Review of: "**Measurement of motion corrected wind velocity using an aerostat lofted sonic anemometer" by W. R. Stevens, W. Squier, W. Mitchell, B. K. Gullett, and C. Pressley**

This paper sets out to demonstrate the measurement of wind speed and direction from a tethered balloon, including motion and attitude correction of the raw measurements. The application for which the data are intended here – modelling of plume dispersion downwind of forest fires – requires only mean winds at the level of the plume, and the validation of the end data product is conducted largely on 15-minute averaged wind speeds and directions.

There are a number of problems with this work, on which detailed comments are given below. These issues are serious enough to render the manuscript unsuitable for publication.

An immediate question is one of motivation rather than of the work itself. If what is required are 15-minute averages of wind speed and direction, simply averaging the raw uncorrected wind measurements will provide a perfectly adequate estimate of the true wind, averaging out the relatively high frequency bias in individual samples that result from the motion of the platform. Given that the platform – a tethered aerostat – effectively orients itself into wind, a simple internal compass heading provides adequate directional information. No expensive motion sensing instrumentation or complex correction procedures are necessary. The authors note that previous work on motion correction has focussed on turbulence rather than mean wind conditions – this is because for turbulence work motion correction is necessary, while for long-period average wind measurements it is not.

Major Issues

A first major concern is the physical location of the anemometer. This is fixed directly below the body of the aerostat envelope. Judging from the photograph in figure 1, I estimate the distance from the anemometer measurement volume to the envelope is of the order of 1m, maybe a little less. The envelope is approximately 3m in diameter, maybe 2.5 from top to bottom. This installation would be totally unsuitable for turbulence measurements – a separation of order 10 times the diameter of a nearby obstacle (e.g. supporting mast) is usually judged to be a minimum for turbulence measurements. A less stringent criteria might be adequate for mean flow measurements, but a separation of at least 1-2 diameters will be needed. As it is, the measurement volume is well within the region of flow distortion around the aerostat envelope and the wind speed measurements will be biased. Worse still, the measurement volume looks to be above the lower extent of the tail, and thus within the deceleration zone for flow into the tail. I would not believe any wind measurement from this system without calibration and correction for the local biases induced by the aerostat and its tail.

Equation 1, representing the correction of measured wind components for platform motion, and their transform into the earth frame is wrong. The two terms on the right hand side of eqn 1 are: $R_{GS}(v - \Omega \times Q)$ – the wind components relative to the platform in the earth (NWU) reference frame, and v_{NWU} the linear motion of the platform, again in the earth frame. The authors apply the correction of measured wind for platform motion by subtracting the platform motion from the measured wind; they ought to be adding the two velocities. Consider – If the true wind is zero, a platform motion to the north of 1 m/s will result in an apparent wind to the south of 1 m/s – same magnitude, opposite sign in the earth reference frame. Subtracting the platform motion from the measured wind will result in a 'corrected' wind of -2 m/s instead of 0.

Similarly, the correction of the measured wind for the flow induced by the rotation of the measurement volume around the motion sensing measurement point should be ($v + \Omega \times Q$) not ($v - \Omega \times Q$). See for example Edson et al. (1998) equation 4 for the identical correction implemented on a flux measuring buoy at sea.

I suggest that these errors are not readily apparent in the results presented here because, by using 15 minute averages for comparison with other sources of data, all the platform motion signal – both that biasing individual wind samples, and the misapplied 'correction' terms in equation (1) – is averaged out. Note that for a tethered balloon the mean platform motion (except when actively winching in or out) should be zero.

The presentation of the effectiveness of the motion correction and comparison of tethered winds with reference measurements leaves much to be desired.
Figure 3 is of very limited use in this regard, but raises some immediate doubts. The plots of vertical velocity show a very definite mean upward flow (adding a reference zero line would help enormously in making this clear). Assuming that the ground is more or less level the mean vertical flow should be effectively zero. If the ground slopes then the mean flow should be parallel to it – does the implied flow angle from the horizontal make sense given any slope of the terrain and the direction of wind? My suspicion is that this significant upward flow component reflects the flow distortion around the aerostat envelope.
Figure 3b, a plot of unsmoothed 10Hz data provides no useful information.

Other Issues

P705, line 25-27. The authors state that a "UAV must fly into the wind for measurement". This is clearly not the case, any more than it is for a full research aircraft. The UAV measures the wind relative to itself. In order to fly it must have a more or less constant air speed (of ~10-20 m s^{-1}) regardless of its direction of travel relative to the wind. Adding the UAV's velocity relative to the earth to the measured wind relative to the airframe will give the true wind regardless of orientation with respect to the wind direction.

P707, line 26 – "These anemometer data were recorded serially using an onboard computer. Wind data presented here were logged serially using the onboard computer." – repetition of same information in very slightly different words. Delete one sentence or the other.

P709, line 27 & Figure 4. The average offset between the 15-minute average wind direction derived from the 'corrected' sonic anemometer data and that estimated from tether angle is found to be 11°. The authors claim this shows 'good agreement'. Earlier (P706, line 3) they note that Van den Kroonenberg et al. (2008) found an uncertainty in wind direction of ±20° in their UAV measurements. This is a somewhat misleading representation of the various uncertainties. The value of ±20° given for Van den Kroonenberg et al. (2008) actually applies only to a subset of their data obtained under low mean winds conditions where the wind direction is inherently variable. This value is a standard deviation rather than an error about the mean. At higher winds they obtained a value of about ±5° with little mean bias. In the results presented here 11° represents a mean bias, not an uncertainty – a rather more significant problem. From the values provided I calculate that the offset from the sodar wind direction is approximately 3.75° with a 14° standard deviation, somewhat better than the offset estimated from the orientation of the tether. I would suggest that the orientation of the tether is NOT necessarily a good measure of wind direction – it is worth noting that the wind direction can change significantly over the lowest 10s to 100 m or so.

Comparisons of wind speed between the balloon and surface based measurements (figure 6) are useless unless all the measurements are adjusted to a common reference altitude. The near-surface logarithmic wind profile is one of the first things taught in a basic boundary layer meteorology course. Attempting to compare measurements at different heights within the surface layer without correcting for this is doomed to failure.

References

Edson, J. B., A. A. Hinton, K. E. Prada, J. E. Hare, and C. W. Fairall, 1998: Direct covariance flux estimates from mobile platforms at sea. J. Atmos. Oceanic Technol., ***15,*** *547–562.*