Review of:
Turbulence measurements with a tethered balloon
Atmospheric Measurement Techniques
Manuscript amt-2015-386
by G. Canut, F. Couvreux, M. Lothon, D. Legain, B. Piguet, A. Lampert, and E. Moulin

Summary:

In this manuscript, the authors present measurements from a tethersonde turbulence platform, where a sonic anemometer and inertial motion sensor are deployed on a tethered balloon to calculate turbulent fluxes from near the surface up to $z = 1000$ m in the atmospheric boundary layer. The authors intercompare the tethersonde data with *in situ* tower measurements. While this technique has the potential to augment other in situ and remote sensing techniques to probe the structure of the atmospheric boundary layer, I have a number of concerns about the manuscript. The authors misrepresent what they are doing—the abstract makes it appear that this is the first time turbulence data has been collected from a tethered balloon, but they cite references to make it clear this is not the case. In many places the manuscript is vague (or sometimes incorrect), e.g. the introduction refers to "boundary-layer processes" repeatedly in a vague sense. My largest concern is the validation of the tethersonde platform. The tethersonde was deployed ∼200 m horizontally from the tower, but the authors do not attempt to estimate the integral lengthscales to convince the reader that the the turbulence can be expected to remain well correlated at the two measurement locations. They report in Fig. 3 up to 50% differences in fluxes between the balloon and tethersonde measurements. Perhaps the measurement platform can collect perfectly fine data, but I am not convinced of this on the basis of what is presented in this manuscript. I would not consider 50% error to be an acceptable validation. Furthermore, the manuscript does not meet my standards for clear technical English—there are a number of grammatical mistakes, errors in units, misspellings, awkward phrasing, and a general lack of attention to detail. Given the sum total of all these concerns, I cannot recommend this manuscript for publication in *Atmospheric Measurement Techniques*.

Major comments on article:

- The abstract does not represent well what the authors are actually doing. The abstract makes the reader think this is the first time turbulence measurements have been collected from a tethered balloon, but the authors cite literature to make it clear this is not the case.

- The authors validate their tethersonde measurements by comparing them with profile tower data. However, this validation is not completely convincing to me. They mentioned that horizontal distances between the two instruments are on the order of 200 m, but never attemped to estimate the integral lengthscales to show that one could expect the turbulence still to be correlated between the two measurement sites. There is some vertical offset as well. They claim good agreement between the tethersonde and the tower data, yet the difference between the fluxes calculated between the two are off by as much as 50%. I do not find this to be good agreement. A sonic anemometer was attached to a balloon together with a GPS inertial motion sensor. The authors say they used the inertial motion sensor to remove the motion of the balloon so their measurements include only a contribution from the turbulence. However,

they fail to provide any details about the motion sensor, and it is unclear to me whether it has the necessary spatial and temporal resolution so that the motion of the balloon platform can be removed to obtain accurate turbulence measurements.

- The English language and grammar in this article need work. There are a number of misspellings, typographical errors, and awkward phrases that do not make sense in English. I have noted some of these passages in my comments below, but ultimately it is the responsibility of the authors (and not of the reviewers) to make sure the paper reads well in clear technical English.

- A number of places in the article lack precision (or are incorrect). In in one place (see comments below) the units employed are incorrect. The authors discuss 'horizontal velocity fluctuations creating TKE,' but this is not an accurate description of how TKE is produced by mean shear—it is the interaction between the vertical momentum flux of streamwise momentum and the mean velocity gradient ($-\overline{uw}\frac{\partial U}{\partial z}$) that produces TKE by mean shear. The introduction refers repeatedly to "turbulent processes" or "boundary-layer processes," in vague terms, but lacks specifics.

Specific comments

- **Abstract** "...first deployment of a turbulent probe below a tethered balloon." Is this the first time ever high-resolution turbulence measurements have been taken from a tethersonde platform? If so be clear. The abstract also does not make it clear whether the article is strictly about the deployment and validation of a new instrument platform, or whether scientific questions are being addressed as well.

- **Introduction, first 2 paragraphs** The motivation here is quite vague. "The time evolution of the parameters close to the surface..." (what parameters?) You say "turbulent processes" or "boundary-layer processes" in the introduction several times without being very specific. It would be better to motivate this discussion in terms of transport of momentum, heat, and trace gases (CO_2), water vapor, etc. which depends on the stability state of the ABL...

- **1st paragraph, l. 25** necessary is misspelled

- **p. 1, 2nd paragraph** "Understanding turbulent processes in the ABL requires the knowledge of the evolution of the profile of the sensible heat flux." True. But this statement makes it sound like it's the only thing one needs to know about. (What about the momentum flux profile? Velocity variances? Temperature variance? Scalar fluxes?)

- **p. 1, 2nd paragraph** "This platform does not allow [one] to obtain vertical profiles, but only provides some measurements at discrete vertical levels." I don't understand what you mean by this statement. **All measurement techniques** (aircraft data, wind profiling radar, lidar, unmanned aerial vehicles, etc.) have some sort of vertical resolution associated with them, so all of these methods provide discrete measurements. The difference is in the vertical resolution of these measurements.

