Reply to Reviewer #2 (Author's response is in red font)

Comments to the paper "Turbulence over the Atlantic ITCZ - Is the upper troposphere above doldrums less turbulent? By Dornbrack et al.

This study investigates atmospheric turbulence over the Atlantic ITCZ using high-frequency airborne measurements collected by the German research aircraft HALO during the PERCUSION campaign in summer 2024. Combined with meteorological data from the ECMWF IFS, the manuscript aims to address the following two questions: (i) Are doldrums associated with a thermally stabilized, less turbulent upper troposphere? and (ii) How much does the turbulence observed in the tropical upper troposphere during PERCUSION differ from measurements taken by the same airborne platform in the lower stratosphere during SOUTHTRAC (Rapp et al. 2021)? Although the authors' thorough analysis and efforts are evident, the current manuscript lacks a sharp scientific focus, primarily because it attempts to address two disparate subjects that do not necessarily fit well together. Therefore, I cannot accept the current manuscript for a full scientific review.

We agree that the original balance between the two aspects of the paper was unclear, and we have restructured the text accordingly (see Reviewer 1, Major Comment 1): The hypothesis referring to the doldrums is now clearly the central contribution of the paper, while the general characterization of turbulence for PERCUSION and EUREC^{4}A has been reduced to a secondary role. Specifically, selected material from the EUREC^{4}A campaign has been moved to a Supplement. We hope that this decision is understandable and acceptable and addresses, at least in part, the reviewer's concerns regarding the structure of the article.

Regarding the first scientific question—which appears to be the main focus, as indicated by the title "*Turbulence over the Atlantic ITCZ-Is the upper troposphere above doldrums less tuabulence?*"—I am not fully convinced of its scientific merit. Given the diverse sources of atmospheric turbulence in the tropical upper troposphere—such as various instabilities (e.g., inertial, Kelvin-Helmholtz, and convective instabilities) associated with convective outflow (Kim et al. 2014; Lee et al. 2025) and the breaking of convective gravity waves (Lane et al. 2003; Kim et al. 2019)—attempting to derive a simple correlation between surface wind speed and upper-tropospheric turbulence based solely on convergence/divergence patterns within the same horizontal area is physically problematic. When upper-tropospheric turbulence is driven by cloud outflow and convective gravity waves, it can manifest far from the primary convective source region. Furthermore, as the authors themselves note, the findings on this topic remain "*preliminary and need further validation for robust conclusions*."

This is the most significant outstanding scientific issue. We have not included a new analysis that directly isolates the doldrums signal from confounding factors caused by remotely generated CIT/NCT (e.g., an analysis in which the relationship between doldrums and turbulence is made dependent on the distance to the nearest convective core, analogous to our own Fig. 6). Instead, our response is based on two reinforced lines of evidence:

(1) It has now been shown that the correlation between TKE_{MAX} and $|u_{10m}|$ is statistically significant (Mann-Whitney U test, $p < 0.001$, Reviewer 1, Major Comment 5) and is not merely a visual impression, and

(2) it is independently reproduced by in-situ dropsonde observations (BEACH dataset from) without relying on ECMWF's IFS at all (Reviewer 1, Major Comment 2).

Furthermore, we no longer describe the finding as merely "preliminary". The manuscript now explicitly includes the caveat that the relationship is "not exclusive," rather than asserting a clear causal relationship, which, in our opinion, represents the scientifically honest attitude given the reviewer's valid objection regarding long-range effects. In addition, we are convinced that high-resolution numerical simulations would provide a far more suitable basis for investigating this question; however, they are beyond the scope of this article.

Therefore, I strongly suggest narrowing the scope of the manuscript by focusing exclusively on the second scientific question. Removing the first question would significantly enhance the clarity and impact of the paper. For the second question, the characteristics of the turbulence observed during the PERCUSION campaign are well presented, and the comparison with the SOUTHTRAC dataset is highly reasonable. Once the manuscript is restructured to focus on this single compelling topic, I will review the scientific content in greater detail.

We have carefully reviewed this recommendation but have decided against removing the doldrums question, as we believe it represents the more innovative and significant contribution of this work — namely, a further examination of Windmiller's (2024) hypothesis and its relationship to new datasets (ECMWF IFS, BAHAMAS) by using an independent methodology. However, we have addressed the reviewer's underlying concern in detail: In addition to sharpening the manuscript's focus and strengthening the evidence for the doldrums hypothesis (statistical test, independent BEACH cross-validation), we have now completely removed the more detailed, purely descriptive quantitative characterization of turbulence — the theoretical TKE–ε relationship, its slope regressions for both $EUREC^4A$ and PERCUSION, and the associated length-scale analysis — from the manuscript. We plan to present this material in a separate manuscript specifically intended for that purpose. We believe that this directly addresses the reviewer's request for a more concise article focused on a single main topic, without sacrificing the findings on the doldrums, which we consider premature to omit given their novelty.

References

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