon drifts 1 km in 100 s (roughly the measurement resolution used for vertical gradient estimates), so horizontal gradients at a km-scale would play a role. I wonder if situations with large-amplitude high-frequency gravity waves and/or turbulence could create strong enough horizontal gradients at this scale so that the inferred vertical gradients are "contaminated"? There could also be a corresponding sampling issue for turbulent patches if their horizontal extent is less equal 1 km.*

We thank the Reviewer for this very important comment. This potential contamination due to the fact that the vertical gradients are estimated with observations recorded typically 110 s apart, and the fact that the balloon has drifted horizontally during that time interval, was indeed overlooked in the previous version of the paper. We now devote Appendix A2 to this question.

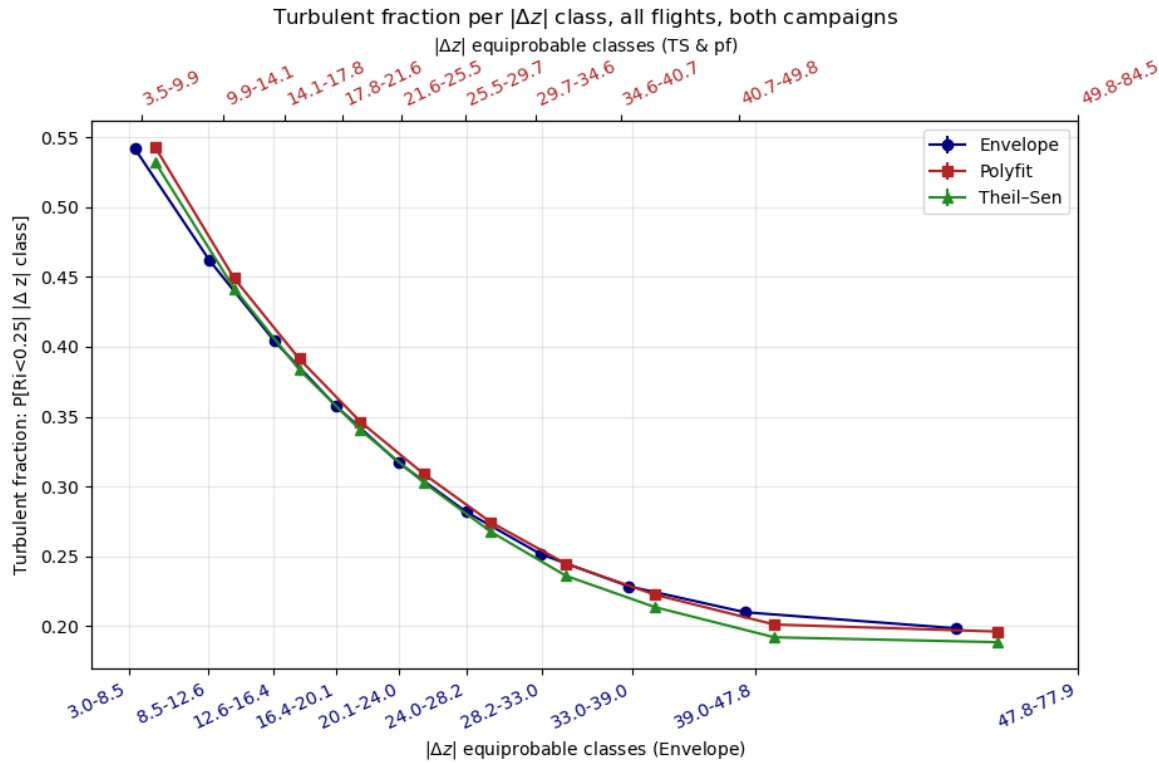
Our main argument relies on the fact that the period associated with the balloon vertical oscillations are shorter than the Brunt-Väisälä frequency, and therefore than any gravity or planetary waves. Hence, if we correlate the variations of temperature (ΔT) or horizontal velocities ($\Delta u, v$) with those of the vertical displacements of the balloon, the wave component in ΔT or $\Delta u, v$ should vanish, and only the component associated with the vertical gradient will remain. This will be also

the case is the balloon is flying in a (horizontal or temporal) gradient of background temperature or wind, since, once again, the variations of T or u, v produced by these gradients will not be correlated with the balloon vertical oscillations.

We have therefore re-estimated the vertical gradients with correlations computed over two balloon oscillation periods, i.e. slightly more than 7 minutes in average. Two different estimators of the correlation have been used: either the linear regression coefficient or the Theil-Sen estimator. The figure below show a sample of the zonal-wind and potential temperature gradients estimates, either with the method used in the paper or with these two novel estimators. The estimators based on correlations provide smoother vertical-gradient estimates, yet time periods of enhanced/reduced gradient variations seem to correspond between all estimators.



