Response to Reviewers #1 Comments

We thank the associate editor, editor and two anonymous reviewers for their thoughtful and exhaustive comments and suggestions, which significantly help us to improve the quality of the manuscript. In this revised manuscript, we have revised the manuscript accordingly. Below, we indicate the original comment of the respective reviewer in blue and our point-to-point response is denoted in black.

Before addressing the comments, we would like to express our sincere gratitude to the reviewers for their exceptionally informative, constructive, and detailed comments.

Reviewer #1 Evaluations:

The study analyses the occurrence frequency of subcritical Richardson numbers, in a comprehensive near-global multi-year HVRRS radiosonde data set, as well as in the co-located vertical profiles in the ECWMF ERA5 reanalysis. Based on several comparability criteria like the seasonality, vertical distribution, and correlation coefficients in different climate zones, an adjusted Richardson number threshold of Rit=1 is identified which exhibits characteristics most suited to diagnose regions in the ERA5 data which might be subjected to the occurrence of Kelvin Helmholtz instability. I have two main concerns (see main comments below), one is more of a criticism on the wording, but it concerns the interpretation of the results at several points, and if the authors agree with the criticism, the wording and some aspects of the discussion throughout the manuscript have to be reworked.

My second main concern is that the interpretation of correlations and correlation coefficients in some of the analysis sections are too strong, which might therefore either need some reworking, a more detailed and broader description of the results leaving room for interpretation, or a reconsideration if these results are suited for publication.

Despite these main concerns, I found the results on the capability of the ERA5 to resolve subcritical Richardson numbers in comparison with the HVRRS interesting, and I think these along with the climatologies as key results could be suited for publication, however, only after consideration of the comments below.

I think the sections on the orographic and dynamical environment of regions of small Richardson numbers need clarification, mainly but not only in the context of the two main comments.

Response: We sincerely appreciate the reviewers for their thorough and insightful comments and assessments, which have greatly benefited our present work as well as our future research. We learned a lot from your suggestive comments. In the following,

we address your comments in detail, and we have revised some parts of our work based on your constructive suggestions, aiming to achieve your desired outcomes.

Main comments:

01)

I am uncertain to what amount the identification of vertical segments with Ri<Rit (with Rit=1/4 in HVRRS and/or Rit=1 in ERA5) is equatable with the identification of KHI.

Response: Thanks for the comments. In the updated file, we have stated this conclusion in the Conclusion and Remarks session:

"…In other words, under a similar occurrence frequency, the identification of vertical segments with $Ri<1$ in ERA5 is equitable with identification of vertical segments with $Ri<1/4$ using HVRRS…"

1. Subcritical Richardson numbers are a necessary but not sufficient criterion for the occurrence of dynamic instability and KHI.

Response: We agree. In the Introduction section of the original manuscript, we also stated that Ri is not a good guide to instability character in general, and Ri>1/4 does not assure flow stability for superpositions of mean and GW motions. Despite these caveats, Ri<1/4 does provide a reasonable guide to expected local KHI structure in cases where clear KH billows arise.

In addition, in the revised manuscript, we principally emphasize the occurrence frequency of low Richardson number based on your professional and detailed comments.

2. Negative values for N² indicate convective instability. If the data was not filtered, I would expect convection to contribute significantly to the statistics of subcritical Richardson numbers.

Response: We totally agree with your assessment. $N^2<0$ was not totally filtered out in our analysis, largely due to the fact that a moving average with a bin of 200-m cannot totally avoid the instantaneous convection.

In the revised version, we have separately estimated the occurrence frequency of $0<Ri<Rit$ and $Ri<Rit$ using radiosonde and ERA5, as illustrated in Figure 8. The percentage of $Ri<0$ relative to $Ri<Rit$ is generally less than 20% in the middle and upper troposphere but it is as high as around 40% in the boundary layer.

In addition, in the supporting information, we have added a new figure as Figure S5 to illustrate the occurrence frequency of $0<Ri<Rit$, which exhibit a similar spatial variability as that of $Ri<Rit$.
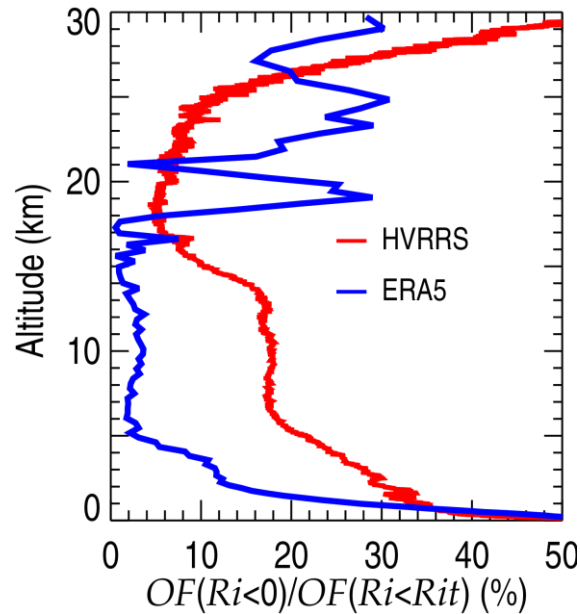
Figure 8. The percentage of *OF(Ri<0)* relative to *OF(Ri<Rit)* in HVRRS (red) and ERA5 reanalysis (blue).
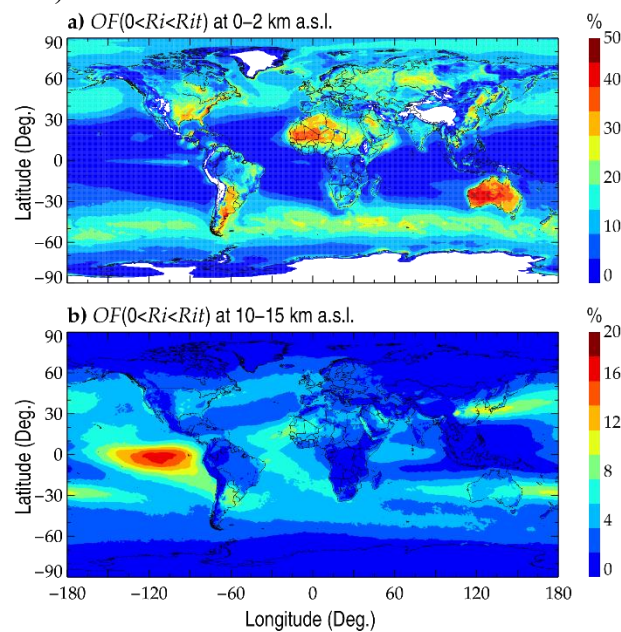

Figure S5. The spatial distribution of the mean *OF(0<Ri<Rit)* in ERA5 reanalysis at 0–2 km a.s.l. (a) and 10–15 km a.s.l. (b). Note that *Rit* is set to 1.

o **HVVRS:**
  ■ In Fig. 15, the secondary occurrence frequency maximum OF(Ri<1/4) below vertical wind shears of 10 m/s/km could be indicative of such convective contributions. But this is hard to say for sure because all values are vertical averages.

