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Discussions

Interactive comment on "A new method to measure bowen ratios using high resolution vertical dry and wet bulb temperature profiles" *by* T. Euser et al.

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Dear referee,

Thank you for reviewing our manuscript. We are sorry to hear that you do not think the current results are worth publishing. Also based on the review written by Stan Schymanski, we realised that the presentation and discussion of the measured results can be improved a lot. However, we still think that the results are worth publishing and we will respond to your comments below and try to explain why we choose to

publish these results. We believe that by presenting and publishing interim results, other scientist can apply the findings and discussions can be started to further develop the new methods. Therefore, we really appreciate all comments.

General comments

1. *The measurements were done in a field with dimensions of 80 X 80 m^2 (7165/6). The systems were installed at some position within this field (not specified in the text) so that for some wind directions the available fetch was lower than 80 m. No footprint analysis is presented for the various measurement systems examined. In such a small field with a 0.5 m canopy, I doubt whether sufficient fetch was available for reliable micrometeorological measurements. In particular, the upper part of the DTS cables is about 4.5 m high; it is most likely that this part of the cable is influenced by fluxes from outside the filed under study. Without a footprint analysis that proves their validity, the present results do not seem to be reliable.*

   The small field size might indeed be the cause of an error in the measurements. However, all the equipment was installed close together and it was not the specific purpose to measure evaporation above sugar beet, but to compare different methods. In addition to that, most of the surrounding vegetation was low as well: mostly grass with some scattered shrubs and trees. The main difference between the vegetation on the sugar beet plot and the surrounding fields is probably that part of it was irrigated and another part was not. In relation to the height of the BR-DTS tower, we now also compared the lower part of the DTS measurements (until 2.2m) with the other measurements and investigate the differences. This results in more comparable results (see Table 1).

2. *The period of measurements is very short, and as stated by the authors sufficient only for a "first investigation" (7164/25-26 and 7177/8-10). Only 5 days are presented, each with very different climatic conditions and missing data. In many data points the R2 for the linear regression between temperature and vapour pressure (Figs. 4 and 6) is relatively low so the performance of the method is not proved. The authors claim that this method is simple (7162/20) so it is not clear why didn't they run it for a longer period in the field and presented more data to better establish its validity. I recommend that the authors extend the measurement period in the future submission.*

   The idea of the method is simple, however development of all new measurement equipment takes more time than expected. During each experiment you discover that certain elements can be improved. Before this experiment we have done a couple of experiment in Luxembourg and the Netherlands, the results from this experiment in South Africa seemed promising enough to us to write a manuscript about it. The purpose of this manuscript is to present a proof of concept, not a fully developed measuring method. Longer measuring campaigns are currently planned.

3. *The direct LE results of the EC apparently do not agree with the other indirect LE results and are therefore discarded by the authors. I do not accept the statement that EC results are unreliable (7175/6-8). I'm sure the authors are aware that this method is nowadays the most accurate and acceptable method for direct flux measurements worldwide. I suspect that due to footprint issues (see comment 1), these results are different from the other methods. In addition, it is not clear whether corrections were applied to the EC data as is common with this technique. The authors mention two EC systems (7171/25) but do not indicate which one was used for the analysis.*

   We will remark on this together with comment 4.

4. *All indirect methods examined here are based on estimating H and extracting LE from the energy balance. Given that H is relatively small (in most cases about 20% of LE or less) errors in H would not affect much the calculated LE which is mostly governed by the net radiation. This is one reason why all indirect methods are in good agreement with each other. It would be more indicative to compare H obtained by the different approaches than LE. Even though H is the parameter estimated by all methods there is no single graph that shows diurnal variations of H or a comparison between H values obtained by the different approaches.*

   Also based on the review of Stan Schymanski we realised that discarding the direct latent heat flux requires more explanation. In relation to point 3 and 4, we can comment the following.

   On the measured days, the energy balance closure of the EC150 set up often has a residual of more than 30% of $R_n - G$. On the days with the smallest residual, there is only fragmented data available for the BR-DTS system. There can be several reasons for this residual, for example an erroneous measurement of the turbulent fluxes or the ground energy storage, occurrence of advection or heterogeneity of the field (Foken, 2008[1]). It is difficult to determine the main cause for the residual.
   The latent heat flux is measured with one method and the sensible heat flux with multiple. We agree that comparing the indirectly obtained latent heat fluxes leads to auto self-correlation, which is undesired. Therefore, we think it is better to compare the sensible heat fluxes of the four different methods. In figure 7, 8 and 9 of the manuscript the latent heat will be replaced by the sensible heat. The

[1]Foken, T. (2008): The energy balance closure problem: an overview. The Ecological Society of America, 18, 1351-1367.

first column shows the results for the entire height of the DTS tower and with a constant value for the psychrometric constant. Table 1 also shows the results for a variable value for the psychrometric constant and for only the lower part of the DTS measurements. It can be seen that the correlation of BR-DTS with the other techniques is comparable with the correlation between these techniques.

