Third Review of the manuscript ”High-altitude atmospheric turbulence and infrasound measurements using a balloon-launched small unscrewed aircraft sustem” by A. N. Haghighi et al.

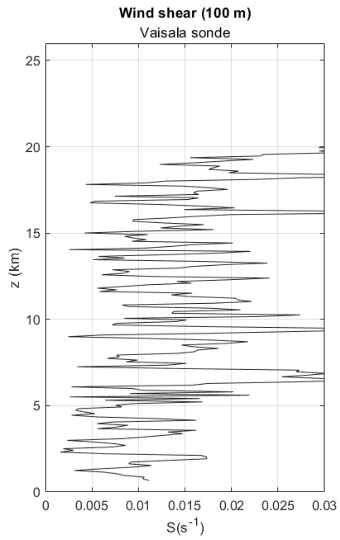
General comments:

The authors' reply clarified points that helped me to better understand what had been done. The amount of work involved is indisputable, but some questions remain as to the reliability and interpretation of some results. Also, at this level of evaluation, more attention to form would have been appreciated, because there are many avoidable presentation errors. This gives the impression that the manuscript has been hastily revised and not proof-read. For example, there are many typos in the added text, and, e.g., in equation (2). Figure 9 caption does not correspond to the new version of the figure.

The manuscript has been improved but, from my point of view, still requires major revisions before publication.

1) The wind profiles measured from the onboard GPS during the ascent (now shown in figure 8) are *very important added value*s and provide convincing arguments for the quality of wind profiles measured by Hydron. They provide much more important information than NWS radiosondes (low resolution and large distance). However, they are only used for wind profile comparisons in Figure 8 and the comparison results are not mentioned in the abstract and conclusions. These additional GPS profiles should provide a good reference and can also be used to estimate wind shear at a vertical resolution of ~100 and ~1500 or 2500 m, since they are not affected by the (large) horizontal excursion as the Hydron. The wind comparison result is particularly good above 10 km for flight 3, with wind fluctuations of similar vertical wavelengths and amplitudes. Contrary to the authors' claims, this seems to suggest that the large horizontal excursion of the Hydron (and horizontal inhomogeneity) may not be the dominant factor explaining the shear (and likely N2) fluctuations at the vertical sampling of 100 m from time series. Also, the shear profiles in Figure 9 obtained at the vertical resolution of 100 m and supposed to be strongly "influenced" by the horizontal excursion (~1500 m) are quite typical of shear profiles estimated from balloon-borne radiosondes at a vertical resolution of 100 m (after applying a low-pass filter with a cut-off at 200 m on 10-m vertical resolution profiles). See the example below from a Vaisala radiosonde. This is another reason why I remain

skeptical about the interpretation of the results (lines 448-449) shown in figures 9 and 10, in terms of horizontal inhomogeneity.



In addition, if the effective vertical resolutions of Ri_t and Ri_z (and shear) are 100 m and 2500 m, respectively, the large difference between the two (figure 10) can be due to the (vertical) scale dependence of Ri (and shear) (*See: Balsley, B. B., Svensson, G., and Tjernström, M.: On the scale dependence of the gradient Richardson number in the residual layer, Bound.-Lay. Meteorol., 127, 57–72, 2008.*). Therefore, the difference in fluctuations at the two resolutions cannot be attributed to the (sole) horizontal inhomogeneity.

It turns out that I do not agree with the statement lines 481-485 because the authors refer to the estimates of Ri_z, always >>1 due to the poor vertical resolution (~2500 m). It is not adapted for the detection of shear instabilities commonly observed in the tropo-stratosphere (~a few hundred meter deep or less).

The fact that the vertical sampling is the same for the two profiles (Figure 9-10) is misleading: it gives the impression that the resolution is the same. The effective resolution should be given by using a clear terminology. For example: Ri_{100} and Ri_{2500}. It should be used everywhere. Line 648 ("… with high Ri….") is ambiguous because the calculated Ri's cannot be directly compared to the thresholds (0.25 or 1).

Lines 410-415: It is not clear what the authors mean. The larger vertical wavelength of the fluctuations should be favorable to larger horizontal scale, and thus to horizontal homogeneity. As a result, the aircraft's orbital trajectory should be less problematic under such conditions. In addition, the argument is weak: the wind profile during flight 2 shows larger amplitude fluctuations during the balloon measurements (ascent) between 5 and 8 km than during the Hydron measurements (descent).

In line 409, do the authors mean "…that are NOT evident in the NWS soundings"?

2) N is shown instead of N^2 in figure 9. The profiles contain negative values, which is not possible, because N is necessarily real positive (when N^2>0) or imaginary (when N^2<0). Can the authors explain how they calculated N in practice?

Lines 483-484 are not clear.

Line 505: "….caused by *inertial* turbulence".

Line 542: n must be equal to -5/3.

In Figure 12, the red lines should be limited to k_l>0.1 because it was the threshold used for linear fitting.

Line 595: "… nearly constant with α" (?)

In the conclusion section, Lines 646-649 comment on comparisons between Ri and EDR (the former figure 13), which are no longer described. Qualitative comparisons can only be made from Figures 14-15 and 16.