Response: We agree with your assessment. Therefore, we also recalculated the joint distribution of HVRRS-derived wind speed, wind shear, and *OF(0<Ri<Rit)*, and it has

been shown in the supporting information as Figure S8. Compared to Figure 15 in the main text, Figure S8 exhibits a very similar distribution as Figure 15.
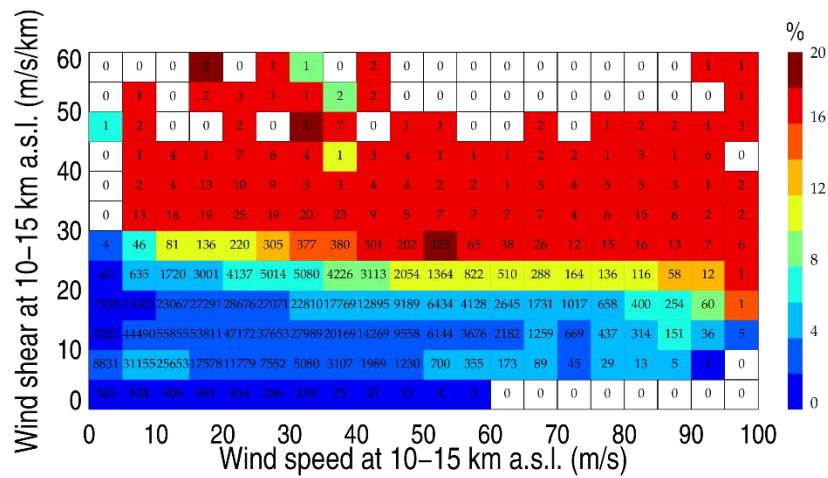

Figure S8. Joint distribution of HVRRS-derived wind speed, wind shear, and $OF(0<Ri<Rit)$, with a bin size of 5 m/s along the x axis and 5 m/s/km along the y axis. Note that all the relationship is based on the mean result of individual profiles at heights of 10–15 km a.s.l.. The number indicates the matched profile number in each grid.

- ■ At p.11 L.304 you include convection in the PBL in the interpretation

Response: Yes. As stated above, we recalculated the occurrence frequency of $0<Ri<Rit$ to partly exclude the influence from convection. While the spatial similarity between boundary layer height and $OF(0<Ri<Rit)$ draw a similar conclusion as that of boundary layer height and $OF(Ri<Rit)$

- o **ERA5:** Negative static stability in the model is (as I understand it) quickly resolved through the cloud parametrisation, and therefore rare in the model output. Still, regions of low static stability ($\rightarrow$ small Ri) could be indicative of regions prone to convection, particularly for the adjusted threshold of Rit=1.

Response: Thanks for the informative comments. The initial motivation of the present analysis was that we noticed that more and more studies use ERA5 reanalysis to evaluate turbulence indices in recent years. However, the performance of ERA5 in calculating wind shears and Richardson number was not globally evaluated at present. For $0<Ri<1$, it would be quite difficult for us to differentiate the convective instability or dynamical instability when using ERA5 reanalysis. However, the present analysis could provide a possible reference for further related studies (using ERA5 to calculate wind shears and Richardson number, or low Ri climatology).

- ■ At p.13 L.361 you mention both KHI and convection in the PBL (diurnal cycle?).

Response: In the revised version, we have removed this phrase. More precisely, both $OF(Ri<Rit)$ and $OF(0<Ri<Rit)$ keeps high consistency with that of planetary boundary layer height (PBLH) over oceans, such as the Pacific Ocean and the Atlantic Ocean. The continental $OF(Ri<Rit)$ in the PBL region could be considerably affected by the vertical resolution of ERA5 reanalysis, which sharply decrease from around 20 m at 0 km a.s.l. to around 120-m at around 2 km a.s.l.

Therefore, two related sentences have been added in the revised version:
"Interestingly, the spatial variation in $OF(Ri<Rit)$ ensembled by years 2017 to 2022 keeps high consistency with that of planetary boundary layer height (PBLH) over oceans, such as the Pacific Ocean near Japan and the Atlantic Ocean near U.S., as shown in Figure S4"
"However, the vertical resolution of ERA5 in the PBL decreases sharply, leading to the fact that the resolution of the PBL data over the region with high elevations can be significantly lower than that of regions with low elevations, which could bring great challenges to the analysis of the impact of topography on low-level $OF(Ri<Rit)$."

- **At p.15 L.418 you write the OF(KHI) maximum is related to convective heating.**

Response: According to Figure S5b in the supporting information of the updated version, $OF(0<Ri<Rit)$ also exhibits an obvious enhancement over the Niño 3 region. Based on the clues shown in Figure S5b, Figure 9b, and Figure 2c, we argue that the $OF(Ri<Rit)$ anomaly could likely be attributed to the ENSO-related tropical convective heating in the upper troposphere, leading to a low Brunt-Väisälä frequency.

I realize that Fritts et al. (2014) (who you cite in the introduction) discuss how the distinction between dynamic and convective instability can be "challenging an misleading", but I am not sure how this discussion for Lidar measurements in the MLT region is applicable for the troposphere.
Response: Thank. In the updated file, we have specified the conclusion of Fritts et al. (2014) to the MLT region.

p.15 L.422 Would you describe convection in the troposphere generally as weak?
I would suggest to replace OF(KHI) in the manuscript by OF(Ri<Rit), and either expand the discussion and/or leave it to the readers interpretation which process dominates in each region and season. See also the more specific comments further below.
Response: Thanks for the suggestion. We totally agree with your comment. In the updated file, we have replaced all OF(KHI)s with $OF(Ri<Rit)$s throughout all texts and all figures/tables.

Response: As suggested, Figure 12 in the updated manuscript (Figure 11 in the old file) has been modified as density histograms. Indeed, the highest data density is clustered at low SDORs. Statistically, the low-level $OF(Ri<Rit)$ is significantly correlated with $OF(Ri<Rit)$ based on hundreds of thousands samples. Complex terrain could locally enhance $OF(Ri<Rit)$ by increasing wind shear or mountain waves. Therefore, in the revised version, we stated that complex terrain may locally enhance $OF(Ri<Rit)$.
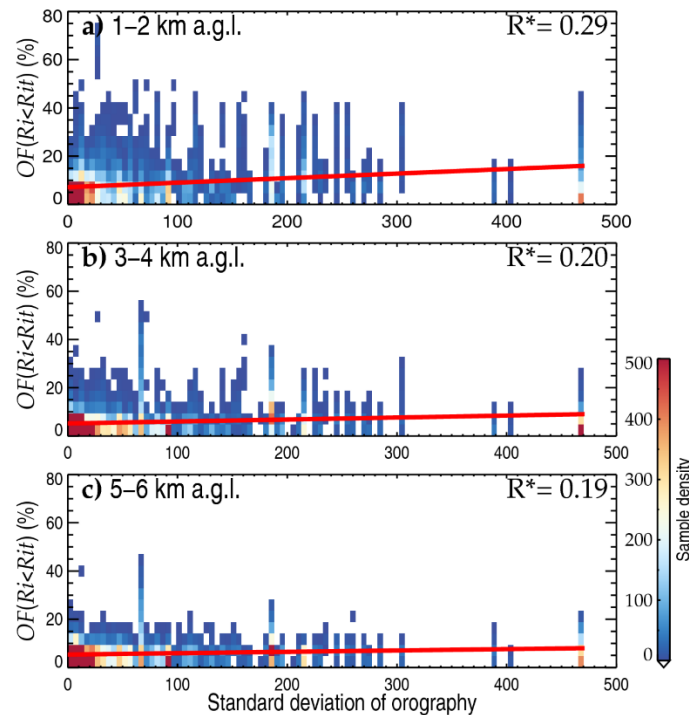
Figure 12. The association of HVRRS-determined $OF(Ri<Rit)$ with different standard deviations of orography (dimensionless). (a), (b), and (c) are for height ranges of 1–2 km, 3–4 km, and 5–6 km a.g.l., respectively. The correlation coefficients between

OF(Ri<Rit) and standard derivation of orography are marked in the top right corner, where the star superscripts indicate that values are statistically significant (*p*<0.05).