Fig. 1 shows the correlation between the sensible heat fluxes derived with the BR-DTS in comparison with the sensible heat fluxes derived with the reference techniques.

All necessary corrections were done for the EC data: correction of the sonic temperature for the effect of moisture, the Webb, Pearman, and Leuning (WPL) correction to the water vapor flux for air density effects (Webb et al.,1980[2]), and coordinate rotation.

In section 3.2.1 (p.7171, L24) we described that the Applied Technologies sonic anemometer is only used to determine the sensible heat flux, so the latent heat flux comes from the EC150 system, we will make this more clear.

5. *The estimate of the Bowen ratio is based on the psychrometric constant (7166/17). However, this constant varies with the ventilation rate of the wet-bulb sensor (Allen et al. 2006, FAO56). In this experiment the ventilation of the wet-bulb fibre optic cable is governed by the wind speed which is variable with time and height above the ground (and along the cable). There is no consideration of this effect in the analysis.*

We did indeed not include this effect. The wind speed was only measured at one height, so we can only include the effect of a variable psychrometric con-

[2] Webb, E.K. and Pearman, G.I. and Leuning, R. (1980): Correction of flux measurements for density effects due to heat and water vapour transfer. Quarterly Journal of the Royal Meteorological Society, 106, 85-100.

Table 1. Specifics of linear regression between sensible heat flux of all different methods. For 'γ variable' the value of γ depends on the wind speed (γ=0.059kPa/°C for u >3m/s and γ=0.071kPa/°C for u <3m/s [a]). For 'DTS 4.8m' the total height of the DTS tower is taken into account, for 'DTS 2.2m' only the lower part

	γ constant, DTS 4.8m		γ variable, DTS 4.8m		γ variable, DTS 2.2m	
	Slope	R^2	Slope	R^2	Slope	R^2
BR-DTS - EC_{EB}	1.09	0.57	1.02	0.61	0.94	0.58
BR-DTS - SLS	1.03	0.62	0.93	0.65	0.97	0.71
BR-DTS - SR	1.00	0.68	0.90	0.71	0.96	0.80
EC_{EB} - SLS	1.17	0.1b	1.17	0.1b	1.17	0.1b
EC_{EB} - SR	1.05	0.70	1.05	0.70	1.05	0.70
SR - SLS	0.95	0.81	0.95	0.81	0.95	0.81

a Values for γ are obtained from: Allen, R. G., Pereira, L. S., Raes, D., Smith, M. (1998): Crop evapotranspiration - Guidelines for computing crop water requirements. Irrigation and Drainage Paper 56, Food and Agriculture Organization, Rome, Italy.

b These time series contain some very extreme outliers and there are not much coinciding moments with data for EC_{EB} and SLS stant in time and not in height. Because of limited knowledge about the relation between wind speed and the psychrometric constant, we followed Fritschen and Gay (1979)[3] to assume full ventilation at $u = 3m/s$. So, for wind speeds below 3 m/s we used $\gamma = 0.071$kPa/°C and for wind speeds above 3m/s we used $\gamma = 0.059$kPa/°C. If the effect of wind speed on the psychrometric constant is included, the results change. The sensible heat from BR-DTS will be more comparable to the sensible heat from the eddy covariance and less comparable with the sensible heat from SLS and SR (see Table 1).

6. *The Surface Renewal technique (7172/19) requires a calibration coefficient. The authors do not mention a calibration process or the coefficient used for the sugar beet plants under study. Hence it is not clear how the SR data were calculated.*

   The surface renewal method was calibrated using the eddy covariance system and a correction factor = 1 was used for sugar beet canopy to estimate H at 1.0 m above the soil surface.