We have furthermore re-computed the turbulent fraction estimates with these new estimators. The figure below shows the turbulent fraction obtained with all estimators of the vertical gradient as a function of the amplitude of the balloon vertical oscillations. The turbulent fraction associated with the envelope estimator used in the paper is in blue, and the turbulent fractions obtained with the novel vertical-gradient estimators are shown in red and green. The three curves look very similar, and converge toward the turbulent fraction reported in the paper, i.e. $\sim 20\%$.



We are therefore confident in our estimates of the turbulent fractions, which are central to our study.

- *The campaign data is certainly great, but sampling is still sparse over some regions. Fig. 8 indicates that over South America, the Atlantic and Africa only few flight tracks contribute to the total data. So the turbulent fractions necessarily suffer from sparse sampling there (perhaps most importantly in time). This sampling problem is a bit hidden by simply looking at the number of measurements (as opposed to contributing flight tracks) and should be discussed more.*

We agree with the reviewer that the sampling is less extensive over South America, the Atlantic and Africa than over the Maritime Continent and the Indian Ocean. The number of measurements alone does not fully reflect the effective sampling, because many measurements may originate from a relatively small number of flight tracks and therefore may not be statistically independent.

To account for this, we have quantified the uncertainty of the regional turbulent fractions using confidence intervals (error bars), and we now explicitly report the number of contributing measurements and flight tracks for each region. We have also added a discussion emphasizing that the regional estimates over South America, the Atlantic and Africa are based on fewer trajectories (7 flights for Africa, 8 for Atlantic ocean and 9 for America) and therefore may be more sensitive to temporal sampling than those over the better-sampled regions.

While these regions still contain several thousand measurements, we agree that the limited number of flight tracks is a caveat to consider, and we have clarified this point in the revised manuscript.

- *The interpretation about the large-scale setup making turbulence more likely in regions associated with the QBO/Walker circulation shear zone (section 5) is quite interesting. Some quantitative evidence is shown in Fig. 14, but I wonder if it could be made more specific by showing a vertical cross section (longitude-height) of zonal wind and overturning streamfunction? There is quite a bit of discussion in terms of the schematic shown in Fig. 13, but such a discussion is necessarily somewhat speculative and should be made more quantitative where possible. The ERA5 data should include the required information about the large-scale circulation and allow such a quantification.*

Here is represented, in the first panel, the mean zonal wind during November 2021-February 2022 (C1 campaign), within the $\pm 5^\circ$ latitude band, from the ERA5 dataset, as a function of longitude and pressure level between 300 and 30hPa. The ERA5 fields are time-averaged and latitude-weighted mean over the $\pm 5^\circ$ latitude band using $\cos(\phi)$ weighting to account for the varying grid-cell area with latitude. The black contours represent the zonal overturning streamfunction Ψ , expressed in $[10^5 \text{ kg/s}]$, calculated from the zonally varying component of the latitude-weighted mean vertical velocity:

$$\Psi(\lambda, p) = -\frac{2\pi \cdot a}{g} \cdot \int_0^\lambda \omega'(\lambda', p) d\lambda', \quad \text{with: } \omega' = \omega - \langle \omega \rangle_\lambda$$

Here, ω denotes the time (and latitude) weighted mean vertical velocity in pressure coordinates, $\omega = dP/dt$, within the $\pm 5^\circ$ latitude band, λ is longitude, and a is Earth's radius.

With this convention:

$$\frac{\partial \Psi}{\partial \lambda} = -\frac{2\pi \cdot a}{g} \cdot \omega'$$

Therefore, if Ψ is increasing towards the East, ω' is negative and it corresponds to relative ascending motion and reversely, if Ψ is decreasing towards the East, ω' is positive and it corresponds to relative subsidence.