> Furthermore, could you point out which regions in Fig. 8 you mean with the first sentence at p.14 L.399?

Response: According to Fig.9a (Fig.8a in the old file), it is notable that low-level continental *OF(Ri<Rit)* is dependent on underlying terrains. However, the vertical resolution of ERA5 in the PBL decreases sharply, leading to the fact that the resolution of the PBL data over the region with high elevations can be significantly lower than that of regions with low elevations. It would be inappropriate to use ERA5 to investigate the possible influence of topographies on low-level *OF(Ri<Rit)*. Therefore, we used radiosonde to investigate the association between low-level *OF(Ri<Rit)* and underlying terrains.

The above concern has been incorporated in the updated text.

> 2. The paragraph beginning at p.15 L.425: Again, I understand the approach of the analysis and the idea behind it, but I would not agree that the conclusion is justified. The correlation is very weak. It is likely dominated by the point cloud at highest data density at low nearsurface-winds. I would not agree that this supports the interpretation at the end of the paragraph.

Response: Thanks for the suggestive comment. We agree with you and totally remove this part in the updated file.

> 3. For comparison: At p.17. L.473 you write: "[The ERA5 determined occurrence frequency of Ri<1/4] is poorly correlated with HVRRS-determined ones at all heights and over all climate zones."
> If this refers to Fig. 7, it might be too strong of a statement considering the overall interpretation of correlation coefficients in the manuscript.

Response: Thanks. This phrase has been modified to be:
"…In addition, it is weak correlated with HVRRS-determined ones at most heights and over most climate zones…"

Other scientific comments:
ABSTRACT
p.2 L.32 I would prefer to write "KHI is indicated by the critical value of dimensionless Richardson (Ri) number, which is predicted to be ¼ from linear stability analysis." or something along these lines.
Otherwise a reader without background knowledge might perceive the critical Richardson number as a purely empirical threshold.

Response: Thanks. The suggestion has been incorporated in the Abstract session.

p.2 L34 and L.36 I would suggest to delete the brackets with "137 level" and "10 m", since these values are not directly comparable.

Response: Amended as suggested.

p.2 L.45 In the context of major comment 01, I would suggest to replace "The occurrence frequency of KHI..." with "The occurrence frequency of subcritical Richardson numbers.."

Response: Done as suggested. The replacement has been completed throughout all texts.

p.2 L45 From Fig. 9d and 10k I would argue that there is also a seasonality in the tropics. I see your point that it might overall be less pronounced, but I still would prefer to not exclude the tropics specifically. There are large scale flow features like the Summer Asian Monsoon and the tropical easterly jet or the variability of the Walker circulation (also in relation to ENSO) which could be associated with the tropical seasonality in subcritical Richardson numbers (Roja Raman et al. (2009), Sunilkumar et al. (2015), Kaluza et al. (2021)).

Response: Thanks for the informative suggestion. In the revised file, we stated that seasonal cycles can be detected over all climate zones throughout the text.

The suggestion about the tropical seasonality has been included in Section 3.3, many thanks!

INTRODUCTION

p.3 L.70 Baumgarten and Fritts (2014) focus on the MLT region, and although the statement (or parts of it) might be applicable to the region analyzed here, maybe there is a more general source in literature which describes the environment in which KHI arises.

Response: Based on the suggestion, the statement has been rephrased as:

"…KHI arises preferentially from micro- and mesoscale wind shear intensification, with maximal occurrence frequency near synoptic scale upper-level frontal zones near jet streams, with mountain waves, and above the tops of severe thunderstorms (North et al., 2014)..."

p.3 L.73 Just to clarify, with "the associated large gradient in jet stream" you mean the wind gradients? Or stability gradients? Maybe you could rephrase it to make it more clear.

Response: The gradient has been referred to wind gradients. Moreover, the statement has been modified as:

"…Large wind shear is commonly associated with regions where stability changes rapidly (e.g., near the top of the boundary layer) and the large wind gradient in jet stream (Grasmick and Geerts, 2020)…"

p.3 L.74 "which may increase clear-air turbulence (Williams and Joshi, 2013)."
Do you mean it could increase in a changing climate? If this is the case, maybe rephrase it in an individual sentence.
Response: The climate change could considerably alter wind shears over some regions. Therefore, the statement has been rephrased as:
"…In a changing climate, wind shear in the North Atlantic upper-level jet stream could be increased (Lee et al., 2019), which may increase clear-air turbulence at cruise altitudes…"

p.3 L.81 Maybe remove the sentence "Among others, KHI is one of the most common causes of turbulence throughout the atmosphere from Earth's surface to the lower thermosphere (Fritts et al., 2011; Sharman et al., 2012)." This is somewhat repeated from the sentence before.
Response: Amended.

p.4 L.89 "Values of Ri close to zero favor strong instability, deep billows, and relatively intense turbulence, whereas values of Ri closer to ¼ favor weak instability, shallow billows (Fritts et al., 2011)." I am not sure if this information is necessary for this study.
Response: In the updated file, we also discussed the percentage of $Ri<0$ relative to $Ri<Rit$, as shown in Figure 8. Therefore, we believe this information could be useful to approximate the possible variation of strong or weak turbulence in altitude.

p.4 L.91 "The threshold value of Ri can be potentially used an indicator of turbulence (for instance, Jaeger et al., 2007)."
Suggestion for clarification: "The Richardson number criterion can be applied as a turbulence diagnostic in numerical model output (e.g. Sharman and Pearson, 2017), and it has been used as such in climatological studies on the occurrence of clear air turbulence (Jaeger and Sprenger, 2007)."
Since you cite Jaeger et al. (2007), a more direct comparison with your results in the later sections could be interesting, since Jaeger et al. also presents a Richardson number climatology with an adjusted Rit threshold. And it would put your results in context with the literature.
Kunkel et al. (2019) include a brief discussion on the capability of ECMWF models to resolve subcritical Richardson numbers, and argue as well that Rit=1 might be a good proxy for observed KHI (although only in a case study).

A very recent study on this subject is by Lee at al. (2023), in their climatology on UTLS turbulence diagnostics. They also set the Richardson number threshold from 0-1.

