Review of "Turbulence over the Atlantic ITCZ – Is the upper troposphere above doldrums less turbulent?" by A. Dörnbrack, A. Giez, C. Mallaun, and C. C. Stephan (egusphere-2026-2628)

General comments

The manuscript uses aircraft measurements from a recent field campaign (PERCUSION) in the region of the Atlantic ITCZ to characterise the turbulence observed at flight altitudes of mostly 13–15 km. The key hypothesis tested is that the doldrums — regions of low surface wind speed in the deep tropics — are associated with decreased upper-tropospheric turbulence. The authors show some indication that this is the case, but also note that the relationship appears necessary rather than sufficient: turbulence is reduced when surface winds are weak, but reduced turbulence (or column-integrated divergence within the ITCZ) does not imply weak surface winds. In addition, the paper characterises the turbulence during the campaign more generally and assesses the suitability of 10 Hz vs. 100 Hz measurements for turbulence analysis, drawing on two other aircraft campaigns (EUREC4A and SOUTHTRAC).

This is an interesting and, as far as I am aware, novel research question, and the data set (183 constant-altitude legs, 89 circles) is a valuable resource. My main concerns — detailed in the major comments below — relate to the scope and structure of the manuscript, to the choice of data sets used to characterise the atmospheric conditions, and to the statistical support for and interpretation of the key result. Overall, I think the paper has the makings of a solid contribution once the scope is sharpened and the analysis choices are better justified and more consistently applied. Major comments are listed below, followed by minor comments organised by section.

Major comments

1. **Two papers in one.** The title, abstract, and first half of the introduction motivate the doldrums/turbulence question, while a second research strand — the characterisation of turbulence during PERCUSION together with the comparison against SOUTHTRAC and EUREC4A and the assessment of 10 Hz vs. 100 Hz measurements (based mostly on these two other campaigns) — is only loosely connected to it. The connection between the two strands is never made explicit, which makes the paper difficult to follow and obscures what the central result is. Please clarify the relationship between the two aims, or separate them: decide which story is the main one, restructure the manuscript around it, and either shorten or move to the supplement the material supporting the secondary story. If the cross-campaign comparison remains, it should be reflected in the title and abstract and carried through consistently (see the comment on Section 3 below, where PERCUSION itself is not actually compared against the other two campaigns).
2. **Choice of data set for divergence/convergence.** The doldrums-related analysis is based on the circle legs, yet the divergence/convergence used to characterise the

doldrums (VIMAD) is computed from ERA5 rather than from the observed circle products derived from the dropsonde measurements (e.g. via the regression method; see the BEACH data set). Since the aircraft observations are the whole basis of the paper, using the measured profiles of convergence and divergence seems the more natural — and more convincing — choice. Related to this, ERA5 vertical velocity and divergence are not generally considered reliable in this region; please either justify their use or discuss this limitation explicitly.

3. **Underuse of the straight-leg data.** Related to the above, it is unclear why only the circle legs are used to address the central question rather than all 183 constant-altitude legs. If the circle products are not used — though that would be the natural choice — the analysis could be extended to all legs, since mean surface conditions in the vicinity of the flight track can equally be inferred from ERA5 (e.g. by identifying a surrounding region and evaluating whether it satisfies the doldrums criterion). This would increase the sample size behind the paper's key result.

4. **Dissipation-rate/spectral analysis (Sects. 2.2, 3, 4).** A large fraction of the paper is devoted to the energy dissipation rate, the TKE–ε relationship, and the 10 Hz vs. 100 Hz comparison, but it is not clear how — or whether — this material is used to address the central hypothesis about doldrums and turbulence. If it is not needed for the main result, I suggest moving most of it to the supplement, retaining only what is strictly necessary for interpreting the doldrums analysis.

5. **Statistical support for the key result.** The claim that TKE_{max} is reduced above the doldrums (Fig. 15) rests on a purely visual/qualitative comparison. It would benefit from a more formal statistical treatment — e.g. a test of whether the low TKE_{max} values associated with doldrums conditions are unlikely to be drawn from the same distribution as those associated with $|u_{10m}| > 3$ m s^{-1}.

6. **Interpretation of the main finding.** One of the more interesting outcomes is that the original hypothesis — reduced turbulence due to a thermally more stable upper troposphere — is not supported (N is in fact lowest over the doldrums), and that the reduction instead appears related to weaker upper-tropospheric shear. This is a nice result, but it is under-discussed. The conclusions speculate that the reduced shear might itself be linked to reduced convective activity; could this be tested directly, for example with an analysis analogous to that of Fig. 11?

