Response to reviewer 2

Dear Anonymous Reviewer 2,

we highly appreciate your feedback. It helped us to improve the manuscript. Below we comment on your suggestions in detail.
All reviewer comments appear in *italic text* below, while authors' responses appear in blue text. Line numbers referenced in the authors' responses refer to the revised document.

This is likely to be a valuable addition to the data corpus of environmental turbulence measurements. I have two general recommendations, one superficial the second of more concern.

General comments

(1) Minor. The Introduction and in part the summary both refer to the field campaign being part of LAPSE-RATE which was focused on remotely piloted aircraft (RPA). The implication is that paper uses data from instruments on RPA platfroms. As far as I can see, this may be a later intention, but for this paper it is not relevant.

Thank you for this suggestion. However, we believe including information about LAPSE-RATE is highly important since the data collection took place within this campaign and this dataset can then be used for comparisons with data from RPA platforms in the future.

(2). Major. The paper describes and compares two lidar systems, both used to estimate turbulent dissipation. A reader coming to this paper and data set would wish to know (a) Are the instrument systems actually fit for purpose to do this, and (b) are these data useful. This is not possible to judge because there is no indication of error analysis or displays of confidence limits or other typical presentations when measurement sets (whether instrument or model output) are compared.

We carefully considered this suggestion and included a new section dedicated to uncertainty analysis of our calculations (Section 5 in revised version of manuscript). We used the law of combination of errors to evaluate how random errors propagate through our calculations. We also compare the uncertainty in ε retrievals with the corresponding values of turbulence dissipation rate for each lidar.

Figures 2 and 3 are noteworth here: as far as I can see, figure 2 is smoothing of a noisy curve (using limted splines), whist figure 3 is fitting a Butterworth-style transfer with pre- defined cutoff (-5/3). There is no knowledge gained from these. I recommend some estimate (with error) of say the displation decay, and whether it agrees or not with Kolmagorov. Only when we have these statistical results can the quality and benefit of these data and methods be assessed by the reader.

Thank you for your thoughtful comment. Building on your comment, we modified the method used to calculate the sample size from the energy spectrum for each 15-minute time period. We decided to do a power fit to the data at increasingly larger frequencies and then compared our results with the Kolmogorov $f^{-5/3}$ law. We then determined the transition frequency for each spectrum from the closest agreement between our power fits and Kolmogorov. Using this method, not only are we forcing agreement with Kolmogorov, but also our results agree with larger transition frequencies during nighttime and smaller transition frequencies during daytime.