Response: Thanks for the exhaustive suggestion. The above comments have been incorporated into the new file. The related part of text has been modified as:

"…The Richardson number criterion can be applied as a turbulence diagnostic in numerical model output (e.g. Sharman and Pearson, 2017), and it has been used as such in climatological studies on the occurrence of clear air turbulence (Jaeger and Sprenger, 2007). Kunkel et al. (2019) includes a brief discussion on the capability of ECMWF models based on case studies to resolve subcritical Richardson numbers, and argues that the threshold value of Ri (Ri_t) taken as 1 might be a good proxy for observed KHI. A very recent study by Lee at al. (2023) also sets Ri_t from 0–1 in their climatology on the upper troposphere and lower stratosphere turbulence diagnostics…"

p.4 L.101 "While in numerical models, turbulent dissipation rate, turbulent diffusivity and other parameters representing turbulent mixing efficiency are the most basic parameters, which need to be accurately parameterized to evaluate the impact of turbulent effect on matter and energy distribution (Gavrilov et al., 2005). For this reason, the indices of turbulence, such as large wind shear, small Ri, the negative squared Brunt-väisälä frequency, could be a great tool to characterize turbulence (Jaeger et al., 2007)."

I am not sure how the two sentences and the two citations fit together. Can you maybe rephrase the key statement you want to make here?

Response: This paragraph has been rephrased to be:

"…In numerical models, turbulent dissipation rate, turbulent diffusivity and other parameters representing turbulent mixing efficiency are the most basic parameters, which need to be accurately parameterized to evaluate the impact of turbulence effect on matter and energy distribution (Gavrilov et al., 2005). However, due to the intermittent nature of turbulence it is generally not resolved in (global) numerical weather prediction models, even at nowadays common/states of the art horizontal resolutions of the order of tens of kilometers (Sandu et al., 2019), and it presents a challenge both in observation and numerical modeling (Sharman et al., 2012; Homeyer et al., 2014; Plougonven and Zhang, 2014). For this reason, the indices of turbulence, such as large wind shear, small Ri and the negative squared Brunt-väisälä frequency, could be a great tool to characterize turbulence (Jaeger et al., 2007)..."

p.5 L.121 Is AEOLUS data assimilated in ERA5? At all, or consistently for the time period analyzed?

Response: According to Banyard et al (2021), Aeolus observations have not been assimilated into ERA5. Thus, we removed this statement in the revised version.

Reference:

Banyard, T. P., Wright, C. J., Hindley, N. P., Halloran, G., Krisch, I., Kaifler, B., & Hoffmann, L. (2021). Atmospheric gravity waves in Aeolus wind lidar observations. Geophysical Research Letters, 48, e2021GL092756. https://doi.org/10.1029/2021GL092756

p.5 L.134 Suggestion: "Thus, our objectives are to: (1) Evaluate the performance of ERA5 at different heights and climate zones in estimating wind shear and small Richardson number occurrence frequencies, in comparison with a large high-resolution radiosonde dataset spanning the years from 2017 to 2022."

Response: Done as suggested, thanks.

1.1 HIGH-RESOLUTION RADIOSONDE DATA SET

p.6 L.155 Are 115 million radiosonde profiles analyzed in this study? I was wondering because taking the upper limit of the color scale in Fig. S1 times the number of radiosonde stations 3000*434 equals about 1.3 million soundings. Maybe I am missing something.

Response: Very sorry we make a serious mistake. We totally misunderstood the word "million" in the context of English for a long time (We thought it means 10,000). This analysis went through several versions. At the beginning of the analysis, about 1.15 million radiosonde was adopted during years 2016-2022. And then, we removed all ERA5 data on 2016 due to the filled storage of our device (Since ERA5 137 model level data needs huge storage and computation resources). We rechecked our radiosonde dataset during years 2017-2022, and found that about 0.95 million radiosondes have been adopted.

The following Linux terminal displays the total count of radiosonde profiles during years 2017-2022.

```
jian@LAPTOP-2ECIDBLP:/mnt/d/RS_all_10m$ find . -name "*-201[7-9]*"  | wc -l
449531
jian@LAPTOP-2ECIDBLP:/mnt/d/RS_all_10m$ find . -name "*-202*"  | wc -l
495860
```

1.2 ERA5 REANALYSIS AND THE COLLOCATION PROCEDURE

p.7 L.177 I would suggest to delete the part "...and 500 m in the lower stratosphere." It is not really necessary nor supported by Fig. S2.

Response: Amended.

p.7 L.184 If the orographic gravity wave dispersion is an ECMWF product, it should be listed here as well.

Response: Amended. The paragraph has been modified as:

"…In addition, the standard deviations of orography (SDOR) and the gravity wave dissipation due to the effects of stress associated with unresolved valleys, hills and mountains in ERA5 reanalysis are extracted…"

2.3 THE OCCURRENCE FREQUENCY OF KHI AND ITS UNCERTAINTY

p.7 L.190 I am missing an introduction of the critical Richardson number as a quantity which can be derived from linear theory. I personally would include this information in a study which focuses on the Richardson number.

Response: The related lines have been modified as:

"Based on a linear theory, the threshold Ri (Rit) defines the threshold where the air flow changes from stability to turbulence, and it is usually suggested to be 1/4 (Haack et al., 2014). Ri is formulated as…"

p.7 L.193 please give the definition of the Brunt-Väisälä frequency and the vertical wind shear.

Response: Amended.

p.7 L.198 "The occurrence frequency of KHI is defined as the ratio of Ri<1/4 relative to all Ri calculations at a specified time period or height interval."
I don't think this sentence is necessary, and it can be misleading since you vary the threshold for the Richardson number.

Response: This statement has been deleted.

p.7 L.200 "In Eq.(1), the length scale of overbar could potentially impact the value of Ri, and eventually, the occurrence frequency of KHI. In addition, the critical value of Ri and the vertical resolution of archived radiosonde could also cause the change in Ri values."
I would suggest to rephrase these two sentences to make the key point more clear.
Suggestion: "The Richardson number calculated from Eq.(1) depends on the vertical resolution of the underlying data, as well as on the averaging interval. Ultimately, this influences the estimated occurrence frequency for subcritical Richardson numbers as a proxy for KHI. We resample the HVRRS data..."

Response: Very thanks for the patient modification! The correction has been made as suggested.

2.4 GRAVITY WAVE ENERGY

p.8 L.226 "Only the perturbations with vertical wavelengths of 0.3–6.9 km are considered as GWs (Wang and Geller, 2003)."
For clarification, is this due to the vertical limitation from 2-8.9 km (i.e. 6.9 km as an

Response: The bandpass of filter was not well determined in different literature. For
instance, Chen et al. (2019, CD) applied a passband of 1–4.5 km to the disturbance field
to extract wave energy. In our recent study on tropospheric gravity waves carried out in
2020/2021 (Zhang et al., 2022, JGR-Atmospheres), we tested several bandpass filters
and found that these filters have little effect on the overall distribution of gravity wave
energy.

Prof. Wang and Prof. Geller did a lot of pioneering work on radiosonde-determined
GWs. They referred the wavelength to be 0.3–6.9 km on their several studies, for
instance, Wang and Geller (2005, JAS). Most GW energy concentrates on waves with
vertical wavelength over 1 km, and the high-pass filter with a length of 0.3 km has
limited impact on GW energy. In addition, as stated by Alexander (1998), the largest
wavelength is not well determined, which is acknowledged as the radiosonde's
"observational filter". This concern has been included in the main text as:

"…The mean vertical wavelength of GWs is about 2 km (Wang et al., 2005), and
therefore, the lowermost threshold of 0.3 km could have little influence on the GW
energy. However, the retrieval of the largest wavelength is not well determined, which
is acknowledged as the radiosonde's "observational filter" (Alexander, 1998)…"

3   RESULTS AND DISCUSSION

Response: As compared to the HVRRS, these shears are slightly underestimated by
5.37 m/s/km, based on all sounding measurements (Fig.2b). The correction has been
made in the updated file.

