1 Response to Reviewer #2

We would like to thank Reviewer #2 for the comments and suggestions to our manuscript. We addressed all the comments, provided additional explanations and analyses, as requested by the Referee. Please note that the comments by the Referees are presented in blue color and with the italic font, whereas our responses are written in black and with the sans-serif font.

1. *This is a well-written manuscript containing thought-provoking results. However, there are several fundamental issues which need to be addressed before the paper can be accepted.*

   We would like to thank the Referee for the positive overall judgement of the manuscript, we tried our best to address the fundamental issues mentioned by the Referee and improve the manuscript accordingly.

2. *The authors borrowed newly developed non-equilibrium theories from turbulence literature and applied them to boundary layer turbulence. In the original theoretical development, the buoyancy effects are not included. So, the authors neglected the effects of stratification altogether (see their comment on page 11). In a transitional boundary layer, the impacts of atmospheric stability cannot be neglected. In the revised manuscript, some efforts must be made to include the effects of stability in the derivations [e.g., Eq. (8)].*

   This comment addresses the buoyancy effects which were not included in the theoretical developments in the original manuscript. In the revised manuscript we included the buoyancy B, the turbulent transport T and the shear forcing P in the kinetic energy equation (8).
   We performed additional data analyses and we were able to estimate the buoyancy forcing B from measurements which were performed in parallel, by sonic anemometers placed on a meteorological mast in Rzecin site and at the roof of the building of the Institute of Geophysics in Warsaw.
   The new results shown in new Figure 4 suggest that the buoyancy forcing was still positive at time $t = -2h$, i.e. two hours before the sunset, but short after it became negative in Rzecin and very close to zero in Warsaw. Nadeau et al. (2021) defined this moment as the beginning of the evening transition, where turbulence start to decay more rapidly than in the afternoon. Since our focus is on times $t > -2h$, which correspond to the evening transition, we neglected the buoyancy forcing in further theoretical analyses in Section 2.2. We were not able to estimate the turbulent complete transport term T, but we assumed that it also becomes small in the absence of forcing. The shear forcing P is expected to play a role only relatively close to the Earth surface, hence both T and P were neglected and we assumed that to the leading order the decay of turbulence kinetic energy is described by Eq. (10). In the revised manuscript we added discussions in Section 2.2, lines 156-160.

3. *The authors briefly mentioned 3rd-order structure functions in the manuscript. I would like to see evidence that the Lidar data conform to the 4/5th law (Karman-Howarth equation) prior to evening transition.*

   To calculate the 3rd-order structure functions and check if the Lidar data conform to the 4/5th law the fluctuations of the longitudinal velocity component (i.e. along the mean wind direction) are needed. The doppler Lidar system measures with the frequency of $1Hz$ only one, radial (i.e. along the beam) component of the wind velocity. The horizontal wind was estimated only every 30 minutes, during the Vertical Azimuth Display (VAD) scans with a constant elevation of 70○ and based on 12 azimuth points. The low frequence of these measurements does not allow to estimate the fluctuations of the horizontal wind component and calculate the 3rd-order structure function.

4. *Give at least a few examples of EDR estimated via second-order and third-order structure functions and compare them against Equations (1) and (2). Please clearly show the structure functions and fitted slopes in the revised manuscript. [Add these materials in Section 5.3].*

   As discussed in the reply to comment 3, we could not calculate the 3rd order structure functions, becaue only thevertical (transverse) velocity component was measured with a sufficient frequency. In the revised version we calculated EDR via the second order structure function at $t = -2h$, when the scaling was still relatively close to the Kolmogorov scaling and show the structure functions, fitted slopes and the profiles of EDR in the new figure 16. At $t > -2$h the slopes become smaller than $2/3$ and estimating EDR based on the Kolmogorov's equilibrium assumptions is not justified, in our opinion.

5. *Elaborate on the (relative) accuracy of longitudinal and vertical velocity estimation from Lidar observations. Give references.*

   In the revised version we added information on the accuracy of velocity estimation from Lidar observations in lines 284-285, and added references to works by Rye and Hardesty (1993) and Pearson et al. (2009). In the revised manuscript we also calculated the standard errors of the mean of all the calculated variables and added error bars on plots.

6. *Do the results hold for the longitudinal velocity component? Why not?*

   As explained in the reply to comment 3, only vertical velocity component was measured with the frequency of $1Hz$. The profiles of the horizontal component of the wind velocity were estimated only every 30 minutes.

7. *The empirical Equation (1) is conventionally used in conjunction with TKE. What is the justification for using it only for the vertical velocity scale? One cannot invoke isotropy here.*

   The Reviewer addresses the assumption of isotropy. Indeed, atmospheric turbulence is isotropic only at relatively small scales. However, with only one, vertical component measured with the sufficient frequencey, we were only able to estimate the vertical velocity scale. We still hope that the derived formulas can describe the scaling of statistics during the dacay of turbulence, however, the constants can be somewhat affected.

8. *Figure 4 (left panel): in the entire convective boundary layer, the inertial-range slope is close to -2. Why? Can we trust the observational data?*

The observed inertial-range slopes of the frequency spectra are indeed close to -2 in the convective boundary layer. In our opinion, the steepening of the spectra is due to instumental issues, in particular due to the finite spatio-temporal resolution of the measurements. The same was argued e.g. by Banakh et al. (2021). The averaging in space and time acts like a low-pass filter, which affects the measured part of the spectra. Additionally, the slopes of the structure functions and the frequency spectra can be affected in different ways. Moreover, the chosen range of scales were the slopes are calculated also affects results. Hence, we wrote in lines 230–232 that we focus on the changes of the scaling in time, rather than on the exact values of the slopes.