Response to Reviewers #1 Comments

We thank the associate editor, editor and two anonymous reviewers again 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:

Review report on the paper "Occurrence frequency of subcritical Richardson number assessed by global high-resolution radiosonde and ERA5 reanalysis" by Shao et al., ACPD.

The authors responded to the questions raised by the reviewers, and improved the manuscript which is now more conclusive compared to the initial submission, both in the handling of the data as well as in the interpretation and description of the results. Still, I think a few clarifications are necessary, mainly of technical or linguistic character.

Response: We sincerely thank the reviewer again for your professional and detailed comments. According to your suggestion, we have fixed errors caused by ERA5 coordinates throughout the texts, which will be discussed later. Another error we have fixed is Figure 9. The altitude in Fig.9b should be 5-10 km rather than 10-15 km (We forgot to inverse the model level of ERA5 in this program).

p.5 L. 121 What does "the last version of the ECMWF model" refer to? The latest reanalysis product? The IFS version?

Response: Sorry for the mistake. "last" should be replace by "latest". The sentence has been rephrased as:

"…By comparison, ERA5 global reanalysis can provide a seamless coverage of temperature and wind, and it is the latest generation of the European Centre for Medium-Range Weather Forecasts (ECMWF) atmospheric reanalysis and is based on the state-of-the-art Integrated Forecasting System (IFS) Cy41r2 (Hersbach et al., 2020; Gu et al., 2023)…".

p.11 L.289 According to Fig. S3 the static stability is not averaged from the surface to 30 km.

Response: Sorry we forget to modified the main text in our last version. Now the sentence has been modified to be:

"…By comparison, the ERA5-acquired N^2 averaged over four height intervals (e.g., 0–5, 5–10, 10–15, 15–20 km a.g.l.) is reliably estimated at all heights…".

Figure 8: Out of curiosity, is the ERA5 distribution above 17 km altitude so irregular because the data density is so low for both Ri<0 and Ri<Rit? How meaningful is the top part of the plot then?

Response: The occurrence frequency of *Ri*<*Rit* for ERA5 is as low as 0.05% in the lower stratosphere (Tab. 4c), which can lead to the abrupt change in terms of *OF*(*Ri*<0)/ *OF*(*Ri*<*Rit*) in the lower stratosphere. The occurrence frequency of *Ri*<*Rit* above 17 km altitude could have potential implications for the investigation of clear air turbulence (CAT), which can be commonly observed in the upper troposphere and lower stratosphere (UTLS). In addition, ERA5 was also used for the study of upper-level turbulence encountered by cruising aircraft (for instance, Lee et al., 2023, JGR-Atmospheres). Also turbulence (or wind shear) in the UTLS have implications for constituent mixing across the tropopause (Lee et al., 2019, Nature). The present analysis can provide some information on the quantitative comparison between ERA5 and radiosonde in the UTLS region.

p.15 L.425ff I would expect the vertical resolution to be enhanced over mountainous areas, due to the surface-following hybrid sigma-pressure coordinates.

Response: We agree. In the previous version, we transferred ERA5 model level to geopotential height based on the definition in https://confluence.ecmwf.int/display/UDOC/L137+model+level+definitions, which can lead to substantial errors in estimating terrestrial $OF(Ri<Rit)$ in the low troposphere. The IFS model level indeed follows hybrid sigma-pressure coordinates, and the calculation (model level to geopotential height) should follow the procedure posted on the ECMWF website (https://confluence.ecmwf.int/display/CKB/ERA5%3A+compute+pressure+and+geopotential+on+model+levels%2C+geopotential+height+and+geometric+height#ERA5:computepressureandgeopotentialonmodellevels,geopotentialheightandgeometricheight-Pressureonmodellevels). The geopotential is estimated based on the python program "compute_geopotential_on_ml.py".