Minor comments

Title/Abstract

– The title poses a single, specific question, but the abstract (and later the paper) covers noticeably more ground (see Major comment 1).

– It would help to state the underlying hypothesis briefly in the abstract — i.e. why subsidence over the doldrums might be expected to reduce turbulence; the explanation currently appears only in the main text.

– It is not clear from the abstract why the doldrums analysis is restricted to the circle legs rather than making use of all 183 constant-altitude legs (see Major comment 3).

Section 1 (Introduction)

– The introduction falls into two parts that are not well connected: the first on the doldrums, the second largely a literature survey of turbulence measurements from various campaigns and altitudes.

– The second part is long and describes a number of individual studies in detail without always making clear why each is relevant to the present question. I recommend condensing it considerably and focusing on what is needed to motivate the analysis that follows.

– EUREC4A is mentioned only once in the introduction, yet later becomes a central point of comparison (Sect. 3).

Section 2.1

– Lines 140–142 already state, citing Appendix B of Dörnbrack et al. (2022), that the 10 Hz and 100 Hz spectra are very similar within the 0.4–4 Hz analysis interval used to determine ε. Given this, the added value of the full 10 Hz vs. 100 Hz comparison in Sect. 3 is unclear; this supports moving that material to the supplement (see Major comment 4 and the comments on Sect. 3 below).

Section 2.2

– It is not clear why the energy dissipation rate is needed for the paper's central (doldrums) question. Several length scales are introduced in connection with the theoretical TKE–ε relationship (integral, Kolmogorov, Ozmidov, buoyancy), but it is not clear how they are subsequently used or compared.

– The distinction drawn between TKE and dissipation rate as having "slightly different meanings" (l. 189 ff.) could be clarified further — do the authors mean that one integrates over the full spectrum while the other reflects only part of it?

Section 2.3

– Please clarify why only one of the data sets collected during PERCUSION is used, given that the circle products derived from the dropsondes would also provide the relevant information (see Major comment 2).

– Column water vapour (CWV), rather than total column water (TCW), is the more standard quantity for defining the ITCZ. Please clarify the choice and its consequences.

– If ERA5 is used for these diagnostics, how is the associated uncertainty accounted for?

Section 3 (EUREC4A)

– Since this section concerns a different campaign and motivates a data-quality point (validity of the 10 Hz data) rather than a result of the present study, I suggest moving it to the supplement (up to around l. 270).

– Two points if it is retained: (a) how might the different meteorological conditions sampled by PERCUSION (inside the ITCZ) vs. EUREC4A (outside) affect this assessment? (b) Can the agreement between ε at 10 Hz and 100 Hz be quantified (e.g. with a correlation coefficient) rather than left to visual comparison? I would have expected the flattening of the spectral slope to appear as a systematic bias — is that the case?

– From around l. 270 onwards, PERCUSION itself is not compared against the other two campaigns, even though characterising the PERCUSION turbulence is one of the paper's stated objectives. The purpose of the EUREC4A–SOUTHTRAC comparison at this point is unclear; please clarify or reconsider whether it is needed in the main text.

Section 4 (PERCUSION)

– The opening paragraph largely repeats the campaign description given earlier; consider condensing.

– The discussion of the ERA5 cold-point tropopause (l. 314 ff.) reads as a data-set description and would fit better in the data section than in the results.

– Line 341: what is the physical reason for the flattening of the spectra at higher frequencies?

– Figure 8: the many overlapping lines make the individual contributions hard to distinguish.

Section 5 (Doldrums and upper-level turbulence)

– The number of doldrums circles is stated inconsistently: Fig. 12 and footnote 3 refer to 15 circles satisfying the strict criterion, yet only 13 circles have wind speeds less than 3m/s in Fig. 13.

– Please clarify what is meant by the "northern edge of the ITCZ".

– Figure 12 is missing a colour bar. The figure is interesting, but its connection to the paper's central question about upper-level turbulence could be made more explicit.

– The comparison with Klocke et al. (2017) is not fully convincing to me: since a vanishing meridional wind is itself a defining criterion for the ITCZ centre in their method, v_{10m} being close to zero there follows almost by construction — and it is referred to as the ITCZ centre there, rather than its northern edge. Please reconcile this with the framing used here.

– Line 408: could this be shown explicitly by restricting the Fig. 12 analysis to the western part of the domain?

Section 6 (Summary and conclusions)

– The conclusions reflect the paper's dual focus noted above (Major comment 1) — the text moves between the doldrums question and the broader turbulence-characterisation/campaign-comparison material without always making the connection clear. I suggest revising this section once the overall scope of the paper has been settled.