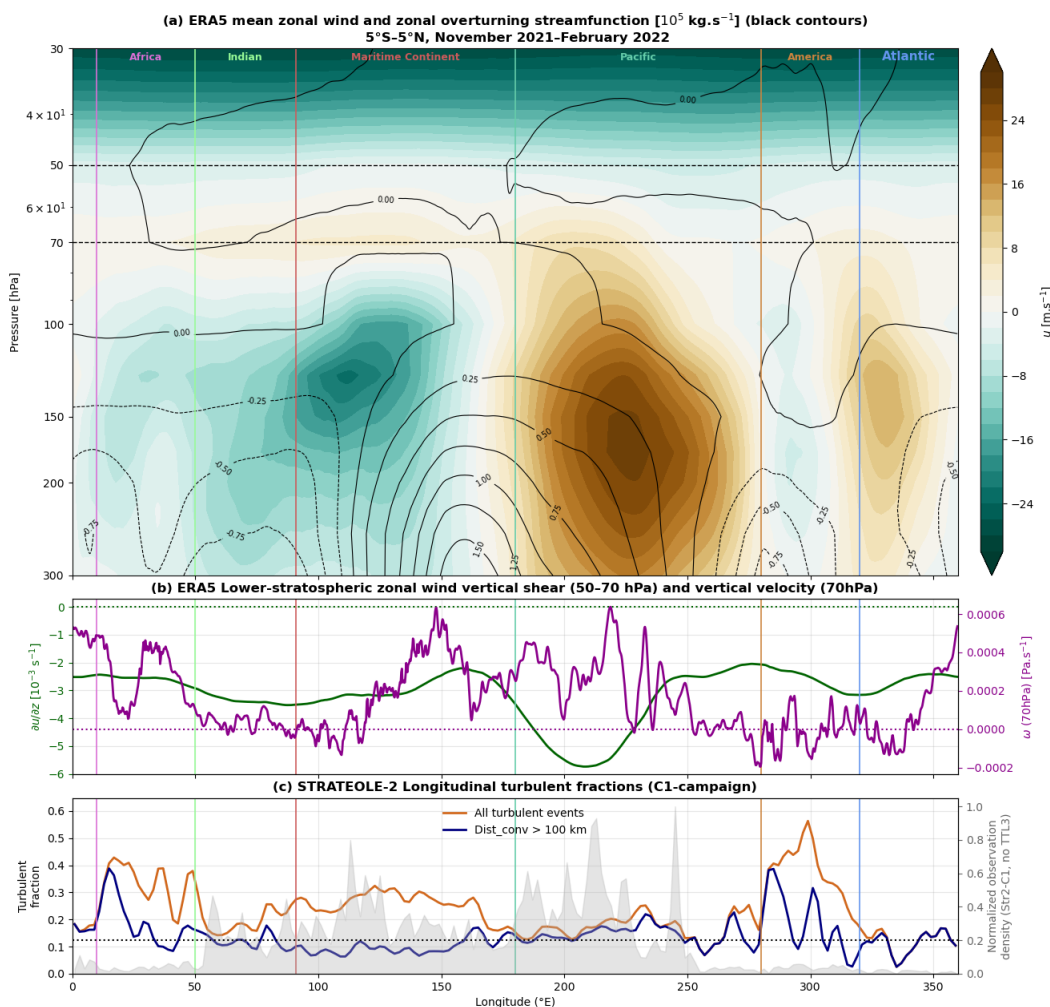
The second panel shows the 50-70hPa zonal-wind shear as a function of longitude (green), together with the mean vertical velocity ω' at 70hPa (purple). Positive values of ω' correspond to subsidence, whereas negative values indicate ascending motion. The 50-70hPa shear is predominantly negative and reaches its largest magnitude over the western and central Pacific.

The third panel shows longitudinal turbulent fractions from Strateole-2 measurements (orange) and their proportion associated to event located farther than 100km from deep convection (navy). The gray histogram shows the relative density of valid observations as a function of longitude, using 2° -wide bins. The number of valid Ri estimates in each bin is normalized by the maximum number of valid Ri estimates among all bins. Therefore, a value of 1 indicates the most

densely sampled longitude bin, while a value of 0.1 indicates a sampling density equal to 10% of the maximum.

The ERA5 cross section shows that the strongest overturning associated with the Walker-type circulation is confined to the upper troposphere (200–150 hPa), but it also shows that the longitudinal structure of the zonal wind extends into the TTL and lower stratosphere, and modulates the wind shear at 50–70 hPa over the Maritime Continent and western Pacific. The enhanced 50–70 hPa zonal wind shear occurs directly above this ascending branch, suggesting that it is therefore the upper-level imprint of the tropical large-scale circulation.

We also notice that we obtain larger turbulent fractions associated with events far from the convection (navy curve) over the maximum of shear in Pacific region than over the maritime continent. It supports our interpretation that the lower-stratospheric shear associated with the Walker circulation contributes to the observed longitudinal modulation of turbulence.



Minor comments:

line 5: I'd move the effective resolution "(~110 s)" to after "high-resolution" to make sure it's not misunderstood as referring to the length of the time series

line 113: I suggest to provide your estimates for the distance from convection for the two example situations shown in Fig. 3

It has been done.

line 132: please provide a comment as to why you didn't use the GPS altitude to compute the vertical displacements (I assume it's not sufficiently accurate?);

Indeed, the pressure data is more accurate, we have added a comment about this in the paper.

also: is the hydrostatic assumption, which would not be needed for the GPS altitudes, sufficiently valid in turbulent regions?