The updated coordinates will lead to some changes in wind shears and $OF(Ri<Rit)$, mainly in the low troposphere. Therefore, we have recalculated all results throughout the text based on the hybrid sigma-pressure coordinate.

Thanks again for your very professional comments, which help us to fix a big technical error.

p.16 L.450 "Generally weak" compared to where? I am not sure that I would agree with this statement.

Response: The statement has been rephrased as:

"…In the free troposphere the percentage of $OF(Ri<0)$ relative to $OF(Ri<Rit)$ is generally less than 20% (Fig. 8), KHI is preferentially generated from strong wind shear…"

p.17 L.471 Just to be sure, do the unresolved orographic gravity waves (their dissipation) cause the low Richardson numbers, or do the unresolved orographic waves occur along with resolved orographic gravity waves which impact the occurrence of low Richardson numbers? Or both effects? Maybe rephrase the sentence to make it more clear how you interpret the results.

Response: The present analysis can only imply the potential contribution from unresolved orographic waves. It is hard to quantify the effect of resolved orographic GWs on *Ri* here.

In Yasiui et al. (2018), resolved GWs in the MLT region was found to interact with wind shears. Also in Lachnitt et al. (2023), they stated that orographic waves lead to turbulent mixing in the troposphere and in the stratosphere.

However, it would be difficult for us to conclude the role of resolved orographic waves in present analysis. We feel sorry for that.

The above concern has been incorporated in the main text.

References:

Yasui, R., Sato, K., & Miyoshi, Y. (2018). The momentum budget in the stratosphere, mesosphere, and lower thermosphere. Part II: The in situ generation of gravity waves. Journal of the Atmospheric Sciences, 75(10), 3635-3651

Lachnitt, H.C., Hoor, P., Kunkel, D., Bramberger, M., Dörnbrack, A., Müller, S., Reutt er, P., Giez, A., Kaluza, T., and Rapp, M.: Gravity-wave-induced cross isentropic mixing: a DEEPWAVE case study, Atmos. Chem. Phys., 23, 355–373, https://doi.org/10.5194/acp-23-355-2023, 2023.

p.17 L.477 I find it a bit hard to follow the interpretation of Fig. 15.

OF(Ri<Rit) > 10% would be yellow and above in the colorscale, I see no direct connection to the wind speed threshold of 25 m/s. The occurrence frequency OF(Ri<Rit) depends mainly (directly) on wind shear, and the average wind shear (along with OF(Ri<Rit)) increases somewhat with the average wind speed. However, this is mainly evident in Fig. S9b and not in Fig. 15. Maybe rephrase this paragraph. p.19 L.523 would have to be adjusted accordingly.

Response: We agree. $OF(Ri<Rit)>10\%$ can be frequently observed when wind shear is larger than 20 m/s/km, rather than wind speed exceeding 25 m/s. The correction has been made in the main text.

Figure 12: I believe SDOR should have meter as unit.

Response: According to the ERA5 website posted on https://cds.climate.copernicus.eu/cdsapp#!/dataset/reanalysis-era5-single-levels?tab=overview, SDOR is dimensionless.

Response to Reviewers #2 Comments

We thank the associate editor, editor and two anonymous reviewers again 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 #2 Evaluations:

Review report on the revised paper "Occurrence frequency of subcritical Richardson number assessed by global highresolution radiosonde and ERA5 reanalysis" by Shao et al. submitted to the journal Amospheric Chemistry and Physics.

Overview This new version of the article shows substantial improvements. The authors have responded in detail to questions and suggestions. The modification of the title is welcome, as are the analyses of RS with vertical resolutions close to those of ERA5. The climatological results appear interesting (vertical shears and occurrence frequency of Ri

Response: We sincerely appreciate the reviewer again for your patient and insightful comments and assessments. According to your suggestion, we have addressed the issue concerning radiosonde resolution in the revised version.

