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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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1 Summary

The manuscript presents a new method to estimate Bowen ratios (BR) using distributed temperature sensing. It is well presented and reads well. In parts, it reads like an advertisement for the new technique, rather than an objective comparison and assessment. Some misleading statements should be removed/reformulated, while the data analysis and discussion should be improved to allow the reader to make an unbiased judgement of the usefulness of the new technique.

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Interactive Discussion

Discussion Paper

2 General comments

I have heard colleagues raving about the new DTS-based method to estimate Bowen ratios one or two years ago, and I was very happy to see it tested in this manuscript. The field and accompanying lab experiments seem well designed and the paper reads well. However, while reading the paper, I got the impression that the authors might be over-selling the advantages of the method a bit and I would like to raise the following concerns:

1. The authors do not specify how much water was evaporated by the wet cable. If the amount is significant in comparison to the latent heat flux within the footprint of the different methods, it would bias the results of all methods in a similar way and hence a close correspondence between methods would not necessarily mean that the latent heat flux from the land surface is accurately estimated. In this context, it would be helpful to mention the footprints of the different methods.

2. The fact that what the authors refer to as the "direct EC method" does not coincide with the results from the proposed and all other energy balance methods deserves more attention. I found it highly misleading to point out that the direct EC method can require corrections and dismiss of it while henceforth referring to the "indirect EC method" as "Eddy Covariance" data. To my knowledge, the direct EC method is the standard and most readers think of this method when reading "eddy covariance data". In fact, the big advantage of the EC technique is that both sensible (H) and latent heat flux (LE) can be estimated independently and energy balance closure can then be used as a data quality indicator. In the "indirect EC method", the authors discard the LE measurement and instead derive it by difference from the energy balance. Thus, if there is a big error in one of the other energy balance components, this error would similarly affect the LE estimates of all other methods and create a false sense of correspondence between methods. If the authors have reason to mistrust the direct EC estimate of

LE, then they should compare the EC-derived H with that obtained from the new method, in order to use a direct EC measurement and avoid error propagation from the other energy balance components.

3. The improvement in comparison to the 2-point BR method is not very convincing. In Table 2, the authors present a decrease in standard deviations of diurnal BR estimations when using the new 13-point technique. They imply in the text that the reduction in standard deviation means “more constant results and less outliers.” Without additional support, this is not convincing, as the Bowen ratio varies naturally during the day, so a decreased standard deviation in the measurements could also stem from missing part of the natural variability. In the conclusions, the authors claim that the new technique is less sensitive to measurement errors and showed a less spurious behaviour of the Bowen ratio values. I could not find much support for these statements in the data. In the contrary, the new method failed under certain wind conditions and the evaporation from the wet cable could lead to a systematic bias.

4. It is not clear what method is used as a reference in the listing of advantages and disadvantages of the new method. I believe that the surface renewal and 2-point BR approaches are even cheaper methodologies, and the “guaranteed” closure of the energy budget is not an advantage at all, but a result of not being able to measure latent and sensible heat fluxes directly (see Specific comments below). The table would be much more helpful if it did list the advantages and disadvantages of the reference methods as well.

I think that the study described in this manuscript is very interesting and important, even if the final outcome might be that the DTS BREB method is not so great for estimating latent heat flux after all, compared to other methods. Therefore I believe that the authors should try to avoid giving the impression that they want to “sell” or praise this method and instead include and discuss all evidence that may be used in favour or against the method. I hope that my comments below will help to improve the manuscript in this respect.

3 Specific comments

1. P7163, L. 26–: It would be helpful to the reader if the authors explained the principles of the BR method a bit more clearly, before discussing its draw-backs.
2. P. 7166, L. 10: The authors probably mean equivalent, not identical (different units!).
3. P. 7166, L. 1–5: It would be good to remind the reader here that knowledge of R_n and G is also needed.
4. P. 7169, L. 1-6: What was the accuracy of the water bath temperature measurement? Why continuous calibration? Do the calibration parameters change over time?
5. P. 7169, L. 10: Could evaporation from the wet cable affect the results?
6. P. 7169, L. 12: Was the rate of water supply monitored?
7. P. 7170, L. 12-14: This section is not entirely clear. Do you mean that the results are sensitive to the water supply rate? How can the appropriate distance for the measurement of the wet bulb temperature be determined in the field? What would be the minimal water supply rate that would allow measuring accurate wet bulb temperature for all relevant points?
8. P. 7170, L. 25-30: Why was the profile expected to be logarithmic? What is the uncertainty related to fitting a logarithmic curve to 2 points?

