Referee #1 reponses

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.

Following the advice, we reorganized this part to be more consistent and not to confuse the 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 additional 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.

Our measurements of the LSC focused on the oscillation periods as a function of the applied temperature differences. The resulting power law aligns with previously measured periods for $T = 12\ ^{\circ}\mathrm{C}$. Details regarding these measurements are provided in the abstract and in Section 3.2, where we present the PSD results. The subsequent discussion on skewness properties represents our attempt to propose a potential mechanism responsible for the observed effects. This section has been refined to improve clarity, and we have incorporated citations to relevant works to better contextualize our findings.

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).

We revised this part providing the information of slopes predictions, clarifying the discussion of our results and improving the argumentation based on other works.

Specific Comments

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

It stands for kilosamples per second. This is explained now in the text.

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?

Our objective was to investigate the small-scale variability of the scalar field, which plays a critical role in microphysical processes within clouds. The use of variable measurement periods was motivated by several factors: the need to capture scalar profiles with fine spatial resolution to better understand the physics at different levels of the chamber, the high temporal resolution of the measurements (2 kHz), which provided robust statistics even for shorter durations, and the limited availability of the Pi Chamber, which constrained the collection of longer time series.

Figure 5 demonstrates that both longer and shorter time series exhibit similar overall behavior. However, as shown in Fig. 6 and discussed subsequently, shorter time series reveal probable unaveraged thermal structures that are not apparent in longer measurements, emphasizing features that would otherwise be smoothed out.

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.

We revised the caption to improve readability.

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

We changed the notation ($\mu \rightarrow \gamma$)

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.

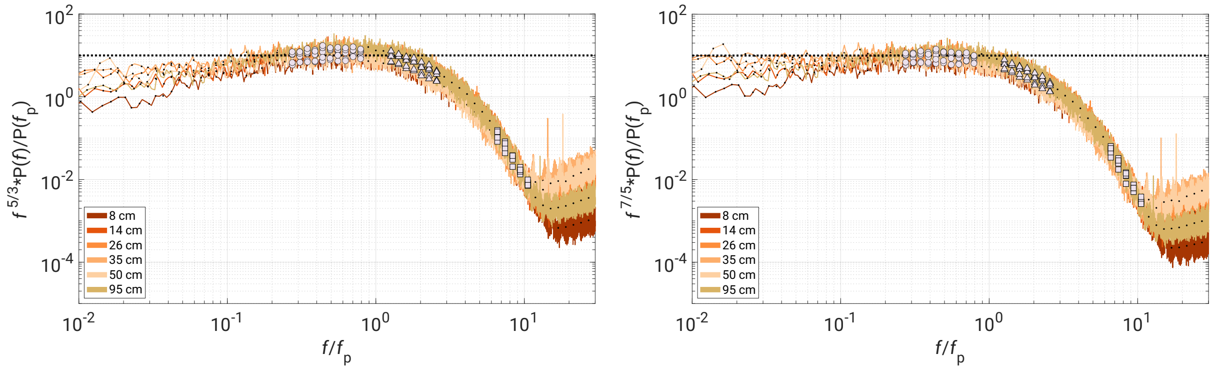
We explain the methodology used starting from line 235, noting that it was originally proposed by Zhou and Xia (2001). The cited paper provides a more comprehensive discussion of this approach. Using this method, we were able to identify the peak frequencies around which the PSDs converge to universal functions.

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

In line 76 there is an explanation of the used abbreviations. Both denote different scalings–OC stands for Obukhov-Corrsin whereas BO is Bolgiano-Obukhov.

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 $f^{7/5}P(f)/P(f_p)$.

The normalized spectra mentioned are presented below, corresponding to the case of $\Delta T = 20$ K, as described in the manuscript. The graphs show results consistent with the prior discussion: the inertial range is slightly better characterized by the −7/5 scaling. However, due to the minimal difference between −5/3 and −7/5, it is challenging to definitively conclude which scaling is more representative. The root mean square errors for this range are addressed in point 9.