Major comments

• Paragraph 2.3: The comparison of Ri estimates and shears from radiosondes with resolutions comparable to those of the model (Table 2), seems relevant. However, I still don't understand your estimate of the vertical gradient evaluated over 10 m and averaged over 200 m. Doesn't arithmetic averaging finite differences over 10 m in 200 m windows amount to estimating the finite difference over 200 m?

1 n∑i=1 n ΔTi Δz = Tn−T1 nΔ z

If so, the gradients, and hence the Richardson numbers, are estimated from finite differences of 200 m! Such a conclusion is supported by your figure 1 as gradients estimated with a vertical resolution of 10 m averaged over 20 bins are very close to the gradients estimated with a resolution of 50 m averaged over 4 bins. Please, clarify this (important) point: are averaged finite differences representative of vertical gradients over 200 m or over 10 m?

Response: The calculation of *Ri* was handled over a vertical gradient of 10-m. While a moving average was previously applied to wind shear and buoyancy frequency. The averaged parameter at altitude *i* can be represented as:

$$\bar{A}(i) = \frac{1}{n}\sum_{j=i-10}^{i+10} A(j)$$

where A demotes wind shear or Brunt-Väisälä frequency and n is the number of vertical bin.

Since 10-m radiosonde variables can be highly polluted by measurement noises, a moving average would be a necessary. In the following text, we will address this issue in more detail.

The above statement has been incorporated in the main text.

• The fact that the frequency of occurrence of Ri<1 in ERA5 is climatologically consistent with that of Ri<1/4 in radiosondes is fortuitous, since it depends on the effective vertical resolution for Ri estimated from the RSs. For example, if we used a better resolution for RS gradients, say 30 m, we would have a higher frequency of occurrence of KHI, and therefore better agreement with a threshold of Ri < 1.5 or 2 from ERA5. I suggest you comment on this point.

Response: The variation of buoyancy frequency and wind shear is strongly influenced by turbulence fluctuations and measurement noises. For instance, in Fig. 3d of Kantha

& Hocking (2011), turbulence can be frequently observed at almost all heights (Thorpe scale greater 0 can be roughly taken as the occurrence of turbulence). Without a moving average, many of the square of the buoyancy frequencies will be less than 0 for a 10-m resolution radiosonde, especially in the boundary layer.

The outer scale of turbulence is about few hundred meters in the boundary layer (Solanki et al., 2022), and then decreases to around 100 m in the troposphere (Rao et al., 2001). For large-eddy simulations, the spatial resolution for turbulence study typically ranges from around 5-m to 100-m, for instance, Schulte et al. (2022), Schalkwijk et al. (2015), Verrelle et al. (2017), and Strauss et al. (2022). In addition, for 10-m radiosonde, measurements noises are a big problem (more information refers to Wilson & Luce, 2011). In Wilson & Luce (2011), they split the profile in segment of 200 m to estimate noises. Therefore, we applied a 200-m moving average procedure to inhabit the effect from turbulence fluctuations and measurement noises.

Without a smoothing in vertical, a higher resolution generally leads to a larger occurrence frequency of $Ri<Rit$. For example, the averaged occurrence frequency of $Ri<Rit$ at 10-15 km a.g.l. is 5.29% for the ERA5 reanalysis, while it is as high as 30% for 10-m radiosondes. In this case, the threshold Ri for the ERA5 reanalysis will even exceed 3, to produce a comparable $OF(Ri<Rit)$ with 10-m radiosondes.

Moreover, in the conclusion section, we have added a phrase to address the limitation of present analysis:

"... It is worth highlighting that HVRRS experiences a 200-m vertical moving average procedure to inhabit measurement noises and turbulence fluctuations. Without this procedure, the threshold Ri for the ERA5 reanalysis would even higher than 1."

References:

Solanki, R., Guo, J., Lv, Y., Zhang, J., Wu, J., Tong, B., & Li, J. (2022). Elucidating the atmospheric boundary layer turbulence by combining UHF radar wind profiler and radiosonde measurements over urban area of Beijing. Urban Climate, 43, 101151.

