Hydrol. Earth Syst. Sci. Discuss., 10, C3347–C3350, 2013
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Hydrology and Earth System Sciences
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.

Anonymous Referee #2

Received and published: 22 July 2013

General:

In this MS the authors test the DTS sensor for Bowen ratio estimations. The sensor is based on a fiber optic cable combined with a computer-controlled laser that transmits short laser pulses. An additional unit receives the reflected frequencies, one of which has temperature dependent amplitude. This technique for temperature profile measurements is not new and was reported in the literature. The new aspects here are its application for Bowen ratio estimates and using the technique to measure not only the dry-bulb but also the wet-bulb temperature. The authors used several other techniques to compare with the Bowen ratio, including eddy-covariance, surface renewal and scin-

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tillometry. I do not recommend publication of this MS in HESS in its present form due to the following main reasons. Numbers in parentheses indicate (page number/line number).

1. The measurements were done in a field with dimensions of 80 X 80 m2 (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.

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.

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.

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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.

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 fiber 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.

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.

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.

2. The section “study area” (7165/1) should be included in the section Materials and Methods.

3. Page 7172 lines 11-12: Please verify the canopy height.

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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.

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.

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

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 7161, 2013.