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Interactive comment on "On the short-term variability of turbulence and temperature in the winter mesosphere" *by* Gerald A. Lehmacher et al.

Anonymous Referee #2

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The authors present an in-depth analysis of a series of measurements taken with four sounding rockets launched in close temporal and spatial proximity. Two sounding rockets carried ionization gauges to measure neutral density profiles (from which temperatures can be inferred) and small scale neutral density fluctuations from which the turbulent energy dissipation rate was determined. The other two sounding rockets carried TMA payloads for measuring the wind field at high spatial resolution. The in-situ measurements were accompanied by ground based sodium lidar measurements from which temperature profiles were derived.

This is a unique set of measurement that allows a first experimental insight into the spatial and temporal variability of turbulence in the mesosphere. While numerical simulations have advanced to the point of making predictions of the likely morphology

and temporal variability of small scale flows in the mesosphere, the corresponding experimental data base is extremely rare - if not absent. As such, this is an important contribution to the literature that certainly warrants publication after some improvements have been made. As such my recommendation is to accept this manuscript for publication subject to minor revisions.

Comments:

- When formulating the scientific objective of their study, the authors state that they intend to "provide a detailed discussion on the static stability and turbulence structure for each profile following the methods developed for neutral density measurements in the mesosphere and lower thermosphere". With all due respect, this is not a scientific objective! Please formulate the scientific objective properly (spatial and temporal variability)and then also come back to this in the conclusions and abstract of this article.

- Page 2, line 4: I suggest to add a reference to the paper by Lübken and von Zahn, JGR 1991.

- Page 2, line 7/8: the general increase of energy dissipation and eddy diffusion with what? with altitude?

- Page 5, lines 31-33: I haven't understood how this works; the thesis of Triplett is not available to me. Please explain in more detail - or delete, since it doesn't matter.

- Page 6, line 6: Typical inner scales... Add: in the mesosphere and lower thermosphere

- Page 7, line 20: The first half sentence sounds rather poetic, maybe re-formulate in a more scientific style.

- Page 7, line 23: "The thermosphere is unusually structured" - compared to which other measurements/data base. Please provide reference.

- Page 7, line 27: Please indicate the distance between the SABER tangent points and

the in-situ measurements

- Page 7, line 29: I suggest to move the internet source of the SABER data to the "data availability" section in after the main manuscript text. See instructions of the publisher.

- Page 8, line 8: Please also show the lidar temperature profile.

- Page 8, line 13/14: I do not think that this is an appropriate way to estimate the absolute temperature error. It does describe the difference between two measurements - OK. But one is a nightly mean and the other a snapshot. I recommend to remove this from the figures and just add a general sentence about the difference between the different measurement (lidar, in situ).

- Page 8, line 17/18: The wording is sloppy here: Nˆ2=0 corresponds to an adiabatic lapse rate but is not the same. Also, the value for stable conditions is completely arbitrary. Please reformulate.

- Page 9, lines 30-31: Is it possible to summarize the observed morphology in a schematic drawing? This will maybe also make it easier to compare to simulations by Fritts et al. and extract the scientific content of this study.

- Page 10, line 9: Wouldn't it be more instructive to first remove the shear or large scale background and then show the hodographs? This would make it easier for the reader to recognize wave features.

- Page 10, line 13: Here and in a few other places the authors compare their findings to results from an earlier rocket flight. However, they leave the reader with the question what they should learn from the comparison. I suggest to either discuss this comparison in more depth and draw a conclusion or to delete it.

- Page 10, line 15: The Richardson number has been used as an index for instability already since the work by Miles and Howard, both in 1961:

Miles, J.W. On the stability of heterogeneous shear flows. J. Fluid Mech. 10, 496–508,

1961.

Howard, L.N. Note on a paper of John W. Miles. J. Fluid Mech. 10, 509– 512, 1961.

- Page 10, line 15: Well, but the epsilon-measurements only give a "zero measurement" in the altitude range of overlap. This should be acknowledged.

- Page 10, line 19-23: At some point the authors should clearly say that they have no coincident measurements of epsilon and the other parameters discussed here (at least no values different from their detection threshold).

- Page 10, line 34: Please explain why. What should be driving the convection here?

- Page 11, line 12: Can this be formulated in a more quantitative manner? What kind of impact? How large?

- Page 12, line 26: "not or not breaking" - something is missing here.

- Page 12, lines 27 - 30: When mentioning the results of Achatz (2007): what is the conclusion for the current work beyond mentioning that these theoretical results exist?

- Page 13, line 1: Do you really mean deep convection? Wouldn't you then need to present vertical velocities? I suggest to reformulate this.

- Page 13, last sentence: either delete or be more specific. Otherwise the statement is trivial.

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