Rao, D. N., Rao, T. N., Venkataratnam, M., Thulasiraman, S., Rao, S. V. B., Srinivasulu, P., & Rao, P. B. (2001). Diurnal and seasonal variability of turbulence parameters observed with Indian mesosphere-stratosphere-troposphere radar. Radio Science, 36(6), 1439-1457.

Schulte, R. B., van Zanten, M. C., van Stratum, B. J. H., and Vilà-Guerau de Arellano, J.: Assessing the representativity of NH3 measurements influenced by boundary-layer dynamics and the turbulent dispersion of a nearby emission source, Atmos. Chem. Phys., 22, 8241–8257, https://doi.org/10.5194/acp-22-8241-2022, 2022.

Schalkwijk, J., H. J. J. Jonker, A. P. Siebesma, and F. C. Bosveld, 2015: A year-long large-eddy simulation of the weather over Cabauw: An overview. Mon. Wea. Rev., 143, 828–844, doi:10.1175/MWR-D-14-00293.1.

Verrelle, A., D. Ricard, and C. Lac, 2017: Evaluation and improvement of turbulence parameterization inside deep convective clouds at kilometer-scale resolution. Mon. Wea. Rev., 145, 3947–3967, https://doi.org/10.1175/MWR-D-16-0404.1.

Strauss, C., Ricard, D., & Lac, C. (2022). Dynamics of the cloud–environment interface and turbulence effects in an LES of a growing cumulus congestus. Journal of the Atmospheric Sciences, 79(3), 593-619.

Kantha, L., & Hocking, W. (2011). Dissipation rates of turbulence kinetic energy in the free atmosphere: MST radar and radiosondes. Journal of Atmospheric and Solar-Terrestrial Physics, 73(9), 1043-1051.

Wilson, R., Dalaudier, F., and Luce, H.: Can one detect small-scale turbulence from standard meteorological radiosondes?, Atmos. Meas. Tech., 4, 795–804, https://doi.org/10.5194/amt-4-795-2011, 2011.

