Author‘s response to Ian Brooks‘ review of: AMT-2019-80

We thank Ian Brooks for his constructive review, which significantly improved the quality of the manuscript. The author‘s response is structured as follows:

Comments of reviewer
Author‘s answer to the comment
“New text passage”

General remarks

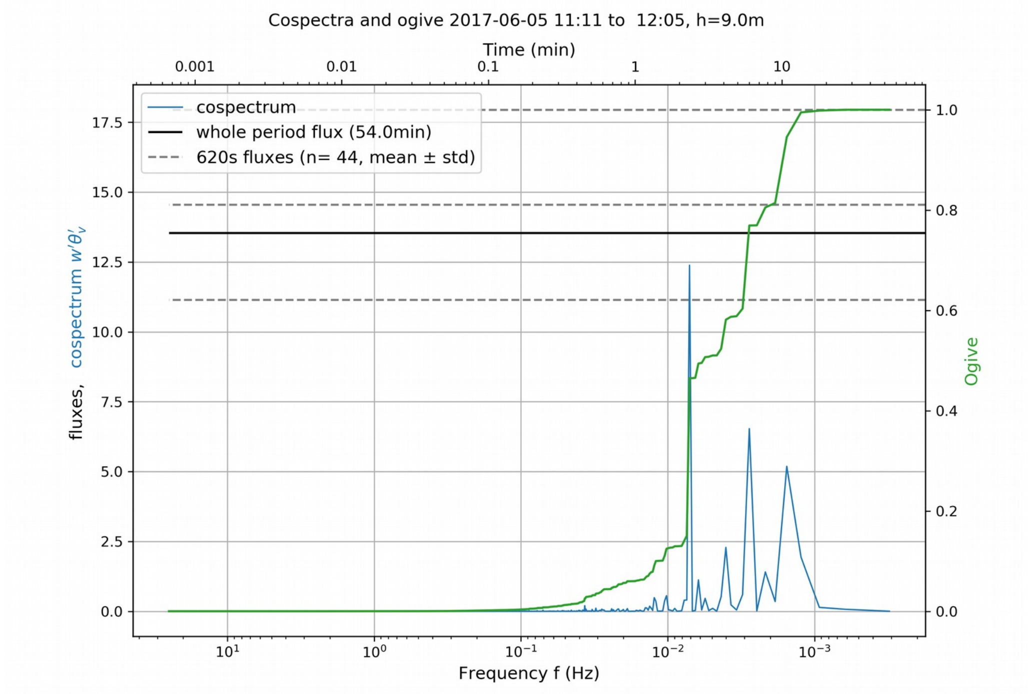
- All suggestions for rephrasing and including references were implemented. This remarkably improved the language quality and made the manuscript more comprehensive.
- The term buoyancy flux, which was used throughout the discussion paper, is misleading. For the flux calculation, we used the definition of the sensible heat flux, but with the potential virtual temperature instead of the potential temperature. We renamed „*buoyancy flux* H_B“ as „*virtual heat flux* H“. This term was introduced by Angevine, 1993 (doi: 10.1175/1520-0450(1993)032<1901:VHFMFA>2.0.CO;2) and the reference is now included in the manuscript.

Answers to specific reviewer comments

- **Page 4, Line 9: The quoted ‘standard lapse rate’ of 6.5 k km-1 is rather low, the dry lapse rate is approximately 9.8 K/km (AMS glossary), and at freezing point in just saturated air is 9.968 K/km. The value given here is approximately the wet adiabatic lapse rate. Ideally the wet/dry value should be used in/out of cloud to give most accurate height.**
  You are right, but here we use the value given for the ICAO standard atmosphere ($z < 11$ km), as referred to in Wendisch & Brenguier 2013 (doi: 10.1002/9783527653218, p. 11). With this, we want to standardize the barometric height calculation, independently of the presence of clouds, and make it comparable to the aircraft heights. However, we compared also to GPS height and the differences were in the range of ±10 m below 1.000 m altitude.

