Karasewicz et al. have performed an analysis of Doppler lidar measurements of turbulence in the ABL just before and after sunset. They find that frequency spectra of vertical component of the wind deviates from -5/3 law for the inertial subrange, but is consistent with theories of non-equilibrium turbulence. They conclude that lassical models of turbulence decay tend to underestimate the dissipation rate of TKE in initial stages of decay. The study makes use of frequency spectra of vertical fluctuations and structure functions in rural and surface roughness- and heat island- affected urban environments before and after sunset.

Overall, I like this study. The authors have collected a very valuable Doppler lidar data set on turbulence in the ABL around sunset. While much effort has been expended in studying the ABL's evolution from morning till afternoon, there is a dearth of studies focusing on the changes in the ABL around sunset, when convective turbulence transitions to nocturnal turbulence, resulting in a shear-driven ABL near the ground and a residual layer elsewhere. As such, this study is an important addition to the literature.

That said, the manuscript and its impact could be improved significantly if the authors provide a proper context to the interpretation of the results and drawing of the necessary conclusions, by presenting the underlying velocity and length scales. What I mean is that the authors should interpret the results in the context of the prevailing convective velocity scale w^*, the friction velocity u^* and the depth of the ABL D, as well as the height of roughness elements (especially in the urban environment). They should also provide error statistics (confidence intervals/error bars on plotted data points for better interpretation of the underlying secular trends. Detailed comments and suggestions can be found below.

I recommend that the manuscript be sent back for major revisions before acceptance for publication.

Detailed Comments and Suggestions:

Eqs. (9), (13) and (14): Kinematic viscosity coefficient ν is artificially introduced to bring in the Reynolds number *Re*. In reality ν must not appear in either equation, since high Reynolds number (asymptotic) turbulence, molecular viscosity should not appear explicitly except at Kolmogorov viscous scales. Now, if *Re* is identified instead as TURBULENCE Reynolds number, then it is fine. This is because *Re* is usually associated with mean flow velocity and the size of the turbulent layer.

How do we know *Re* decreases during decay? Certainly *U* decreases with time, but *L* can increase or decrease, so that *U***L* may not necessarily decrease. A better justification would be useful.

The central idea of the paper depends on integral scale *L* decreasing with time under non-equilibrium turbulence decay. This must be proven beyond any doubt.

Eqs. (8) and (14) are valid only for isotropic turbulence since the implicit assumption is $u^2 = q^2 / 3$, where q^2 is twice the TKE.

L189-190: I don't understand this. In both Eqs.(13) and (15), time derivative of L^2 is NOT inversely proportional to *Re*. The statement is erroneous?

L255: While heat flux analysis is not central to this study, it is important in order to determine the Deardorff convective velocity scale *w**, since the variance of vertical component of turbulence velocity scales like $(w^*)^2$ under convection (and like friction velocity $u*$ under shear). It is important to know what w^* and the ABL depth D are during the spectrum and structure function measurements. The paper should present their variation with time during the study period.

Figure 3: What is the height of the ABL? Important to know where exactly in the ABL, measurements are being made, Interior, near the top, near the surface layer, inside the surface layer?

L297 and Figure 4: "steeper than Kolmogorov?" Why? Instrumental errors? You mean "red_part of orange and yellow-part of green." Right?

Figure 4: How exactly do you fit the line to compute the slope? I can think of large systematic errors resulting, depending, for example, on the range of frequencies chosen to fit the spectrum in Figure 3.

Figure 4: What is the ABL height as a function of time? That would be useful in interpreting the results. For Warsaw, what is the height of roughness elements (buildings) above the ground? In other words, how close or far away are the measurement points from the ground and from the ABL top?

Figure 4: Extensive regions of steeper slopes during the actively heated, vigorous ABL. Why?

Figure 5: Would be useful to plot also in a third panel, the product of turbulence Velocity scale and integral length scale, since this is proportional to the "Reynolds number *Re*". Better call it turbulence Reynolds number to distinguish it from conventional Reynolds number based on mean velocity. This is important in view of the statements made earlier on how Reynolds number behaves during decay.

Figure 5: Error bars on the black dots (median values) are essential to interpret the variability with time of both scales, especially the length scale. The scatter in the length scale is much larger than in the velocity scale and so the trend indicated is not so easy to interpret in the absence of error bars.