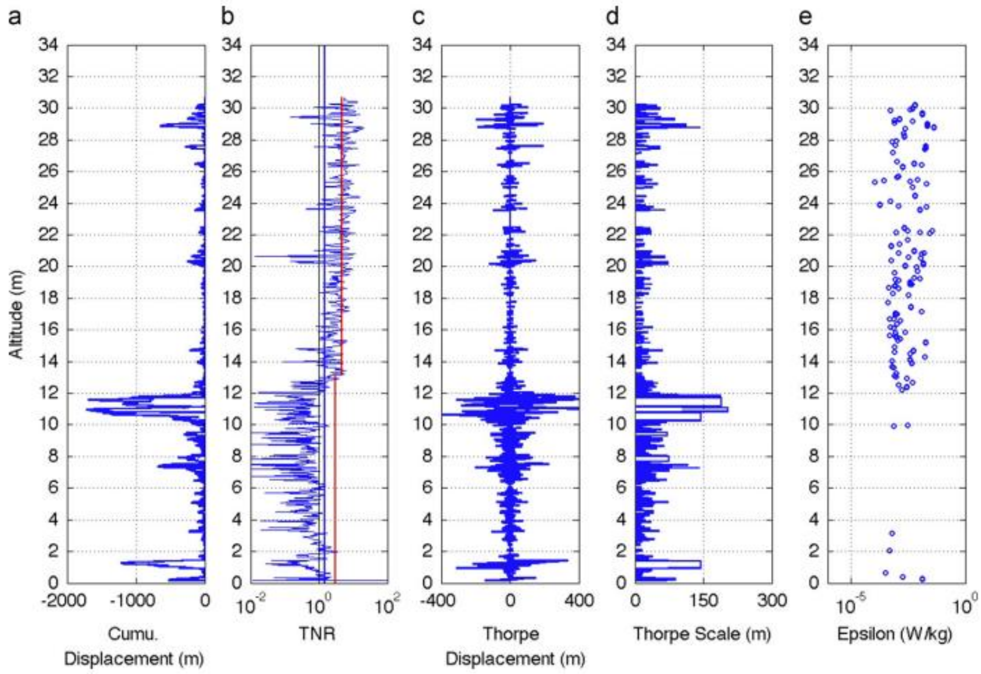

Fig. 3. (a) Cumulative displacement (m); (b) Trend to noise ratio (TNR); (c) Thorpe displacement (m); (d) Thorpe scale (m); and (e) TKE dissipation rate (W/kg) for the sonde 070623B (#8) released on June 23, 2007. In panel (b), the blue line shows TNR = 1.5, and the red lines show the bulk TNR in the troposphere and the stratosphere. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

Figure A. Figure 3 in Kantha & Hocking (2011).

Specific comments

– You state here and there (e.g. line 252) that the shear resolution is equal to 10m. Is it really the case (because of the averaging procedure, see above). If so, the statement "For 10-m radiosondes, the moving average in a step of 200-m could offset the effect of chaotic movements, at least to some extent" (lines 200-202) is certainly inaccurate.

Response: Thanks for the suggestion. We have removed this statement in the revised draft. It would be difficult to assess the chaotic movement in this study. The chaotic movement may be characterized by the accuracy of wind speed. Different types of radiosonde can have various accuracies. For instance, Vaisala-92 has an accuracy of around ±0.2 m/s (Wang et al., 2020), Vaisala PTB201A has an accuracy of ±2% and 5° for wind speed and wind direction, respectively (Conroy et al., 2016), and WXT510 Vaisala has an accuracy of ±0.3 m/s (Tratt et al., 2011). However, it is also difficult for us to address the accuracy of all soundings due to the near-global distribution of radiosonde. In addition, we have included the following statement to address this issue:

"…However, it is hard to quantify the movement in present study…"

References:

Wang, D., Guo, J., Chen, A., Bian, L., Ding, M., Liu, L., et al. (2020). Temperature inversion and clouds over the Arctic Ocean observed by the 5th Chinese National Arctic Research Expedition. Journal of Geophysical Research: Atmospheres, 125, e2019JD032136. https://doi.org/10.1029/2019JD032136

Conroy, J. L., D. Noone, K. M. Cobb, J. W. Moerman, and B. L. Konecky (2016), Paired stable isotopologues in precipitation and vapor: A case study of the amount effect within western tropical Pacific storms, J. Geophys. Res. Atmos., 121, 3290–3303, doi:10.1002/2015JD023844.

Tratt, D. M., S. J. Young, D. K. Lynch, K. N. Buckland, P. D. Johnson, J. L. Hall, K. R. Westberg, M. L. Polak, B. P.Kasper, and J. Qian (2011), Remotely sensed a mmonia emission from fumarolic vents associated with a hydrothermally activ efault in the Salton Sea Geothermal Field, California, J. Geophys. Res., 116, D 21308, doi:10.1029/2011JD016282.

– I wonder about the relevance of taking into account the 0-2 km height interval in this climatological study. This altitude interval is representative of the diurnal atmospheric boundary layer (ABL) at low latitudes, but certainly not at high latitudes. Is it relevant to compare the same 0-2 km altitude interval in the Arctic and at the equator? (I don't think it is).

Response: PHL depth strongly varies with local time, latitude, season, land cover, etc. Also, algorithms can arise large uncertainties in estimating PHL depth. Its variation is a complex topic. PHL depth in the tropical can be quite different with that of polar regions. Therefore, we have referred the 0-2 km altitude as the low troposphere throughout the text, instead of PBL.

– Figure 11: please, use the same ranges for the x axis in order to help for a direct visual comparison.

Response: Amended as suggested, thanks.