Specific comments

1. *The claim that this approach is simple (7162/20) is not well justified. There appear to be serious technical constrains and difficulties in operating the system, especially the wet-bulb cable, including the long-term use of an ice-bath (7169/20) and the water supply (7178/1-3). The authors may discuss this approach as compared to installing several high-accuracy temperature-humidity probes at different levels and obtaining the same data utilizing a more conventional technology.*
   We agree that the term 'simple' might be a bit over enthusiastic, see also our remark at general comment 2. We would like to add that the ice-bath only needs

[3]Fritschen, L.J. and Gay, L.W. (1979): Environmental instrumentation, Springer-Verlag, New York.

to be used for about 30 minutes during the entire measuring campaign, as the only purpose of this bath is to determine the bottom of the spirals. The temperature of the water bath required for calibration is of non-importance as long as it is monitored under different temperature conditions.
The advantage of using a DTS setup instead of several high-accuracy temperature-humidity sensors is that the DTS uses only 1 sensor, so the disadvantage of the standard BR method of small differences between separate sensors is diminished. This will not be the case when again multiple temperature-humidity sensors are used.

2. *The section "study area" (7165/1) should be included in the section Materials and Methods.*
   For the clarity of the paper we think it is better to keep the study area section separate from the materials and methods section, as the latter is already long and diverse. However, if the referee has specific arguments to combine these two sections, we will consider it again.

3. *Page 7172 lines 11-12: Please verify the canopy height.*
   The canopy height mentioned on this page in indeed wrong, we thank the referee for pointing this out. It should be 1m above the canopy.

4. *I would remove some of the suggestions in section 5, especially the last paragraphs which is not directly related to the scientific issues addressed by this MS.*
   The purpose of the manuscript is comparing BR-DTS with other methods and argue that it is worthwhile to investigate and develop BR-DTS further. The main reason why we think it deserves additional research is because the results are comparable with other methods, but it has different application possibilities. Therefore, we have added the last paragraphs.

5. *I cannot agree with some statements in Table 3. (i) Closure of the energy balance is guaranteed since the Bowen ratio method, in general, relies on the assumption*

of a perfect energy balance closure. It is not a unique advantage of the present measurement technology. (ii) The methodology does not appear to be cheap. The Surface renewal technique with a single miniature thermocouple is certainly much cheaper than the DTS system. (iii) It is not clear how this specific technique is preferable over other techniques in separating soil evaporation and canopy transpiration. (iv) The authors mention the required fetch as a disadvantage. I agree, but this is a disadvantage of all techniques used in this MS.

Also based on the review of Stan Schymanski, we realised that this table 3 is confusing. Therefore, we will or add the advantages and disadvantages of all the used methods or we will remove the table and describe in the text the advantages and disadvantages with respect to specific other methods. We will remove the point about the closure of the energy balance as an advantage.

6. *In Fig.6 it is not clear why so different sunset hours are shown in the different days.*

   The transition of R_n from negative to positive and vice versa is used as indication for sunrise respectively sunset, as the value of R_n has a large influence on the results derived from the BR method. We will adapt the caption to 'The vertical lines show the moments between which R_n is positive.'

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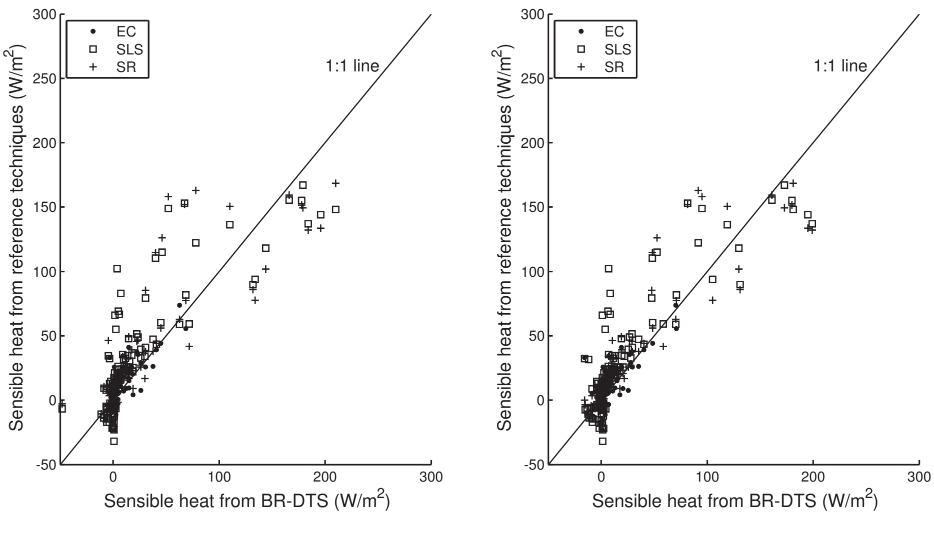

Fig. 1. Sensible heat for BR-DTS and reference techniques. Left: total height of DTS (1-2.2m and 3.2-4.8m), Right: only lower part (1-2.2m) of DTS measurements