Response: This statement has been delated in the revised version.

Response: This phrase has been removed as suggested.

Response: Amended.

Response: Thank. Point taken.

Response: Thanks for the modification! Point taken.

Response: The link of Table 1 has been added.

Response: Point taken.

Response: This was caused by data processing errors. Two of files on 2017 and 2020

were only partly downloaded. The following CDO operation raised errors when calculating the monthly mean wind shears. We have fixed these errors and updated the figure.
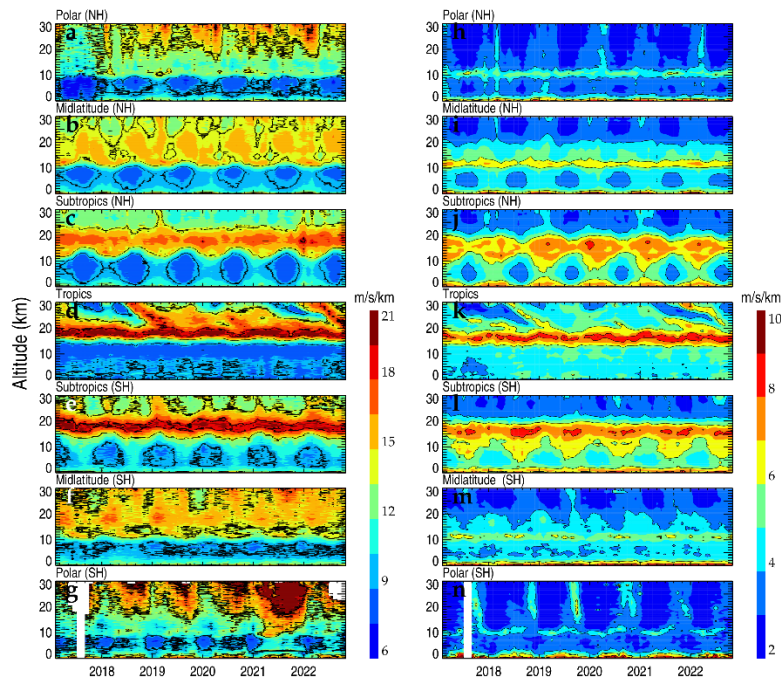

Figure 3. Monthly mean wind shears during years 2017–2022 in HVRRS (a–g) and ERA5 reanalysis (h–n) at different climate zones. The ERA5 derived wind shears are spatially and temporally collocated with those of HVRRS. NH=Northern Hemisphere; SH=Southern Hemisphere.

p. 10 L.279: "…which is about 3 km lower than that in the HVRRS."
Just out of interest, would you say this is the same shear layer shifted downward, i.e., a 3 km altitude bias in the dynamic structure of the UTLS?
Or are the shear magnitudes at the altitude of the maximum in ERA5 comparable to the ones in the HVRRS and the model fails to resolve the further increasing shear in the stratosphere?
Response: According to the fact that the vertical resolution of ERA5 in the UTLS smoothly decreases with height, we trend to suppose that model might be failed to resolve the further increasing shear in the stratosphere. Therefore, we have added a statement in the revised version as:
"…One possible reason might be that the model fails to resolve the further increasing shear in the lower stratosphere…"

p.10 L.283 I would suggest to remove the second part of the sentence, and just write: "The comparison between HVRRS-based and ERA5-based shears at three typical regimes are tabulated in Table 1."

Response: Thanks! Amended as suggested.

Response: Point taken. The comparison has been conducted at four height intervals, they are, 0–5 km a.s.l., 5–10 km a.s.l., 10–15 km a.s.l., and 15–20 km a.s.l., as displayed in the revised Figure S3.
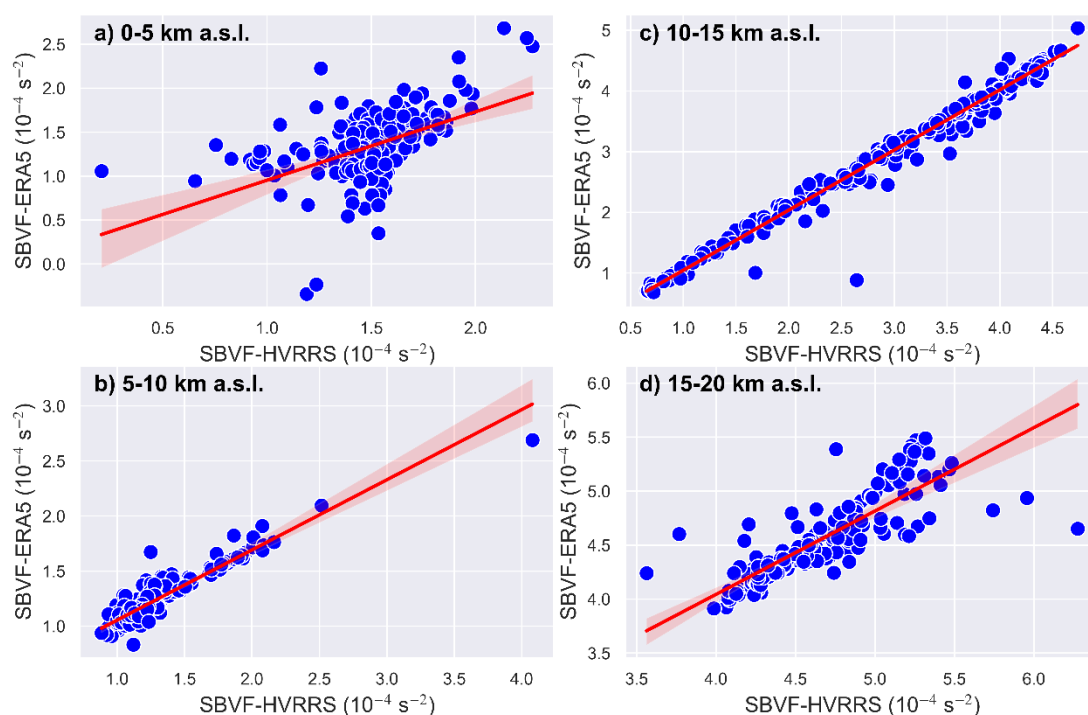

Figure S3. The joint distributions of HVRRS-retrieved and ERA5-determined squared Brunt-väisälä frequency together with the linear regression (red line) at heights of 0–5 km a.s.l. (a), 5–10 km a.s.l. (b), 10–15 km a.s.l. (c), and 15–20 km a.s.l. (d). The light red shadow denotes a significance of 95%. The Brunt-väisälä frequency is averaged during the whole study period. The ERA5 derived N^2 is spatially and temporally collocated with that of HVRRS.