We explored this issue during the analysis phase by comparing the observed pressure variations with those expected from hydrostatic balance, following an approach similar to that suggested by Petr Šácha (2025). The objective was to identify non-hydrostatic departures that could potentially be associated with turbulent events. However, the inferred residuals were dominated by measurement noise, and no robust non-hydrostatic signal could be identified.

This is consistent with the expected magnitude of non-hydrostatic effects in the lower stratosphere. The non-hydrostatic correction is proportional to the vertical acceleration (Dw/Dt), which is expected to remain much smaller than g even in the turbulent regions sampled by Strateole-2. For the gravity-wave and turbulent fluctuations observed by Strateole-2, typical vertical velocities are of the order of a few cm/s to a few tens of cm/s, and characteristic time scales are several minutes. This gives vertical accelerations on the order of $(10^{-3})-(10^{-2}) \text{ m.s}^{-2}$, i.e. less than about $10^{-3}g$. Therefore, non-hydrostatic pressure perturbations are expected to be very small compared with the hydrostatic pressure gradient.

Furthermore, the comparison between the pressure-altitude-based and GPS-altitude-based gradient estimates shows very similar results, suggesting that the hydrostatic assumption does not significantly affect the Richardson number estimates or the inferred turbulent fractions.

line 151: I suggest to move this comment about what is shown in Fig. 4 to its caption

line 195: this is a common problem, also for radiosonde measurements, for which correction methods exist - please comment if any correction of this known bias was applied to the measurements here

The temperature measurements used in this study are the processed TSEN Level-2 observations, for which the correction for solar radiative heating is already applied. We therefore did not apply an additional correction to the temperature measurements. Note that, because we are interested in temperature differences over periods of 110 s, the corrections have here little incidence. The remaining daytime uncertainty is nevertheless larger than at night, as documented for the TSEN measurements (Wilson et al., 2023).

The localized temperature perturbations that can occur when the sensor passes through the warm wake of components of the flight chain are more difficult to correct for and are not explicitly corrected in the present data set. We have therefore clarified this point in the manuscript and acknowledge that such residual perturbations may contribute to the uncertainty of temperature-derived quantities, particularly during daytime.

As an additional check, we compared the turbulence statistics obtained from daytime and nighttime observations. The main geographical patterns remain consistent between the two subsets, suggesting that the larger daytime temperature uncertainty does not dominate the reported regional differences.

line 252: please provide those different latitudes for reference

It correspond to 43°N (RASCIBA Project, South of France). We have added it in the paper.

line 308: I think you need to provide justification for the choice of $\Delta t = 3h$ (also relevant for section 4.3.1)

For the turbulent density, the 3-hour segment length was chosen as a compromise between statistical robustness and temporal resolution. A 3-hour window generally contains a sufficient number of valid Richardson-number estimates to provide a meaningful estimate of turbulent density, while remaining short enough to capture changes in turbulence occurrence associated with variations in the surrounding convective environment. Significantly shorter windows would increase sampling noise, whereas substantially longer windows would smooth the temporal variability of the dynamical environment encountered by the balloon.

The kinetic energy was estimated over 3-hour segments as a compromise between spectral resolution and statistical robustness. A 3-hour window contains 360 samples at the 30 s sampling interval, which provides a sufficiently stable estimate of the power spectral density

while allowing the integration of energy up to the upper limit of the selected frequency band (3 h). Shorter windows would substantially increase the variance of the spectral estimates and would not adequately resolve fluctuations near the 3-hour period. And on the other hand, significantly longer windows would reduce the assumption of local stationarity along the balloon trajectory by mixing different dynamical environments.

Then, in order to compare kinetic energy, distance to convection and turbulence occurrences, it was easier to have estimates of these three variables with the same temporal resolution.

line 337: this makes sense, but is this sufficient to claim that the obtained kinetic energy is really due to GWs? Could it include turbulent kinetic energy (which would be quite relevant for the results shown in Fig. 11)?

The wind fluctuations for periods between the BV period (~ 5 min) and several hours are widely interpreted as the signature of the gravity wave (gwW) field. The frequency power spectrum of this field, $S_{\{u,v\}}(\omega)$, is well described by a power law (see Fig.1b., Podglajen et al., 2020):

$$S_{u,v}(\omega) \propto \omega^{-2}$$

There is no evidence that small scale turbulence has any impact in this period interval (no spectral signature). So we interpret the half variance of these fluctuations in the period-interval 10-180 min as the kinetic energy of the GW field.

line 344: what does the detrending refer to, i.e. what trend is being removed?

