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Atmospheric Measurement Techniques Discussions

Interactive comment on "Estimation of turbulence dissipation rate and its variability from sonic anemometer and wind Doppler lidar during the XPIA field campaign" *by* Nicola Bodini et al.

Anonymous Referee #2

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In this manuscript, a technique to measure turbulence dissipation rate from Doppler lidar observations is presented using data collected from several Doppler lidars during XPIA. The dissipation rates are compared with those from sonic anemometers for verification (and to determine the sample length for the best agreement). Statistics of dissipation are presented for the experiment, which serve as a brief climatology of dissipation at the site.

The manuscript is generally written and organized well, and results in this manuscript are of significant interest to a wide audience in the Doppler lidar and boundary-layer fields. Still, there are some significant omissions in the description of the technique and

how the presented results are interpreted. As such, I recommend that this manuscript be reconsidered for publication after major revisions, after the following concerns have been addressed.

General/major comments:

a) How exactly is the turbulence dissipation calculated using the Doppler lidar data? More details need to be added to Sect. 3.2 so that this technique could be applied by a reader. From the Halo data, it must be the vertical staring observations. From the V1/V2 profiler data, which beam position is used (and why)? Was dissipation calculated from each beam separately, and the mean of those used?

While isotropy is assumed, turbulence is rarely isotropic in the boundary-layer, especially under stable conditions when turbulent eddies are more horizontally oriented. As such, there could be differences (particularly with the Halo which just uses vertical beam) between the lidar estimates and sonic anemometer estimates (which use the horizontal variance alone) from anisotropy. This should be briefly discussed.

b) In Sect. 4, the sampling length for calculation of dissipation during stable, neutral, and unstable conditions is chosen as the minimum of the MAE between the sonic and lidar estimate. This is fine when there is sonic anemometer data for both verification and classifying stability, but most sites that this technique could be applied to will not have coincident sonic measurements. How could this technique be applied to other sites, where the turbulence characteristics/stability might be quite different? This is a major limiting factor in the applicability of this technique, and currently there is no discussion of how this could be applied to other sites given this limitation. Also, does the minimum in the MAE vary between slightly stable and strongly stable conditions, when the inertial subrange may be much smaller? Should the analysis in Fig. 5 be done with more stability classifications (strongly stable/unstable, weakly stable/unstable, neutral)?

Perhaps this technique could be refined so that the sample length varies with the outer scale of the inertial subrange, as determined from the Doppler lidar data alone. Then, the technique could be easily applied to other lidar data. Alternatively, the authors could add a short section (a few paragraphs) on how this technique could be applied at locations without sonic anemometer data for stability and determination of the sample length to use.

Specific comments:

a) p. 2 line 5; p. 21 line 27: Here, the authors make the case that both production and dissipation of TKE need to be known for turbulence closure. The authors state that by measuring dissipation, the scales at which the assumption of local equilibrium are broken will be assessed. However, in order to do this, production must also be measured. The authors should add a few statements on how production of TKE can be measured for the full closure.

b) Figure 1 caption: Would be good to clarify that contours in the right panel are in m.

c) p 3 line 1: Spell out XPIA in full here, for those unfamiliar with the project.

d) p. 4, line 2: Was this sonic also a CSAT3 or was it different? If it was a different type of sonic, are there differences in the design that may cause the observed dissipation to be much higher than for CSAT 3 (possibly more obstructions, if it's an RM Young anemometer) as later discussed in Sect. 5? Given its importance to the results, more details should be provided about this sonic, its siting, and any QC applied to it (was any data thrown out when it was waked by what it was mounted on)?

e) Table 1: Can the pulse width (FWHM) be added as a row to this table, as well? This will be useful in understanding the smallest eddies that can be resolved by a given lidar.

f) p. 5 line 19: How did the measured dissipation rates between the two sonic anemometers compare to each other when both were unwaked? Were they often similar, or were there often substantial differences? This might be useful to form a 'baseline' estimate of how much uncertainty is in any dissipation measurement from

the sonic anemometers themselves.