Response: Based on the spatial distribution of $OF(Ri<Rit)$ (Figure 9a), $OF(0<Ri<Rit)$

(Figure S5a), and wind shear (Figure 2b) at 0–2 km a.s.l., the high occurrence frequency of subcritical Ri could be attributed to negative or small N^2. The clue in Figure S3a could also support this conclusion. In the updated file, we stated that

"…The high occurrence frequency in the PBL regime could be likely related to the negative or small N^2…"

p.11 L.307 "In addition, an obvious seasonal cycle of occurrence frequencies is revealed by HVRRS in the middle and upper troposphere and has a maximal in the spring season (March–April–May), which is consistent with the finding in Zhang et al. (2019).
The maximum in the middle troposphere is during spring, the upper tropospheric maximum is also evident in winter I would say.
Response: The modification has been made as suggested.

p.11 L.313 What does the value 8% refer to? Is it an estimation from Fig.4, or was it calculated from the data? If the latter is the case, which data exactly?
Response: In the updated file, we rephrased this sentence to be:
"…However, the ERA5 reanalysis does not provide such a seasonal cycle pattern, and the occurrence frequency of $Ri<1/4$ is significantly underestimated by around 8% (Fig.4b)…"

p.12 L.319 In Fig. 5 it says "10-15 km".
Response: The correction in the main text has been made.

Figure 6: Maybe line plots would increase the readability compared to scatter plots. Or smaller markers.
Response: Point taken. Figure 6 has been replaced with line plots.
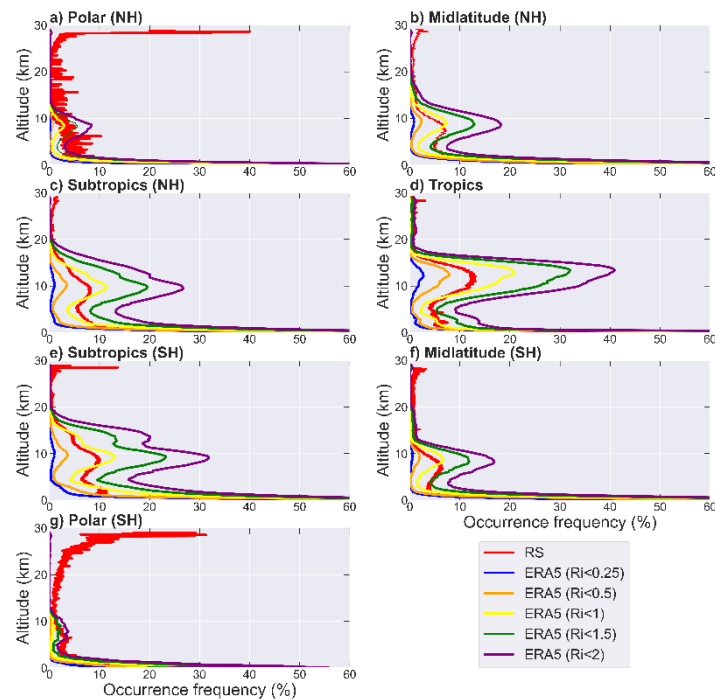
Figure 6. The altitude variation of the occurrence frequency of *Ri* below certain thresholds (0.25, 0.5, 1, 1.5, and 2) in ERA5 reanalysis in various climate zones. The ERA5 derived *Ri* is spatially and temporally collocated with that of HVRRS. The occurrences of *Ri*<1/4 in HVRRS are overlapped with red lines.

p.13 L.345 Please see major comment 01. Of course this is open to discussion, but I would suggest to replace OF(KHI) in the following analyses with OF(Ri_{ERA5}<1) or something similar. I don't think this lessens the impact of the results.

Response: Point taken. All *OF*(KHI)s have been replaced throughout all texts, tables, and figures.

p.13 L.358 I am not sure if I can follow this argumentation. Could you point out exemplary regions for the spatial consistency?

How meaningful is it to compare multi-year averages for these two highly variable quantities?

Response: The boundary layer height in ERA5 reanalysis is based on the bulk Richardson number algorithm, which is closely associated with the *OF*(*Ri*<*Rit*). Over ocean, the spatial variation of *OF*(*Ri*<*Rit*) and PBLH is quite similar. However, the continental *OF*(*Ri*<*Rit*) at 0–2 km a.s.l. is significantly influenced by the underlying terrain, especially over region with high elevations.

p.13 L.361 See major comment 01.

Response: The statement has been deleted due to the change of context.

Response: Point taken, thanks.

Response: The incomplete ERA5 files has been replaced. Also, Figure 10 has been replotted. While the contour lines in (a-g) are too dense to be read, as shown in Figure A. Therefore, the contour lines in Figure 10a-g are still missing.
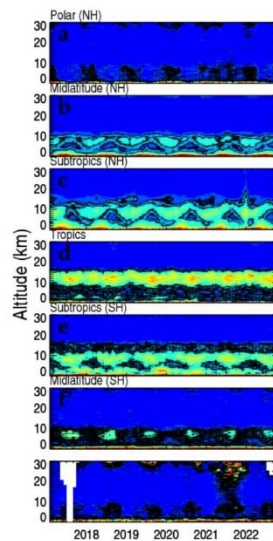

Figure A. The monthly averaged *OF(Ri<Rit)* in the HVRRS (a–g).
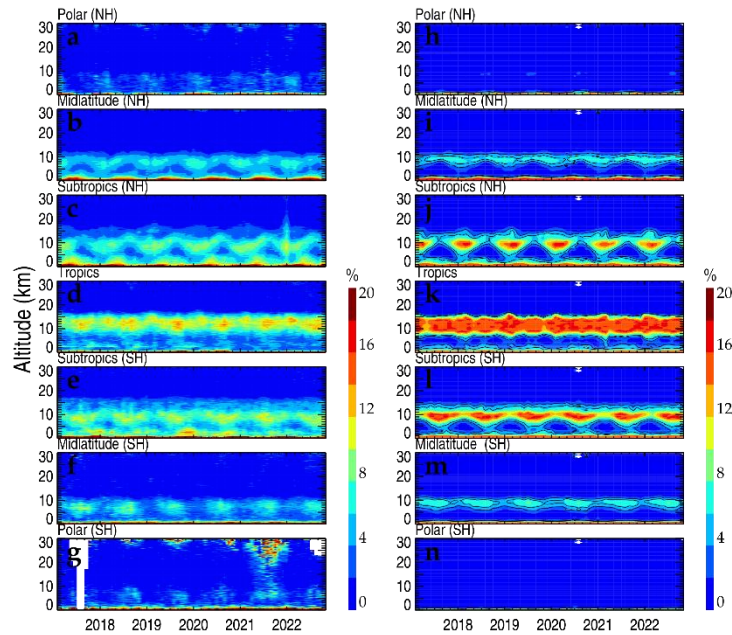
Figure 10. The monthly averaged *OF(Ri<Rit)* in the HVRRS (a–g) and ERA5 reanalysis (h–n) in seven climate zones. NH=Northern Hemisphere; SH=Southern Hemisphere.

p.14 L.372 I would suggest to remove the sentence "For regions without high-resolved wind and temperature measurements, the ERA5 model product could be a good choice to represent the thermodynamic instability of background atmosphere."
This has been introduced in the motivation for the study.
Response: Point taken.

p.14 L.377 Suggestion: "The seasonal variation of OF(Ri<Rit) with Rit,HVRRS=1/4 and Rit,ERA5=1 for all climate zones is further analyzed in Figure 10. In the mid latitudes and subtropics, the OF(Ri<Rit) exhibits maximum values in the PBL, as well as a local minimum in the middletroposphere and a local maximum at altitudes around 9 km. In the stratosphere the occurrence frequencies decrease to values of the order of 1% (Fig.10b,c,e,f)."
There is a significant variability in the vertical profiles across the different data sets, seasons and climate zones, and the single-value average occurrence frequencies in the current version of this paragraph might be confusing.
Response: Thanks for the correction. Modification has been made.

