Review report on the paper **“Occurrence frequency of Kelvin Helmholtz instability assessed by global high-resolution radiosonde and ERA5 reanalysis”** by Shao et al. submitted to the journal Amospheric Chemistry and Physics.

Overview

This paper describes the spatial and temporal variability of instabilities in the atmosphere from a huge radiosonde (RS) database (115 million profiles (?) from 434 stations!). The resulting statistics from the radiosonde analysis are compared to those obtained from ERA5 reanalyses. The authors observe a severe underestimation of the vertical shear from ERA5. They find a better agreement between the instability climatology from RS and ERA5 by taking a threshold Ri < 1 when using ERA5. The authors also observe a positive correlation with the standard deviation of the orography.

The analysis of the RS and ERA5 data is interesting and the climatological results seem very convincing. The comparison RS/ERA5 is also interesting. For these reasons, this work deserves publication. However, the interpretation of the results does not always seem to me to be correct. For instance, the fact of interpreting all instabilities detected from the Ri < 1/4 criterion as the result of Kelvin-Helmholtz instabilities is not justified, in particular in the boundary layer, or in the tropical troposphere. Therefore, substantial modifications seem to me necessary, if only the title.

I therefore recommend that this article be published with some substantial modifications.

Major comments

1) lines 190-217: about the method for estimating the gradients from the HVRRS profiles. If I understand correctly, a moving average of the estimated gradients is performed over height segments of 200 m. But over what vertical scale are the gradients evaluated (which are then averaged)? Are they calculated over 10 m differences? If so, why this choice if one purpose is to compare with the estimates from ERA5? Why not estimate the vertical gradients of HVRRS on vertical scales comparable to the model (100-400 m) for comparison? Moreover, it would be simple to adapt the resolution of the HVRRS gradients to the resolution of the model according to the altitude domains considered.

Figure 1 is illuminating on that purpose in showing that the estimates of the occurrence frequency (OF) of Ri on a 100 m scale, without averaging, is little different from that estimated on 10 m differences averaged over 200 m.

Such a resolution (10 m averaged on 200 m segments) is relevant to establish the climatology of instability occurrences (Ri < 1/4) obtained from HVRRS, but is arguably questionable for comparison to the climatology deduced from ERA5.

2) paragraph 3.2: The authors correctly note that the frequency of occurrence (OF) of KHIs depends on the vertical resolution of the gradients. The fact that the OFs from the model coincide better with those from the HVRRS with a threshold Ri < 1 is therefore fortuitous since it depends on the resolution of the HVRRS. (If you had calculated the gradients on a 50 m scale, and not 10 m, you would have lower OFs, and therefore a different threshold to apply on the model estimates). Can you please comment on this fact?

3) The authors systematically attribute Ri < 1/4 occurrences to KH instabilities. This is certainly not always the case. Thus, the diurnal boundary layer is very probably close to a neutral static stability (Ri ~ 0) without having anything to do with KH instabilities. The same is true in the tropical troposphere, where deep convective cells develops up to about 15 km altitude. The Ri < 1/4 occurrences are most likely a signature of an unstable flow, but not that this instability is due to a KHI. I recommend that the authors modify the discussion and conclusion paragraphs accordingly. As well as the title of the article.

4) Horizontal winds measured under radiosonde at the scale of a few tens of meters are affected by the chaotic movements of the gondola due to the pendulum and to the balloon's own movements (see for example Ingleby et al., 2022, https://doi.org/10.5194/amt-15-165-2022 and references therein). A low-pass filter is applied to the HVRRS profiles to reduce these effects. This filtering should have an impact on the effective resolution of the wind measurements (which is much larger than 10 m). Although it is difficult to assess the impact of this filtering, I suggest that you discuss this fact.

Specific comments

- **line 155**: 115 million HVRRS profiles??? Do you confirm?

- **Line 233**: a “tropospheric segment” from 2 to 8.9 km is chosen. Why this choice (if interested in the OF(KHI) in the 0-2 and 10-15 km height ranges?

- **line 240** (Fig. 2): what is the vertical resolution of the shear estimates from HVRRS? Please, specify.

- **lines 259 & 282**: Are the ERA5 shear estimates dramatically lower than the HVRRS estimates if the RS gradients are estimated with the same resolution as the model (i.e. 300 m for the 10-15 km altitude domain)?

- line 306: I agree with the statement that PBL is mixed by convection during daytime. Clearly, the occurrence of Ri < 1/4 is not attributable to KH instabilities in such cases.

- line 376: this statement is not visible in figure 9.

- lines 381-3: I doubt that the Ri < 1/4 occurrences are only due to KHI in this region where deep convective cells are frequent.

- **lines 414 & 793**: la Niño → la Niña

- line 436: I do not understand your concluding sentence, figure 14b showing precisely that the probability of occurrence Ri < 1/4 depends almost not on the total energy of the gravity waves, but almost exclusively on the horizontal wind shear (if it exceeds 18 m/s/km).

- **line 443-456**: I do not understand your conclusion about the wind speed. Figure 15 clearly shows that the occurrence Ri < 1/4 does not depend on the wind speed (if the wind speed exceeds a few m/s), but occurs with high probability if the shear exceeds ~20 m/s/km. This is a convincing result!