- **Page 16, Line 24: regarding the averaging time for fixed altitude flux estimates. While the 620s periods is not unreasonable, it may be too short under some conditions. It is worth examining cospectra and ogives curves to evaluate whether all the low-frequency flux contributions are captured, and if not estimate the missing flux contribution. 30 minutes would be long enough to capture all the flux contributions under most conditions, but clearly must be traded off against the available time and number of levels you can sample.**
  That’s a good point. We calculated the co-spectrum and ogive for a 1h period (5 June 2017) with the balloon kept near the ground, associated with a high degree of turbulence. The ogive suggests, that after 620s, 91% of the flux value are captured. Further, the flux averaged over the whole period is close to the mean value calculated from 44 (overlapping) 620s-segments within the whole period. From this, we conclude, that the 620s period results in an error in the order of 10%. For higher altitudes, we expect the integral time and length scales to decrease which in turn - assuming a similar correlation coefficient between vertical velocity and temperature - results in a decreasing statistical error. We also estimated the statistical error based on Lenschow’s arguments already in the manuscript and now include the resulting statistical error for the 620s period.

  “Applying this averaging time, the random flux error is around 40% in the turbulent regions.”



- **Page 19, Line 33: in discussion of vertical wind, it is worth noting here that these regions of significant upward/downward motion cannot be mean motions, but the sampling of up/down moving portions of a large, boundary-layer scale eddy. It is notable, however, that their vertical location coincides with the regions of higher/lower horizontal wind. Possibly this too is sampling of a large scale eddy?**
  A first idea was that this might result from an artefact in the wind vector transformation. For the balloon, the wind/ turbulence and the motion of the instrument itself are closely connected, which makes the transformation challenging. Therefore, we verified again the transformation of the wind vector during the ascent and could not find, that the vertical wind variation results from a measurement artefact (e.g., an offset in the pitch angle). We are now confident that the shown wind velocity components are correct. Your interpretation of sampling within one large-scale eddy is convincing and in the manuscript we made a short comment about this possible explanation:
  “As the location of upward motion and increased horizontal wind coincides, this might be an indication for a large, boundary-layer scale eddy.”

  A minor issue of phrasing here (and in discussion of standard deviation of w) – the phrase ‘wind speed’ tends to imply a mean quantity, and thus ‘vertical wind speed’ seems inappropriate for w, for which simply ‘vertical velocity’ is perhaps better terminology.
  “*Wind speed*” was replaced by “*wind velocity*”, when it did not refer to the mean value.

- **Page 21, Figure 11 caption. Here and elsewhere I think the term ‘terrestrial’ to refer to the infra red irradiance is misleading, since much of the infra red budget has nothing to do with the surface. ‘infra red’ would be preferable.**
  As our analysis focuses mostly on clouds, the term ‘terrestrial’ might truly be misleading. Thus, we will change the term to *“thermal infrared” (TIR)*. According to the definition of TIR from ~3-50 µm (Manfred Wendisch and Ping Yang, 2012), this range will closely agree to our measured values of 4.5-42 µm.

- **Figure 11 and 14 – why do the solar irradiances stop at cloud top, while the infra red values extend above?**
  The misalignment of the solar sensors exposed to direct sunlight are usually corrected using the method described by Bannehr and Schwiesow (1993). In case of a strong

movement and misalignment of the sensor package, the correction could not be applied successfully and a bias remained. These conditions with strong wind shear where predominantly observed above clouds. For measurement periods in higher and constant altitudes (calm conditions), which are not shown in the paper, the attitude correction worked properly. For observations below and in clouds, the radiation field is assumed to be dominated by diffuse radiation, which is isotropically distributed. Here, no correction is needed. Based on these limitations and to avoid misinterpretation of the measurements, we decided to show only data measured below and in clouds in Figure 11 and 14. On 14 June 2017 a second cloud layer above the boundary layer cloud guarantied diffuse downward radiation. A discussion on the treatment of the solar radiation in cloud free conditions is added to the revised manuscript (Sect. 3.3.3).