Figure 10: I would suggest to remove the gray shaded areas, I don't think they are necessary for the analysis.
Please adjust the x-axis range in Fig. 10k and 10l so that the JJA maxima are not cut off.

Response: Amended as suggested.

Response: Amended as suggested

Response: As stated above, the modification concerning underly terrains has been made.

Figure 11: Could you plot the data as a binned 2d-histogram? What is the unit at the x-axis?
Response: The Figure has replotted as 2d-histrogram. SDOR is dimensionless, which is indicated in the caption of Figure 12.

Response: Thanks. We recalculated all ERA5 data to estimate $OF(0<Ri<Rit)$ (Figure S5 in the updated supporting information) and found that $OF(0<Ri<Rit)$ also has a significant enhancement over the Niño 3 region.

Response: The paragraph has been modified to be:
"In the free troposphere where the convection activity is generally weak, KHI is preferentially generated from strong wind shear, which may be closely associated with mean flows and wave activities."

Provided that you agree with my criticism concerning the results of the analysis, I don't know what would be the best way for improvement:
You could either go without this part of the analysis (I don't think it would hurt the overall impact of the manuscript too much).
Or you could present the result without such a strong interpretation and leave more room for interpretation. I am not sure how meaningful this would be.
Or you could go into a more detailed analysis to try and sharpen the results. If this is within the scope of the study.
Response: We agree with your comment. This part has been totally deleted in the updated file.

p.16 L.436 "Overall, large OF(KHI) always corresponds to strong GW activities and large wind shears, likely indicating that GW activity is crucial for the occurrence of KHI."

Is this derived from Fig. 14b? If so please link the Figure in the text.

I also think that a more detailed description would be helpful. What do you define as "strong GW activity" in the plot? Where do you see the correlation between strong GW activity and OF(Ri<Rit)? Maybe I am misreading the plot, but I find it hard to follow the conclusion.

Response: Based on the comment, we modified the statement to be"

"…The joint distribution of $OF(Ri<Rit)$ with GW energy and wind shear indicates that large $OF(Ri<Rit)$ (for instance, larger than 10%) generally corresponds to GW energy larger than 10 J/kg or wind shear exceeds 14 m/s/km (Fig. 14b)..."

p.16 L.440 Is the orographic gravity wave dissipation an ECMWF product? If this is the case please include it in the data description.

And just for clarification, is this the quantity derived from the parametrised gravity wave drag due to subgrid-scale orography? So you identify regions and time steps (months) of strong resolved gravity wave activity based on the parametrised non-resolved gravity waves?

Response: In the website of ECMWF, it says "It (GW dissipation) is calculated by the ECMWF Integrated Forecasting System's sub-grid orography scheme, which represents stress due to unresolved valleys, hills and mountains with horizontal scales between 5 km and the model grid-scale". In the data description, we have added the related description about GW dissipation.

This parameter cannot represent resolved gravity waves, which could be still a challenge for general circulation models. We extracted this parameter was to investigate the possible association between mountain barrier and $OF(Ri<Rit)$ by the propagation or dissipation of mountain waves, since radiosondes can only provide limited information on global mountain waves.

Figure S5: According to the text (p.16 L.443) the correlation is based on monthly averaged values (of the gravity wave dissipation and the OF(Ri<Rit)). Please include this information in the description of Fig. S5.

Response: Amended.

Again just for clarification: What years are the data basis for this plot? 2017-2022? So about 60 data points are correlated at each grid point?

Response: Yes, these data cover time period of 201701 to 202210, and 70 data points were used. This information has been included in the caption of Figure S7 in the

updated files

Response: As suggested, we added four sub-plots around Figure S7a, to demonstrate the temporal variation of GW dissipation and $OF(Ri<Rit)$. Two of them locate at the Rocky Mountain and the rest of them locate at the Andes Mountain. These four grids were randomly selected. Based on these sub-plots, the monthly averaged parametrized GW dissipation keeps high consistency with the monthly $OF(Ri<Rit)$.
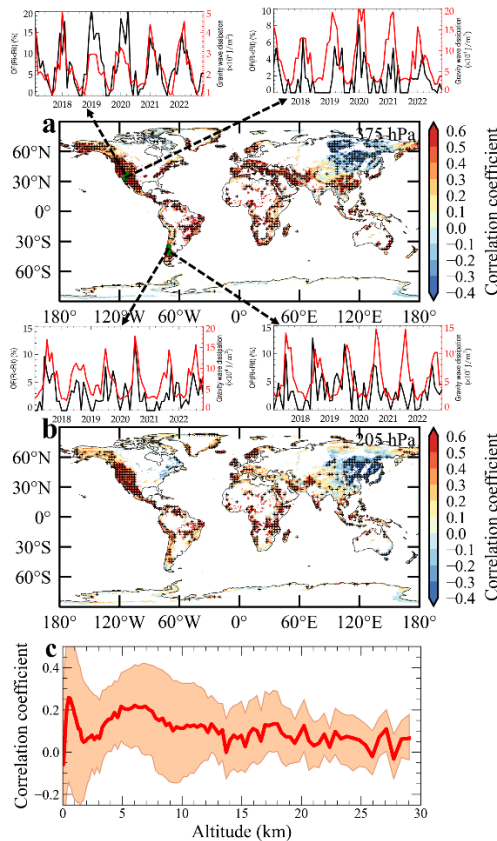

Figure S7. The correlation coefficient between monthly averaged ERA5-based orographic GW dissipation and monthly ERA5-based $OF(Ri<Rit)$ at pressure levels of

375 hPa (a) and 205 hPa (b) during time period of January 2017 to October 2022. where plus signs indicate that the values are statistically significant (*p*<0.05). Four subplots around (a) display the monthly variation of orographic GW dissipation (red) and *OF(Ri<Rit)* (black) over two grids of the Rocky Mountain and two grids of the Andes Mountain. The coefficient at continuous heights from the ground up to 30 km is further displayed in (c), where the light red shadow denotes a significance of 95%.

Figure 15: I am not sure if I read this plot correctly. The bin size for the filled color contour (i.e., the OF(RiHVRRS<1/4)) is apparently something of the order of 1 m/s in the x-axis, and 1 m/s/km in the yaxis. If this is the case, shouldn't most of the filled contour display missing values, within 5x5 bins where only 1 or 2 matched profiles are located?

