Reply to the comments by Referee #3

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(Dated: June 11, 2015)

We thank Referee 3 for his/her review of our work. We are particularly encouraged to see that Referee 2 finds "This is a well documented description of the research site that the authors are using to study turbulence and drops in clouds." and that "Publication is recommended."

Below are our responses to the comments raised by Referee 3 to improve our manuscript.

1. **The referee writes:** *General point: The authors argue that the the large scales are similar to laboratory flows. This is apparently to justify using the site as a high Reynolds number laboratory for cloud studies. Possibly they are trying too hard to make the laboratory connection and it would be of interest to the readers to find out what they see as significant differences to the laboratory, rather than pointing out only the similarities.*

   The authors reply: We thank the referee for this very interesting comment. In the previous version of the manuscript, we already discussed the differences between the flow at UFS and the idealized conditions or laboratory flows. For example, we showed the the flow at UFS is anisotropic in general, as indicated by both the ratio between longitudinal and transverse integral scales (page 8 of the discussion paper) and the fluctuation velocities in different directions (page 11 of the discussion paper). Nevertheless, we did emphasize the similarities because: (1) Given the complicated flow conditions, the similarities between the flow at UFS and the well-controlled laboratory flows come as a surprise; and (2) These similarities allow us to apply well-developed techniques to analyze the turbulence at UFS.

2. **The referee writes:** *A few specific recommendations: 1. What dates (or at lest seasons) were the photos in Figs 1 and 2 taken?*

   The authors reply: Thanks for pointing this out. The photos shown in Figure 1 were taken in May 2007 (top panel) and October 2009 (bottom panel). The photo in Figure 2 were taken in April 2009. We added this information in the captions of the figures.

3. **The referee writes:** *2. Fig. 3. It would be good to know this type of data for more than two years. Perhaps a web site can be provided.*

The authors reply: Figure 3 actually contains data recorded in 12 years, from 2000 to 2012. More data are available on DWD (Deutscher Wetterdienst, German Weather Service) website, which we now added in the caption of Figure 3.

4. **The referee writes:** *3. Fig. 8. The inertial subrange is one decade, if that. Presumably resolution issues and noise then take over. There could be some mention of this. Also it would be good to have normalized frequency, or scale information as the abscissa. In the insert, there is no indication of a diurnal bump. Comment is needed.*

   The authors reply: The Referee is right. The measured spectra deviate from the expected inertial subrange at about $f \sim 10^{-1}$ Hz. The main reason is the resolution issue of the ultrasonic sensor we used, which rounds off the velocity data to 0.1 m/s when sending back the data. As a result the measured spectrum gradually becomes *above* the expected inertial range. In the revised version, we mentioned this in the caption of Figure 8.

   As motivated by referee's comment, we added in Figure 8 the spectrum measured using a hot-wire (shown as Figure 4 in the accompanying paper by Siebert et al.), which is sampled at 1000 Hz and has a much better resolution. As can be seen from the new Figure 8 in the revised manuscript, the spectra obtained from three different sources (DWD, our own sonic sensor and hot-wire) overlap very well and show clearly a wide inertial range for frequencies between $10^{-2} - 10^{2}$ Hz.

   The abscissa of Figure 8 is in Hz. It is not clear to us which normalization the referee suggests to normalize the frequency (by minute, hour, day, or some large-scale flow time scale?). Therefore, we think it is better to leave the plot as it is and the readers can convert to the normalized frequencies that they are interested in.

   We are somewhat confused by the comment that "In the insert, there is no indication of a diurnal bump" since the diurnal bump is clearly visible in the inset of Figure 8, which is the bump corresponding to $f = 1/86400\ \mathrm{s}^{-1} = 1.15 \times 10^{-5}$ Hz.

5. **The referee writes:** *4. Fig 12. Meteorologists like to know the value of the un-normailzed dissipation rate. Perhaps mention of typical values could be given in the text.*

   The authors reply: We thank the referee for this suggestion. In fact, we already gave the typical values of the energy dissipation rate in the text. More specifically, in the paragraph following Eq. (7), we stated:

“We therefore take the averages of the values obtained using Eqs. (5) and (6) as the measured ε. The energy dissipation rates determined in this way are in the range of 10^{-4} to 10^{-2} m^2s^{-3}, which are comparable with previously reported values of atmospheric turbulence measurements (Wyngaard, 2010) and are also typical for turbulence in clouds (Siebert et al., 2006).”