- **p. 1, 2nd paragraph** "Another *inconvenience* of *an* aircraft platform..."

- **p. 1, 2nd paragraph** "into the heat fluxes characteristics..." This reads awkwardly.

- **p. 2, top paragraph** You cite the Lapworth and Mason (1988) paper, and mention they conducted turbulence measurements with a propeller anemometer from a tethersonde platform, but yet your abstract says that your study presents the "first deployment of a turbulence probe below a tethered balloon in field campaigns." So your abstract is misrepresenting what you are actually doing here. Is this the first study to deploy a tethersonde with a sonic anemometer? If so, make this clear to the reader.

- **p. 2, 2nd paragraph** Now you are describing what you are actually doing, i.e. determining whether a sonic anemometer deployed from a balloon can be used to probe turbulence structure in the ABL. This is what needs to be in the abstract.

- **p. 2, end of 2nd paragraph** "Conclusion ends the paper." Again, not good English.

- **p. 2, Sec. 2** Overview is misspelled.

- **p. 2, Sec. 2.1** "the weight of the cable..." Your units are wrong here.. Weight has units of N (or kg m s^{-2}), **not** of kg m^{-1}. This is mass per unit length.

- **p. 2, Sec. 2.1** "can be attached to a wide variety of *balloons*..."

- **p. 2, Sec. 2.1** Be careful with capitalization here (names of manufacturers, instrument models...)

- **Sec. 2.1, 2nd paragraph** Does the Gill WindmasterPro provide sonic *virtual* temperature?

- **Sec. 2.1, 2nd paragraph** "A first performance lies in the low weight of the system" This reads awkwardly.

- **p. 3, Sec. 2.2** I find the discussion of the inertial-GPS motion/attitude sensor to be lacking. What is the sampling rate? Spatial resolution? Without this information, I'm unsure whether you're unable to remove the motion of the balloon to obtain accurate turbulence measurements.

- **p. 3, Sec. 3** I know that some horizontal offset between the balloon and tower is unavoidable, but if they are too far apart (i.e. an integral scale or more), the turbulence will not be correlated between the two locations. Did the authors attempt to quantify to consequences of this horizontal offset (e.g. by estimating the integral lengthscales from the autocorrelation function and verifying that they are more than 200 m)? I believe this is something the authors need to address.

- **p. 4, Fig. 2** Do you know why the PDF of u' looks more flat near the peak for the tethersonde than the tower measurements? Also, it appears that the spectra are actually decaying faster than $-5/3$, which I find to be odd.

- **Sec. 3.2** Section heading should be *Fluxes*. There are other places following this in the article where English grammar, punctuation, and readibility need to be improved, but this is the responsibility of the authors, not of this reviewer.

- **Sec. 3.2, 1st paragraph** Two things to point out here: 1) eddy covariance is also used to calculate momentum and heat fluxes, not just trace gases. 2) It may be better to denote instantaneous flux as, e.g. $w'a'$, since x is usually used in micromet for streamwise coordinate.

- **Fig. 3** This figure is not very convincing to me as a validation. During some periods there is 50% error (or more!) between the tower and balloon measurements. Perhaps the instrument is working fine and this difference is due to the horizontal and vertical offsets between the tethersonde and tower (or perhaps because of random errors due to the averaging time, see below), but I have a hard time accepting that a 50% error constitutes an acceptable validation. This is my biggest concern with the manuscript.

- **p. 5, 1st paragraph** "The agreement is satisfactory..." I do not agree. At times the differences between the measurements is 50% or greater.

- **Sec. 4.1 - TKE profiles** Here you use 20 minute averaging times based on the ogive analysis. This may be enough to reduce systemic bias (e.g. Lenschow et al., *J. Atmos. Ocean. Tech.*, 1994), but I suspect random errors (which are known to increase with height, see chapter by Wynaard in *Workshop on Micrometeorology*, 1973, D. Haugen, Ed.) may be an issue if you're using a 20 minute averaging period throughout the mixed layer. So I'm not entirely convinced that a 20 minute averaging period is long enough.

- **Sec. 5.1** "it is maintained close to 1" What is? Units?

- **Table 2** I usually see articles use z/z_i rather than z_* to denote the ratio between the height and the ABL depth.

- **Figure 7** This figure is not large enough for the axis labels to be legible.

- **Sec. 5.2, 2nd paragraph** "When $A = 0$, the vertical contribution to TKE is zero ($w'^2 = 0$) and the turbulence is only created by horizontal wind fluctuations. This is not a correct statement. In this case, where there is only shear production of TKE and no buoyant production, the shear production term is $-\overline{uw}\frac{\partial U}{\partial z}$, so TKE is being produced by interactions between the momentum flux and the mean wind shear.

- **Secs. 5.2-6** Again, there are a number of places here that read awkwardly and the English needs improvement.

- **Sec. 6** Two comments here. As the authors mention from the literature they cite, this is not the first time a tethersonde has been used for turbulence measurements. They should be careful to accurately represent what they are doing. I do not agree with their point about very good consistency with traditional turbulence measurements. While the system may indeed work correctly, the authors do not manage to convince me of this when they show differences in the fluxes of 50% or more between the *in situ* and tethersonde measurements.