Response: We checked the plot codes of Figure15 as well as Figure 14b and found that one of line was badly designed, which can raise index error when the grid has no matched data. We redesigned the code and replotted Figure15 and Figure 14b.
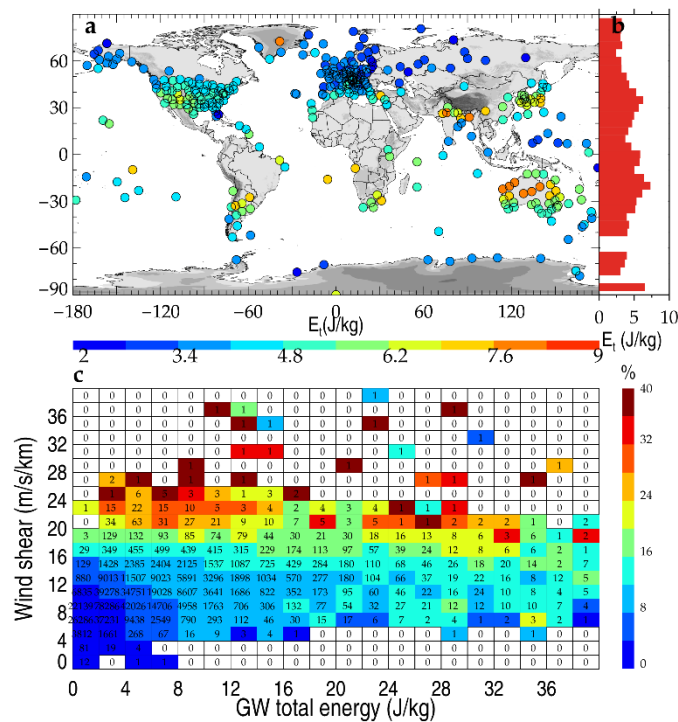

Figure 14. Geographical distribution of mean tropospheric GW total energy obtained from the HVRRS (a). The latitudinal variation of mean energy in a grid cell of 5° latitude (b). The joint distribution of *OF(Ri<Rit)* with GW energy and wind shear (c). The *OF(Ri<Rit)* and wind shear are derived from individual HVRRS profiles and vertically averaged over the tropospheric segment that is used for GW study. The numerical number in (c) indicates the matched profile number in each grid, using a bin size of 2 J/kg along the x axis and 2 m/s/km along the y axis.
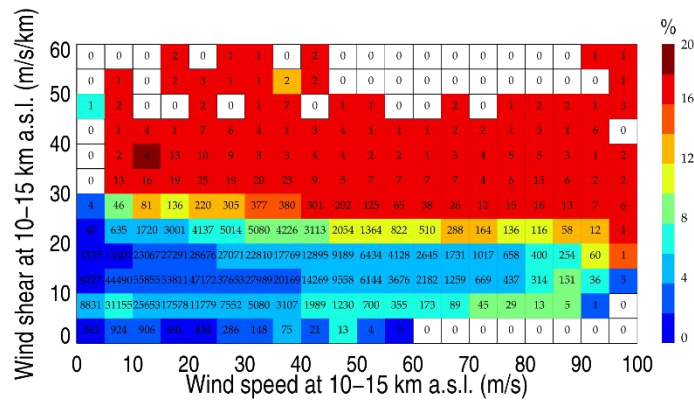
Figure 15. Joint distribution of HVRRS-derived wind speed, wind shear, and *OF(Ri<Rit)*, with a bin size of 5 m/s along the x axis and 5 m/s/km along the y axis. Note that all the relationship is based on the mean result of individual profiles at heights of 10–15 km a.s.l.. The number indicates the matched profile number in each grid.

p.17 L.461 "The occurrence of KHI is potential crucial for many implications, such as aircraft, mass transfer, and climate change, just name a few, but it is very hard to be globally understood due to its fine structure."
Please rephrase this sentence.
Aircraft → aircraft safety?
Climate change → how?
Response: The sentence has been rephrased to be:
"…The occurrence of KHI is potential crucial for many implications, such as aircraft safety and mass transfer, but it is very hard to be globally understood due to its fine structure..."

p.17 L.463 Suggestion: "This study uses the ERA5 as the latest reanalysis product from the ECMWF as well as a comprehensive data set of HVRRS radiosonde soundings to globally characterize the distribution of low Richardson numbers as a proxy for the occurrence of KHI, for the years 2017 to 2022."
Response: Thanks! Amended.

p.17 L.471 I would suggest to remove the sentence "The underestimation therefore influences the performance of KHI analysis."
Response: Amended as suggested

p.17 L.473 See major comment 02.
Response: The statement has been modified as:
"…In addition, it is weak correlated with HVRRS-determined ones at most heights and over most climate zones…"

Response: Based on Table 4, $OF(Ri<Rit)$s at 10–15 km a.s.l. over midlatitude and subtropic regions revealed by ERA5 and radiosonde are quite similar in terms of magnitude.

Response: The correlation between $OF(Ri<Rit)$ and near-surface wind has removed from manuscript, as stated above. The joint distributions among $OF(Ri<Rit)/OF(0<Ri<Rit)$, gravity waves and background wind speed have been reworked. Accordingly, the conclusion has been revised.

Response: Thanks. Amended as suggested.

Response: Done

Response: Done

Response: Done

Response: Done

Response: Done

Response: Done

Response: Amended

Response: Done

Response: Done

Response: Done

Response: Done

Response: Done

Response: Done

Response: Done

Response: Done

p.17 L.468 "vertical wind shear" instead of "shears"

Response: Amended.

<u>References:</u>

Dutton, J. A., & Panofsky, H. A. (1970). Clear air turbulence: A mystery may be unfolding. Science, 167(3920), 937–944. https://doi.org/10.1126/science.167.3920.937

Kaluza, T., Kunkel, D., & Hoor, P. On the occurrence of strong vertical wind shear in the tropopause region: A 10-year ERA5 northern hemispheric study. Weather and Climate Dynamics, 2(3), 631–651. https://doi.org/10.5194/wcd-2-631-2021, 2021.

Lee, J. H., Kim, J.-H., Sharman, R. D., Kim, J., & Son, S.-W. Climatology of Clear-Air Turbulence in upper troposphere and lower stratosphere in the Northern Hemisphere using ERA5 reanalysis data. Journal of Geophysical Research: Atmospheres, 128, e2022JD037679. https://doi.org/10.1029/2022JD037679, 2023.

Roja Raman, M., Jagannadha Rao, V. V., Venkat Ratnam, M., Rajeevan, M., Rao, S. V., Narayana Rao, D., and Prabhakara Rao, N.: Characteristics of the Tropical Easterly Jet: Long-term trends and their features during active and break monsoon phases, J. Geophys. Res.-Atmos., 114, 1–14, https://doi.org/10.1029/2009JD012065, 2009.

Sharman, R. D., & Pearson, J. M. Prediction of energy dissipation rates for aviation turbulence. Part I: Forecasting nonconvective turbulence. Journal of Applied Meteorology and Climatology, 56(2), 317–337. https://doi.org/10.1175/JAMC-D-16-0205.1, 2017.

Sunilkumar, S. V., Muhsin, M., Parameswaran, K., Venkat Ratnam, M., Ramkumar, G., Rajeev, K., Krishna Murthy, B. V., Sambhu Namboodiri, K. V., Subrahmanyam, K. V., Kishore Kumar, K., and Shankar Das, S.: Characteristics of turbulence in the troposphere and lower stratosphere over the Indian Peninsula, J. Atmos. Sol.-Terr. Phys., 133, 36–53, https://doi.org/10.1016/j.jastp.2015.07.015, 2015.

Response: Many thanks for the provided references