g) Eq. 5: By using this equation to estimate dissipation, it is implicitly assumed that the line-of-sight atmospheric variance (σw2 in Eq. 8) is strictly the result of turbulent motion. However, non-turbulent motions such as gravity waves in a stable layer may increase the line-of-sight variance but are not turbulent, thus there is little dissipation with them. Under these conditions, turbulence dissipation would be overstated. This may be especially important at the BAO for westerly winds, due to the close presence of mountains to the west that may induce mountain waves when the atmospheric conditions permit. This may affect the statistics later presented in Sect. 5, as dissipation may be overestimated due to the presence of these waves.

h) Eq. 8: In the term σw2 it should be clarified that this is not the true atmospheric variation of the wind, as the smallest scales of turbulence are not resolved by the lidar.

i) p. 8 line 23: Do the line-of-sight velocities need to be de-trended? Since the windows over which the variance is calculated is short (<1 min), the de-trending will effectively remove variance contributions from large eddies, especially during unstable conditions, causing an underestimate of variance (and consequently dissipation).

j) p. 9 line 5: Since the sampling window is so short, measurement uncertainty/representiveness (i.e., Lenschow et al 1994) is a significant factor in the quality/error of the variance measurement as well and should be mentioned.

k) Figure 4: Could vertical lines be added to denote the inertial subrange and/or sample length used?

l) p. 10 line 15: Could an equation be included here for how exactly the metric presented in Fig. 5 is calculated? I assume the error is normalized by some value (as the y-axis is unitless), but this is unclear. Without this information, it is difficult to interpret Fig. 5. The caption for Fig. 5 needs to be clarified accordingly, as well.

m) p. 14 line 33: As SNR typically decreases with range, is it possible that the increase in dissipation above 600 m is due to more noisy/random errors in the line-of-sight measurements above 600 m? Thus, the increase above this height is not real (due to atmospheric turbulence), but instead due to increasing measurement errors.

n) Figure 9: The labels on these plots are small and difficult to read. Could they be made larger?

o) p. 17 line 1: Are there other studies that also confirm the finding here that there is a significant gradient in dissipation right near the ground, but the changes are much smaller above 50 m? What physically results in this large almost order of magnitude change in dissipation from the surface upwards? It would be good to expand on this. Without justification or other studies that show similar results, these results seem a little suspect. Was the 5-m sonic near anything that may obstruct the flow to cause dissipation to be so large?

p) Sect 5.1: This LLJ event is atypical compared to most in the Great Plains, where the LLJ slowly reaches a wind speed maxima in the middle of the night, after which the wind speed slowly decreases. The rapid decrease in wind speed at 03 UTC seems more like there was some other disturbance (possibly on the mesoscale) that resulted in the jet diminishing. Looking at the tower data (https://www.esrl.noaa.gov/psd/technology/bao/browser/), there was also about a 45 degree wind shift at the time the LLJ ended. Based on surface observation maps (http://www2.mmm.ucar.edu/imagearchive1/surface/ict/20150407/sfc_ict_2015040703.gif, http://www2.mmm.ucar.edu/imagearchive1/SatSfcComposite/20150407/sat_sfc_map_2015(, there was a Denver cyclone in the area with an associated quasi-stationary front near the BAO site. Is it possible that the observed increase in dissipation was not from the LLJ itself, but is induced by a front (possibly the quasi-stationary front drifting over the site) or disturbance in the vicinity? Could a different LLJ event be chosen for this analysis? Otherwise, the text must be modified accordingly to make it clear that this observed behavior is not typical for LLJs and the presence of this quasi-stationary front likely plays a role.

Technical corrections:

a) Figure 4 caption: Should be a) after 22:15 UTC.

b) p. 12 line 26: WINDCUBE is misspelled.

References:

Lenschow, D. H., Mann, J., & Kristensen, L. (1994). How long is long enough when measuring fluxes and other turbulence statistics? J. Atmos. Ocean. Tech. Technol., 11, 661–673.

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