Second 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 have thoroughly revised their manuscript but there are still many shortcomings. Some additional information is unclear and not convincing. I think that the revised version cannot be published in its current form. In this second review, I focus on the main issues only.

(1) As outlined in the first review, the manuscript hesitates between the evaluation of a new technique and a scientific work on atmospheric processes, which makes the paper very unclear. The primary objective of the paper should be to provide all the information needed to give the reader a clear idea of the platform's strengths and weaknesses. It is not the case. For example, section 3.1 shows the comparisons between temperature, humidity and wind profiles obtained by Hidron and radiosondes. The differences and similarities between the two should be emphasized. Paragraph 4, page 14 (lines 354-364) is out of the topic, because it describes the temperature structure of the troposphere and stratosphere without considering the differences/consistencies between the UAV and radiosonde profiles. In addition, it is not clear why this description is useful, since it is not used afterwards.

(2) The description of Figure 7, page 17, focuses on the main trend of the wind profiles and on the presence of the jet-stream during flight 1, detected by both instruments. However, the authors do not discuss the “high-frequency” <U> fluctuations revealed by Hydron measurements, even though they are the most striking pattern of the three profiles (these fluctuations do not appear in the radiosonde data but their vertical resolution may be insufficient). Are they real or artefacts due to the helical pattern of the Hydron path, for example? Can they result from inertia gravity waves? Is a spectral analysis consistent with IGW? These questions, not addressed by the authors, are of prime importance, as their answer obviously conditions the quality of shear and Richardson number estimates.

(3) The newly introduced paragraph on stability conditions is unclear and not convincing. First, the text does not describe the red profiles (N2). We implicitly understand that they are obtained from the second method (described from line 407). The “noisy and spike” structure of N2 profiles and the large differences between the wind shear estimated from the two methods are suspect. Figure 9 shows Ri profiles for one method only (likely the method corresponding to the red profiles in Figure 8). Unfortunately, they are clearly not physical, as no results in the literature show such a stable atmosphere at a vertical resolution of 100 meters (or even lower). The authors should check the literature and their calculation methods.

(4) The expression of ε (14), line 492 does not seem to be correct because it is equivalent to write $E_{ll}(k_l) = A$. As a consequence (?), there are inconsistencies in Figure 10. There are no substantial differences between TKE measured during flights 1, 2 and 3 (e.g. $\sim 0.1\ m^2 s^{-2}$ around 20 km). However, EDR(flight 1) is about ~5 times EDR(flight 2 and 3), i.e. ε(flight 1) is about $\sim 10^2$ larger. This is not consistent with TKE. The EDR profiles show an “envelope” that should correspond to the instrumental noise floor (apparently confirmed by a spectral slope close to 0 in figure 14e). The noise level was likely much more important during flight 1. During the present jet-stream, flight 1 should be more prone to turbulence, whereas Fig 10d seems to indicate the virtual absence of inertial turbulence. It is counter-intuitive and then suspect.

(5) The analysis in section 3.5 has been thoroughly revised, and considering the strong reduction of the resolution, this representation may show interesting trends. However, the description of the results are very difficult to follow. In addition, the results depend on the validity of the estimates of N^2, S^2 Ri and EDR described in the previous sections (e.g. the sudden “jump of S^2 above ~15 km is suspect). The authors conclude that all scenarios can occur (e.g. high turbulence with high Ri, low turbulence with high shear, etc.) but I believe that the reliability of the quantitative results is not high.