Atmospheric Measurement Techniques Discussions

Atmos. Meas. Tech. Discuss., referee comment RC3
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Comment on amt-2022-178

Anonymous Referee #3

Referee comment on "Detection of turbulence occurrences from temperature, pressure, and position measurements under superpressure balloons" by Richard Wilson et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2022-178-RC3, 2022

The authors analyze superpressure balloon (SPB) measurements to detect the occurrence of layers with near-zero vertical gradients of potential temperature and with low Richardson number values Ri. They assume that any layer with near neutral stratification or with Ri < 0.25 must be turbulent. In particular they assume that low values of stratification and Ri are consequences of turbulence. However, the paper does not measure turbulence.

The methods are rather demanding because one cannot measure at various heights with one balloon at the same time. Still, the evidence is consistent in identifying layers of weak stratification, liklely without turbulence.

The paper is rather detailed, full of technical discussions, partly difficult to follow for an outsider, but appears to be technically sound. Certainly, it is interesting to interpret the balloon measurements as the author do.

Also, the authors discuss the oscillation periods of the SPB oscillation, and explain the reason for this difference, which I found interesting.

For these reasons, the paper should be published.

The title of the paper is misleading. The paper does not present turbulence data. Perhaps this can be improved. A more correct title would be: "Detection of Conditions for Turbulence from …" - or similar.

Moreover, I suggest more discussions of open issues:

I miss any other evidence for turbulence. I would assume that some high frequency variability should become observable if and only if the ambient air is turbulent. Without such evidence, the paper remains a bit speculative. Please state explicitly whether you have such additional evidence or not and, if not, how large is the remaining risk of misinterpretation of the data?

Also, I miss a relationship between the places and times where you find turbulence and the large-scale synoptic conditions or in relation to deep convection or frontal motions in the troposphere, which you might identify, e.g., from NWP analysis or satellite pictures.

As mentioned above, the authors assume that any layer with near neutral stratification or with Ri < 0.25 must be turbulent. In particular they assume that low values of stratification and Ri are consequences of turbulence (see lines 205 ff). However, the authors do not discuss the opposite relationship: any other dynamical process deforms the atmosphere such that neutral stratification and low Ri are reached in a certain domain. Then turbulence starts as a consequence because now the conditions for turbulence are given. How can you exclude this sequence of processes?

The article starts with the postulate that turbulence contributes to vertical transport in addition to the upwelling Brewer-Dobson circulation. However, the later paper does not discuss any implications of the present findings on the occurrence of domains with low stratification (potentially turbulence) in relation to this introductory question. Perhaps you can return to this initial question in the final discussions or start with or add another motivation to perform this kind of turbulence research?

What does it imply when the atmosphere is turbulent to 5 % of all time. Would you have expected a larger or smaller fraction or is this fraction simply unknown?

Line 60. Certainly, the literature includes more studies on turbulence with research aircraft than discussed in the paper by one of the co-authors: Podglajen et al. (2017). Please extend the literature review.

Table 1: I recommend to identify the flights with TSEN data in this table.

Line 115: I do not understand how you improve resolution by degrading the data. Please explain.

Line 152: “almost inextensible” – can you quantify this statement?

Fig., 8: I do not understand the big polygon with light red line left of -10 K/km stratification reaching from low to high probability. How was this polygon constructed? Also, I cannot see a green line (as stated in Line 350) in this diagram. I was unable to understand this discussion therefore. Please improve.

Line 395: The conclusion starts with a sentence containing the word “mainly” – that raises the question of what else is the purpose of this paper. Please explain or change.

Line 404: The fact that you cannot see high-frequency fluctuations does cause some concerns to me. What could be done in future to improve on this important missing evidence?

Line 424: Here, finally, you mention overflights of the maritime continent. I miss a discussion of overflights of certain domains inside the paper. Since you know the balloon’s position, it should be easy to generate a map of the occurrence of neutral (potentially turbulent) layers as function of geographical position and season. I suggest that you do that before this paper gets published.

Finally: This is an interesting paper.