Review of:
High-resolution temperature profiling in the Π Chamber: variability of statistical properties of temperature fluctuations
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Summary

The authors present results from high-frequency (2 kHz) temperature measurements in the Michigan Technological University Pi chamber using an ultra-fast thermometer (UFT). They collect data at multiple heights throughout the depth of the chamber, and use the UFT data to analyze the temperature standard deviation and skewness and spectral properties of the temperature fluctuations. Overall this is an interesting manuscript and one that is well-suited for *Atmospheric Measurement Techniques*. However, I do have some comments I wish to have the authors address before publication, mainly dealing with clarity of presentation and some of the spectral analysis presented in the manuscript.

General Comments

1. **Review of previous work in RBC, l. 44 and following** I appreciate that the authors review the previous literature, but I found this section somewhat difficult to follow. Discussion of aspect ratio effects is interspersed with discussion of spectral scaling and Taylor's hypothesis. You may wish to consider revising this section so it is more clear to readers.

2. **ll. 185–200** Discussion of the role of the large-scale circulation and relationship to temperature skewness. I agree with this discussion in principle, but were these properties of the LSC measured in the present study? It just seems a bit more speculative. It may be a good idea to add some aadditional citations here with respect to the LSC and to make it clear to readers what was measured in the present study and which conclusions you are drawing based on previous work.

3. **l. 239 ff, discussion of spectral slopes** This is a very interesting part of the article, but I think the results get a bit buried in the discussion. Can the authors include a more explicit discussion of the predicted scalings for different regions of the spectrum from different sources? It's not completely clear from the text what the expect scalings are in different regions of the spectrum (or even whether predictions exist).

Specific Comments

1. **l. 82** What does kS s^{-1} stand for here?

2. **l. 99 ff.** Discusssion of sampling period. "This variability stemmed from the LSC period ...and the uncertainty surrounding whether different turbulence properties might be observed for shorter time segments." I am not sure I understand from this passage why a variable measurement period was used. 3 minutes would only be about 2.5 large eddy turnover times. Is this enough to converge statistics?

3. **Fig. 4** "Top panel (a) corresponds to full vertical scan of the cell." I had to read the caption a couple of times before I understood that this single timeseries corresponds to different measurement heights. You may wish to revise so this is more clear to readers.

4. **l. 179** I'm not sure I like the notation μ'_T for the skewness. μ makes me think of the mean.

5. **l. 227 and Fig. 8** Why premultiply by f^2 (rather than f or $f^{5/3}$ for instance)? This is not very clear from the text.

6. **l. 236 ff.** Discussion of BO and OC scaling. Have these acronyms been defined? I am not sure what you are referring to.

7. **Fig. 8 and discussion** Have the authors looked at the power spectral density pre-multiplied by f to these different exponents? It may be interesting, for example to look at $f^{5/3}P(f)/P(f_p)$ or $f^{7/5}P(f)/P(f_p)$.

8. **l. 284** Spectral slope of -7 in dissipative range. Is it expected to have a power law in the dissipative range? The model spectra presented in Pope's textbook on *Turbulent Flows* (2000) includes exponential decay in the dissipative range. Granted this is for the energy spectrum rather than the scalar spectrum, but it may be beneficial to discuss this point further.

9. **Fig. 9, fitted slopes** Can the authors comment on how much uncertainty is present here in the fitted slopes? For momentum, if a fitted spectral slope did not correspond to $-5/3$ in the inertial range, I would suspect that the data are too noisy to estimate the slope accurately. Related to this, you may wish to include additional detail regarding how these spectral slopes were estimated.

10. **l. 330** Applicability of results to atmospheric surface layer. I don't disagree with this statement, but I will note that the atmospheric surface layer is typically shear-dominated, so there may not be a direct translation of the present results based on RBC data.

Technical Corrections

1. **l. 39** This is not a complete sentence.

2. **l. 56** Do you mean underlying, rather than underling?

3. **l. 277 ff.** This sentence does not read well; consider revising.

4. **Sec. 4, bullet point headings** These should be capitalized, e.g. "Basic Characteristics," "Topographic Effects," etc.