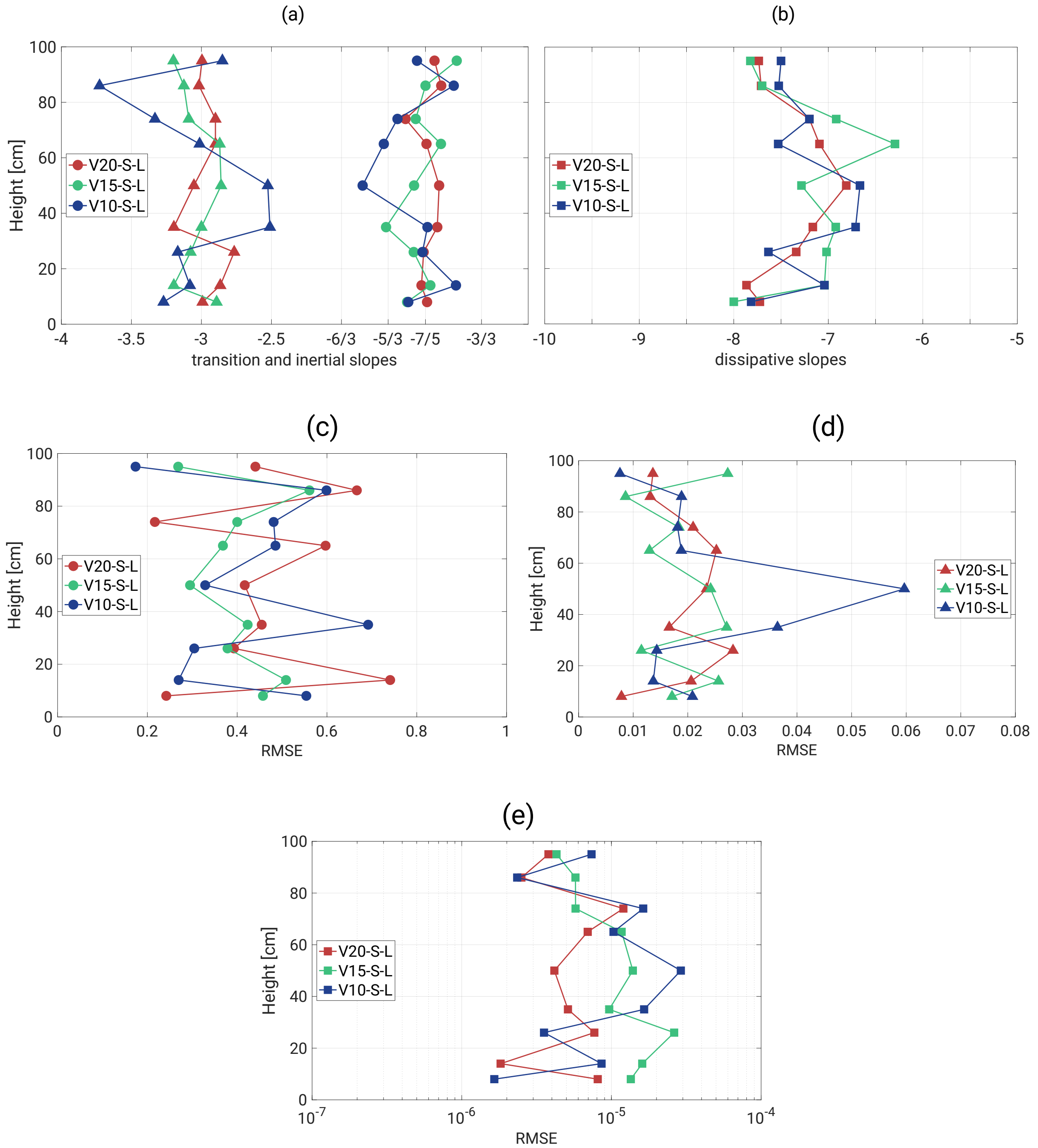
8. l. 284 Spectral slope of -7 in dissipative range. Is it expected to have a power law in the dissipative range? The model spectra presented in Pope's textbook on *Turbulent Flows* (2000) includes exponential decay in the dissipative range. Granted this is for the energy spectrum rather than the scalar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

A comprehensive discussion on scalar spectra with respect to Schmidt number (Sc) can be found in Sreenivasan, K. R., *Proceedings of the National Academy of Sciences*, **116**(37), 18175–18183, doi:10.1073/pnas.1800463115 (2019). For the case of Sc ≈ 1, what can be translated to magnitude on the order of unity of the Prandtl number, there is no theory determining scalar field in the dissipative range. Since temperature acts as active scalar the resulting spectra can differ from the respected energy spectrum. In case of spectra we obtained, the slopes were estimated as power law functions which fitted well. More on this we included in the next point as well as in the extended discussion in the manuscript.

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.

The methodology of slopes estimation is outlined in the manuscript starting from the line 241: “To estimate the slopes, we employed a methodology outlined in Siebert et al. (2006) and Nowak et al. (2021), averaging raw spectra over equidistant logarithmic frequency bins (twenty bins per decade in our case) and fitting using power law functions. Additionally, to assess the linearity of the slopes in log-log coordinates, we computed the Pearson correlation coefficient p for the resampled points.”



The equidistant logarithmic bins are presented as black points in Fig. 8, then we selected segments of points (marked as circles, triangles and squares in Fig. 8b) with the highest Pearson coefficients for the best power law fits. The resulting slopes are then presented in Fig. 9 (we attached both panels as (a) and (b) graphs above). To show the uncertainties of the fitted slopes we exported root mean squared errors (RMSE) in graphs above: inertial range (c), transition range (d), and dissipative range (e). The largest RMSE are indeed observable for the inertial range which is why we cannot make a definite conclusion of which scaling is better represented in our results. The following ranges are characterized by small (d) and very small (e) uncertainties which support the idea of power law dependence in dissipative range for scalar field.

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.

We agree with the referee that the atmospheric boundary layer represents very complex physics and shear effects are significant. This is why we merely outlined the possible connections between the real atmosphere and our results as thermal structures play an important role in mixing processes. Nevertheless, we think that application of the UFT thermometers in the atmospheric surface layer could give a lot of interesting information about its thermal structure.

Technical Corrections

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

The sentence was improved.

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

Unfortunately, we do not find neither a word "underling" nor "underlying" in our work.

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

The sentence was improved.

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

We improved the bullet point headings following the referee's advice.