“Misalignment of the sensor with respect to the horizontal plane affects the measurements of the downward irradiance in cloudless conditions (Wendisch et al. 2001). When the radiation field is dominated by direct solar radiation and the misalignment does not exceed 5-10° (depending on solar zenith angle), a correction is possible as demonstrated by Bannehr and Schwiesow (1993). Due to wind shear, the broadband radiation package started to swing, reaching roll and pitch angles of up to 20° while the heading changed quickly (up to 180° in 5 s). In these conditions, which predominantly occurred above clouds, the algorithm failed to correct the sensor movement. To avoid a misinterpretation of the remaining misalignment bias, data in such conditions had to be excluded from the analysis. In calm wind conditions, the attitude correction worked properly. Below or within clouds it is assumed, that diffuse and isotropically distributed radiation dominates the radiation field. Hence, a correction is not required in these conditions. To identify the presence of clouds shielding direct sunlight, the camera is used.”

- **Page 22, Line 2 & figure 11. The strong negative buoyancy flux just below cloud top might be associated with entrainment. It is hard, however, to find a physical explanation for a strong upward flux just above cloud top – this might be an artefact of calculating an eddy covariance flux from a slant profile that crosses a strong and changing gradient at the inversion where perturbations from the ‘mean’ can result from changes in the mean rather than true turbulent fluctuations. I would treat this value with extreme caution**
  and
  Page 24, Line 6 – the negative peak in buoyancy flux ‘inside the cloud’ is located just below the inversion base – again possibly associated with entrainment mixing, though again, calculating a flux from a slant profile across a strong and changing gradient is problematic, and I would want to carefully select the portion of the profile used to stay below the inversion base.
  Thank you for this comment. We realized that in fact in regions with strong changes in the temperature gradients (e.g. transition from well-mixed to extremely stable regions) the high-pass filter creates artificial variability. Therefore, we now use a Bessel filter (N=20) instead of Savitzky-Golay filter as applied in the discussion paper. This filter has a better performance in regions with a rapid change of the gradient. For moderate inversions with a smoother transition the low-pass filtered signal can now follow the raw signal resulting in reasonable fluctuations. Now, the slant profile variances in Fig. 10, 12 and 13 are of slightly smaller magnitude. Nevertheless, for strong inversions associated with strong changes in temperature gradient we excluded this region from the flux calculation because even the Bessel filter is not able to follow the sharp jump resulting in artificial fluctuations in the region.

p. 15, l. 21: “*On the slant profile, turbulent fluctuations are determined using a high-pass 20th-order Bessel filter. The cut-off frequency* $f_c = U/z_c$ *for the filter is given by the cloud layer thickness* z_c *and mean horizontal wind velocity U and is in the range from 0.009 Hz to 0.025 Hz.”*

p. 22, l. 2: Due to the revised filter algorithm, the positive and negative peak is not that strong anymore, but we excluded the temperature inversion region from the slant profile flux calculation, as the filter cannot follow the sharp transition between well-mixed and extremely stable regions.

“The magnitude of the virtual heat flux derived from the slant profile is significantly smaller than the fluxes measured at constant altitude. Positive heat fluxes of up to 3 W/m^2 *are measured within the whole mixed layer. The cloud top region is excluded from the virtual heat flux calculation. Due to the strong change in the temperature gradient at the transition from the well-mixed layer to the inversion, the filter algorithm creates artificial fluctuations in this region, which results in unrealistic fluxes.”*

p. 24, l. 6: Due to the revised filter algorithm, the positive and negative peaks are much weaker, but still present. Here, the temperature inversion is not very sharp, therefore no values have to be excluded from the flux calculation.

“The virtual heat flux fluctuates around zero with a slight negative tendency within the cloud, changing to positive values near the surface. Due to the gradually changing temperature gradient at the inversion, no values have to be excluded from the flux calculation.”

Furthermore, we calculated the fluxes on the slant profile for overlapping (instead of non-overlapping) 50s-periods to get a more complete picture of their vertical structure.