L315-316: This interpretation is not so obvious, in the absence of confidence intervals (error bars).

L320: The cited heights must be put in proper context by specifying the average height of roughness elements and the ABL height.

Figures 5 – 7: Why median values instead of mean values? Perhaps both? Please justify.

Figures 5 – 7: Information on ABL height and roughness elements would be useful in digesting the results.

Figures 5-7: Just before sunset, the buoyancy flux decreases rapidly, because of how the solar radiation decreases toward the end of the heating cycle. This itself can lead to non-equilibrium conditions, even before the heating is cut off at sunset ($t = 0$). This should be mentioned.

Figures 5 – 7: The characteristic time scale in the ABL is the eddy turnover time scale, proportional to D/w^*, where D is the depth of ABL and w^* is the convective (Deardorff) velocity scale. It is useful to know what this time scale is, since with cutoff of heating, one can expect convective turbulence to decay and lead to the formation of a residual layer with shear-driven layer adjacent to the surface, on this time scale.

Figures 5 – 7 and L 327-339: Clearly, the departure from -5/3 (Figure 6) appears to increase with height at Rzecin, whereas Warsaw does not show this tendency. Information on ABL height and height of roughness elements would be useful to understand this.

Figure 8 and L345-350: The mean horizontal velocity appears to be roughly 5 m/s, remarkably uniform with height and measurement location. Anyway, the corresponding friction velocity u^* is likely to be 0.15 – 0.25 m/s (say 0.2 m/s on the average). What are the values of w^* during this time? This is important to know since the ratio w^*/u^* must be high enough to assume convective nature of turbulence. Otherwise, u^* complicates interpretation of the results.

L366: What is the value of H?

Figure 10: Plot of w^* and u^* with time relative to sunset would be useful here, since w^* is likely more relevant before sunset and u^* after. In other words, the underlying scales would be useful in better understanding of the results.

Figure 11: Why choose U_0 and L_0 at -2 hr? The time chosen should be justified in comparison with the eddy turnover time scale D/w^*. Without error bars, it is hard to interpret the secular trend, especially of integral length scale.

Figure 12: This is the "piece de resistance" of this study. While the conclusions to be drawn from these plots are clear, it would help to remember the assumptions and data processing and interpretations that preceded this. Also, the data points should have at least approximate confidence limits (error bars) for completeness.

L378: I don't believe Reynolds number is a relevant quantity in asymptotic turbulence that exists at high enough Reynolds numbers. In Figure 12, Re is just U^*L normalized by a constant value of kinematic viscosity anyway and so it is indicative of U^*L, the quantity proportional to turbulent viscosity. Therefore *Re* is more appropriately turbulence Reynolds number.

Figure 13: These results follow naturally from results in Figure 12 and presumed behavior according to Eqs. (1) and (2). However, the choice of U_0 and L_0 are crucial to the interpretation. Some justification is needed as to their choice at 2 hrs before sunset (see above) in terms of the eddy turnover time scale.

Conclusions: It appears to me that the conclusions must be tempered a bit and justified better. What I mean is this:

1. One can expect a drastic change of turbulence characteristics in the ABL (transition from convective turbulence in the entire ABL to residual layer and shear-driven turbulence near the surface) immediately after sunset, leading to non-equilibrium conditions. This transition is not adequately addressed, since the governing velocity scale changes from w^* to u^* near the surface, and turbulence (non-intermittent) presumably dies down in the residual layer. As such, some discussion of u^* is essential, instead of lumping everything into a "Reynolds number" based interpretation.
2. The results suggest non-equilibrium conditions are present BEFORE sunset, since data at 2 hr before sunset is used as initial conditions. However, the behavior of turbulence between -2 hr and 0 hr depends critically on how w^* decreases during this time. It is

likely that w^* decreases more rapidly during this period than during mid-morning to mid-afternoon conditions. In any case, without knowing w^* and D, it is hard to interpret the results and unconditionally agree with the conclusions.

Minor Points:

L1 & L12: Replace “short” by “shortly”
L20: Typo - should be homogeneous.
L109: Inertial subrange is a more appropriate term.

Eq. (14): $\frac{\nu}{UL}$ must be $\left(\frac{\nu}{UL}\right)^2$