We estimate the PSD on the anomalies on each segments ($x'_{seg} = x_{seg} - \text{mean}(x_{seg})$).

line 399: do you have evidence for your hypothesis that the Walker circulation reaches above 100hPa? Main convective outflow is between 200-150 hPa, so quite a bit lower (see also major comment 3)

We agree that the main convective outflow associated with deep tropical convection is generally located between about 200 and 150 hPa. Our intention was not to suggest that the core of the Walker circulation is located above 100 hPa.

Rather, we meant that the large-scale dynamical influence associated with the Walker circulation may extend into the upper TTL and lower stratosphere through vertically coherent circulation anomalies and wave-driven coupling. We have revised the text to avoid implying that the Walker circulation itself extends above 100 hPa, and instead refer to the possible influence of large-scale tropical circulation variability on the 100–70 hPa layer.

line 408/409: with typical ENSO time scale of 4-7 years there would be order of 10 La Nina's in 40 years, i.e., the "sixth strongest" would in fact not be particularly strong ... perhaps simply say "moderately strong" (which I think corresponds to the official NOAA distinction)

You're right, it has been corrected in the paper.

Fig. 13: what are the color contours in the "Zoom over the Pacific"?

The colored contours in the zoom over the Pacific ocean correspond to the squared shear (S^2), it is a zoom of the S^2 Hovmöller plot presented in Figure 14.

Appendix A: why do you need to approximate the relations (e.g., Eq. A7)? Couldn't you simply compute the full relation based on the data?

At the beginning, we approximated the wind gradients in order to have a simpler expression of the Ri relative uncertainty, but you are absolutely right, the full expression would be :

$$\frac{\sigma Ri}{Ri} = \frac{1}{|\Delta z|} \sqrt{2 \left(\frac{\beta_\theta}{N^2} \right)^2 \cdot \sigma_\theta^2 + 2 \left(1 + \frac{4}{S^4} \left[\left(\frac{\Delta u}{\Delta z} \right)^4 + \left(\frac{\Delta v}{\Delta z} \right)^4 \right] \right) \cdot \sigma_z^2 + \frac{8}{S^4} \left[\left(\frac{\Delta u}{\Delta z} \right)^2 \cdot \sigma_u^2 + \left(\frac{\Delta v}{\Delta z} \right)^2 \cdot \sigma_v^2 \right]}$$

However, this expression only holds if the uncertainty in N^2 and S^2 are assumed to be independent, which is not the case since we use the same Δz for both estimates. Thank you very much for your comment. We redid the calculations considering the following expression of Bulk Ri, insted of the ratio of N^2 and S^2 : $Ri = \beta_\theta \Delta \theta \Delta z / (\Delta u^2 + \Delta v^2)$, and the propagation of uncertainties yielded the following expression :

$$\frac{\sigma Ri}{Ri} = \frac{1}{|\Delta z|} \sqrt{2 \left(\frac{\beta_\theta}{N^2} \right)^2 \cdot \sigma_\theta^2 + 2\sigma_z^2 + \frac{8}{S^4} \left[\left(\frac{\Delta u}{\Delta z} \right)^2 \cdot \sigma_u^2 + \left(\frac{\Delta v}{\Delta z} \right)^2 \cdot \sigma_v^2 \right]}$$

The Ri uncertainty can be rewritten as follows, if we consider $\sigma_u = \sigma_v = \sigma_{u,v}$ (Wilson, 2023):

$$\delta Ri \approx \frac{|Ri| \sqrt{2}}{|\Delta z|} \sqrt{\left(\frac{\beta_\theta}{N^2} \right)^2 \sigma_\theta^2 + \sigma_z^2 + \frac{4}{S^2} \sigma_{u,v}^2}$$

It gives:

$$\delta Ri \approx \frac{\sqrt{2}}{S^2 \cdot |\Delta z|} \sqrt{(\beta_\theta)^2 \sigma_\theta^2 + N^4 \cdot \sigma_z^2 + 4 Ri \cdot N^2 \cdot \sigma_{u,v}^2}$$

We can notice that the Ri uncertainty becomes very significant if Δz or S^2 tends to zero, and also when Ri takes on high values. But this third case is not relevant for our study because whether the real Ri value is 10 or 100 has no effect on the turbulent fraction estimate.

Appendix B: why only show T^+ , u^+ , v^+ in Fig. B1 (i.e. why not also include their antisymmetric parts)?

We added a new figure with the antisymmetric parts in the paper.