9. P. 7171, L. 11: What were the water supply rates in the lab and the field respectively?

10. P. 7172, L. 5: Why humidity probe if an open path gas analyser was used? Did the GA not measure water vapour concentration?

11. P. 7172, L. 6-7: Why was LE obtained from the energy balance and not directly? After all, the direct estimation of LE is the strength of an EC system.

12. P. 7175, L. 6-12: To my knowledge, LE is measured directly in the standard Fluxnet approach, so I cannot believe that this technique is less reliable than the energy balance techniques. Instead of removing this contradicting evidence from further analysis, the authors should discuss why the direct and indirect measurements were so different. Perhaps one of the other components of the energy balance was not estimated correctly, which would have led to the same error in all of the indirect approaches.

13. P. 7176, L. 14-19: The Bowen ratio varies naturally throughout the day, so why should a lower standard deviation in the measurements imply higher data quality? A lower standard deviation might be the result of fewer outliers, but it might also result from missing part of natural variability.

14. P. 7176, L. 24-27: This is misleading as any absolute error in the BR will result in an infinite relative error when BR=0. Why not show absolute errors, or errors in the subsequent estimation of H and LE?

15. P. 7177, L. 12: This is highly misleading, as this refers to the “indirect” method, whereas the deviation from direct eddy covariance results was very high. This ought to be mentioned here.

16. P. 7177, L. 12-17: The main motivation was to improve on the two level Bowen ratio method, but the relevant comparison is not sufficiently discussed here.

17. P. 7177, L. 19-21: I did not find clear support in the results for the claim that the BR-DTS method is less sensitive to measurement errors and that it shows less spurious results. Could you be more specific? What about the spurious results due to blowing moisture onto the dry cable?

18. P. 7178, L. 4-5: What would be the disadvantages of increasing the distance between the dry and wet cables? How far apart can they be?

19. Table 2: Please clarify in the caption that these are indeed the standard deviations of diurnal BR values. How many values were used for each day? The relative improvement is a bit misleading, as it is highest for days with generally low standard deviation, i.e. where both methods show very constant values, anyway. I would recommend to leave out this column.

20. Table 3: Need to state what the BR-DTS is compared with here. EC or two-point BR? What do you mean by guaranteed energy closure? The fact that H and LE are ultimately obtained by difference of the remaining energy balance components? In this case, it should be formulated as a disadvantage, as energy balance closure cannot be taken as a diagnosis tool to assess reliability of the data, as for example in the standard ("direct") eddy covariance method.

21. Fig. 6: Maybe clearer: "Top panel: R^2 value of linear regression between T_a and e_a for 13 data points between 1 and 4.6 m (see Fig. 4). Bottom panel: half-hourly Bowen ratio values derived from the linear regression. The vertical red lines mark sun rise and sun set, ..."

22. Fig. 7: Why are there no points for the standard 2-point BR method? Why are there no points for EC measurements related to BREB values above 300 W m^{-2}?

23. Fig. 8: What does CSAT on the vertical axis stand for? Please mention that the indirect eddy covariance method was used here!

24. Fig. 9: Both methods can have an error, so it would be better to call it “relative difference” rather than relative error.

25. Fig. 10: The relative error is not very meaningful here, as it goes to infinity for BR $\rightarrow 0$. Furthermore, errors in the estimation of the Bowen ratio are quite irrelevant for periods when H + LE is small, e.g. in the early morning hours. Why not show the difference in derived LE or H instead? Would it not be helpful to see the data points behind the grey bar in order to get a feeling for the error in this period?

4 Technical corrections

- P7167, L. 19: “accurately”
- P. 7171, L. 5: “to come to equilibrium”
- P. 7177, L. 4: “concept of”

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