Peer-Review of “Detection of Turbulence from Temperature, Pressure and Position Measurements Under Superpressure Balloons."

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1 Summary of the Content

The manuscript of Wilson and coauthors establishes a new way of assessing whether a superpressure balloon flew through calm or turbulent air. In short, this is done by comparing the balloon's vertical oscillations around its equilibrium height to the vertical temperature gradient. If there is a non-positive correlation between altitude and potential temperature increment, this is seen as the result of turbulent mixing. Hence, the turbulence flag is set *TRUE*. Furthermore, this approach is compared to the classical Richardson criterion, where the flow is expected to be turbulent for $Ri < 0.25$. Both methods are used on several superpressure balloon flights from the Strateole-2 CO campaign, each method using two slightly different computational methods (different correlation estimators and different linear fitting methods).

2 Overall Feedback

First I would like to thank the authors for sharing their method. Especially, I liked their approach of explaining their ideas in simple terms the first place, followed by a rigorous mathematical description thereafter. As far as my knowledge goes, the method is new and I really appreciate having another tool to retrieve turbulence from standard instrumentation on a superpressure balloon, even though the strength of turbulence cannot be quantified. Furthermore, I really like the careful consideration of instrumental noise in the approach. Their referencing in general is appropriate. Their use of the English language is easy to understand and correct as far as I can judge. The only comments I have on this are listed in Section 4 and Section 5 of this review.

Even though I very much like the general approach of the method and presentation, I think the manuscript will benefit from a *Discussion* section after ”Results“ and before ”Summary and Concluding Remarks“. This could especially enhance the discussion of the new correlation method in comparison with the more established Richardson method. I guess that this will foster confidence in the results from the new method, as it cannot be compared to a quantitative turbulence retrieval using the given data set.

Overall, I think that the approach shown by the authors is a very valuable contribution to the field of turbulence retrievals. It is well presented and absolutely fitting the scope of AMT. I recommend publication after minor revision. In Section 3 I list some points that according to my perception would complement the work of the authors especially in terms of discussing their results.

3 Main Comments

3.1 Similarity of results from correlation method and Richardson method

In ll. 390-391 you state that "The percentage of similar turbulence detection with these four indices ranges from 97.9% (r_S vs. Ri_{LSF}) to 99.07% (Ri_{LSF} vs. Ri_{TSF}). The different turbulent indices are therefore very consistent." Unfortunately, I do not immediately see this conclusion. If we for example consider the correlation method using r_S vs. the Richardson method using Ri_{TSF}, Table 6 reveals 98.59 % of identical turbulence detections for flight 02_STR2. Table 5, however, states that only 4.4 % (3.7 %) of the respective flight was found to be turbulent. *98.59 % of identical turbulence detections* sound like a high number in the first place, but it could still mean that up to one third of the turbulence detections using one method are not seen using the other.

Could you maybe discuss this a little further? In my opinion it might be helpful to use the Richardson method (Ri_{TSF}) as the "established method" and then compare how specific and how sensitive the new correlation method is (e.g. r_S). You could even use a bar diagram, stating "correct positive", "correct negative", "false positive" and "false negative". Does that work for you?

3.2 High occurrence rate of negative Richardson numbers

I was pretty surprised by your manuscript finding a super adiabatic temperature gradient in 80 % of all turbulent cases (l. 367), corresponding to a high occurrence rate of negative Richardson numbers for turbulent cases during flight 2 (Fig. 13). However, in Figure 11 the probability density for $N^2 \leq 0$ is given as zero. If Ri is calculated from the equation in line 213 in both cases on a 1 hr average, I do not fully understand how both results can occur at the same time. But maybe this is a misunderstanding on my side.

According to my knowledge, most of the turbulence is described to occur for $N^2 > 0$ in the literature (e.g. Ko et al., 2019).

As said above, this may well be a misunderstanding on my side. If on the other hand side most of the turbulence in your measurements occurs for $Ri \leq 0$ indeed, could you maybe bring that up in the discussion? I would find that very interesting.

3.3 Possible influences by warm downwash from the balloon

On ll. 139-143 you state that flight 03_TTL3 shows a higher noise influence due to the temperature sensor being located closer to other parts of the gondola. Given the amplitude of the vertical balloon oscillation of up to 100 m (l. 193), the oscillation could also bring the sensors into the much larger wake of the balloon. This would then cause longer lasting warm spikes that may not easily be recognized as an instrumental effect. This is well known for ascending balloons (e.g. Gaffen, 1994; Kräuchi et al., 2016; Söder et al., 2019; Tiefenau and Gebbeken, 1989). It might be helpful discuss this possibility in a very short manor and to include a sketch of the flight train so that the reader can more easily assess the situation. As said, maybe this doesn't apply to your measurements, but it might be easier to see with the help of a sketch or similar.

3.4 Nature of turbulence that can be detected by the correlation method

I think that it could be beneficial for the reader to discuss the type of turbulence that can be detected with your knew method a little further in the discussion.
In the introduction, you state by various citations that many turbulence encounters in the equatorial UTLS are driven by KHIs (ll. 38-44). Your correlation method relies on the instrument being immersed in an iso-thermal layer (equal potential temperature). For a standard KHI, this is expected in the center region, but not at the edges, where at the beginning of the turbulence

development also strong viscous dissipation ε takes place. Regarding the evolution of KHIs I find Fritts et al. (2003) and Werne and Fritts (1999) very valuable contributions, one of which you already cited. However, this point is not a *must* from my perspective, but rather a *nice to have*.

4 Minor Comments

ll. 112-113: it could become more clear by saying "allows to obtain vertical temperature profiles of 2 km length below the balloon" instead of "allows to obtain 2-km long temperature profiles below the balloon"

ll. 118-119: maybe rather "taken into account in order to assess whether" instead of "taken into account to assess that"

l. 237: rather "is one of the few techniques" instead of "is one of the sole technique"

l. 245: rather "expectation that is estimated by" instead of "expectation, is estimated by"

l.456: maybe rather "Yet, they will retain the same sign as the correlation coefficients" instead of "Yet, they will remain of the same sign than the correlation coefficients"

5 Typos

l. 16: probably "true *or* false" instead of 'true of false"

l. 34: "radars" instead of "radar"

l. 102: delete surplus "the"

l. 108: "will for instance stand for" instead of "will for instance stands for"

l. 124: either "signal spectra exhibit" or "signal spectrum exhibits"

l. 148: "night/day" instead of "nigh/day"

l. 299: should be "for each *of* the eight flights"

l. 350: "red" should be "orange"

l. 361: missing full-stop after "Figure 10"

l. 445: "differentiations" instead of "differentiation"

References

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