In this paper the authors use a wind lidar to derive the terms in the TKE-budget. The two reviewers both had objections to some of the authors text and explanations and the authors responded to these in an adequate fashion. I have now reviewed the paper and I have some major concerns that I want to have answered before finally deciding on this paper.

Major comments

I am a big fan of using remote sensing instruments for this type of study and I think the time-height distributions shown in the last section of the paper illustrates their potential. There are, however, two potential major flaws in the representation of the TKE budget terms, especially relating to the dissipation term and the buoyancy term. Most of the comments below are based on the authors not properly explaining the choice of parameters in their Reynolds averaging for the two different instruments. I would also point out that with one of the termsin the budget equation, the dissipation, being estimated indirectly and another being calculated as a residual, the average budget profiles, like in Figure 10 & 12 must balance to zero; hence, if the dissipation is larger than the shear production, the buoyancy has to pick up the rest to balance the results. Therefore, Figure 12a is puzzling since that doesn't seem to happen here.

Starting with the buoyancy term, this is - contrary to the authors claims - *never measured*; it is estimated as a residual – after ignoring the pressure transport term which in my opinion *is* OK. But the reason to believe in the method comes down to the residual – assumed to be buoyancy – being much larger compared to other leading terms; that the residual cannot be explained in terms of errors in the other terms.

One could have assumed the time tendency term to be small and then proceed to calculate the buoyancy term as a residual, but the authors choose not to do this which is interesting. Instead they calculate the time tendency by taking a 5-second finite time-difference on a signal that appear to have a 20-minute averaging window. So, I can't help wondering what would happen if that time difference was taken over one minute or maybe even 20 minutes. The way this was done means that the magnitude of the residual – which is assumed to be the buoyancy production – could have been substantially smaller if the time difference had been substantially larger. Maybe taking the time difference over another time window has a small effect; maybe one could even assume that the time tendency is negligible.

Either way I would have preferred to: 1) Never say you measure something that is not actually measured, instead use "derive" or estimate"; 2) all terms in the budget be derived over the same time Reynolds averaging time window and; 3) describe carefully the way the turbulence terms are estimated in the Reynolds averaging for both sonic and lidar measurements.

Moving on to the dissipation term, the whole theory behind the suggested way of estimating the dissipation rate relies on the Kolmogorov theory and the existence of an inertial sub-range in the power spectra. Here the authors deviate from this by allowing the exponent to deviate from the expected -5/3 slope; one of two criteria for determining that such a range to exist in their data. They compare the results to observations from the sonic anemometers with good results, but they never discuss how the latter was estimated. Presumably it was estimated using the same type of method since the sonic sampled at 10 Hz cannot actually measure the dissipation directly. However, with an 0.2 Hz sampling rate for the lidar, it seems more likely that deviations from the expected slope that is discussed is due to never actually reaching the inertial sub-range and not from actual deviations of the exponent as explained in the text. Still the comparison to sonic observations, that sampled at 10 Hz, looks good, so why is that? If the estimates from the sonic are done for the same frequencies as for the lidar, maybe both estimates are wrong and that is why they compare so well.

To believe in this as a robust method, we should at least get to see some spectra from both sonic and lidar and time series of the calculated exponents (also from both) along with a discussion of what this would mean in case the lidar measurements at 0.2 Hz never reaches into the inertial subrange. If it does not reach into the inertial sub-range, the Kolmogorov no longer applies and Equation (2) cannot be used – at all. If it it just barely reaches this frequency range, equation 2 would allow calculation of dissipation from a single-frequency spectral estimate assuming the exponent is indeed prescribed to be the expected -5/3. I wonder what the results from such an approach would give?

Hence for the dissipation rate estimates I would like: 1) To know if both instruments are interrogated using the same technique with the same parameters or, if not, what was the difference(s); 2) to see examples of power spectra from both instruments to be able to judge if the spectral estimates used are indeed inside the inertial subrange and; 3) see time series of the value of the exponent arising from the method the authors claim to have used.

Detailed comments

Lines 17-19: Please repharse, as the buoyancy is in fact not measured by the lidar.

Lines 33-34: Langauge: "… terms, which are key physical quantities …"

Line 43: Maybe this is nit-picking but all these parameterization attempts to *model* turbulence. To simulate turbulence you need a DNS or possibly (stretching a bit) LES.

Line 66 and elsewhere: Why do you not start out with the full reference and then skip it at the end: "Nilsson et al. (2016a) used …".

Lines 84-85: Not to mention the cost of maintaining a tower compared to running a lidar!

Section 3.4: Why are we not shown a comparison between the shear-production terms instead of the shear stress terms?

Line 305: What do you mean by "gleaned"? Did you not actually measure this term?

Figures 9 & 11: There are some regions of these plots where the results seem quite noisy. Is there a way by which you can quality control these results, maybe looking at signal strength or pulse return rates?

Figures 10 & 12: By definition these profiles must add up to zero; that doesn't seem to always be the case, especially with in Figure 12a