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Interactive comment

Interactive comment on "Technical note: using Distributed Temperature Sensing for Bowen ratio evaporation measurements" *by* Bart Schilperoort et al.

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Dear referee,

Thank you for the critical review. We are happy to hear you feel that the vertical profile measurements using DTS could be very useful to improve flux measurements. We hope that we can improve the manuscript and clarify some points using your comments.

General comments: *The idea of vertically-resolved temperature and humidity mea-*

surements using DTS is very nice, and with some technical and conceptual adjustments such an approach is likely to improve near-surface flux measurements. However, I find that the present study falls short in bringing to bear the main advantages that full profiles could offer and at the end, the measurements are reduced to invoking the standard logarithmic profiles followed by extensive averaging to remove "noise" that, in fact, could offer the most interesting new insights into the fluxes of interest... I found the comparison with EC measurements a bit weak and contributing to the ambiguity in the value of the new method (e.g., the comparisons made by Euser et al. 2014 including Surface layer scintillometer were somewhat more definitive). In order for such a new method to gain traction, it is imperative (in my view) that the method is tested over as simple surfaces as possible such as water surfaces or a flat land surface after irrigation and follow drying, etc. to remove as much as possible confounding effects of canopy and other aerodynamic masking effects. Alternatively, the authors should convincingly show how this new DTS profiler performs better than simple two point measurements routinely done by standard BR stations.

We understand the concerns you have, and agree that DTS measured profiles could have more information in them. The main goal of this manuscript however, is to improve on the previously published research done by Euser et al. (2014) on a few points, and to take care of unanswered questions that arose from that publication. It is part of ongoing improvements and an exploration of the possibilities of DTS. In this case we chose for measuring in the forest due to the increased complexity and difficulty that can arise from such a measurement site, while flat land surface could have shown more conclusive results. Additionally, while measured profiles could have more information in them, we only studied the Bowen ratio above the canopy, and used fibre optic cables with a diameter of 6 mm. This large diameter causes them to respond slowly to temperature changes, which averages out events with timescales smaller than 2 to 3 minutes.

Specific comments:

p 3 l 5: it is unclear how eqs. 2 and 3 were implemented with the continuous temperature and vapor pressure profiles (unlike the standard 2 points of classical BR)?

The gradients of temperature and vapour pressure were calculated from the measured DTS profiles by fitting a logarithmic profile to them. From this fitted profile the gradient was calculated. The exact implementation of equations 2 and 3 is explained in section 3.2: Data Processing.

p 5 l 20: how was the thermal energy input by the water supply considered in the DTS measurement?

We supplied the water at the top of the cable. As this water is warmer than the wet bulb temperature, we removed the top two meters of the wet cable from the analysis. In these top two meters the water have reached the wet bulb temperature while slowly flowing down. We will explain this more clearly in the revised manuscript.

p 8 l 10: I fail to see the value of using a DTS profile if at the end one invokes a logarithmic profile (an assumption) to fit to a subset of the data for inference of the real temperature and humidity profiles.

I don't understand the basis for Flag 1 (eq. 20) – why should the instantaneous vapor pressure gradient always fit a logarithmic profile (such a profile is a product of significant averaging in the first place).

I understand the origins in the MOST assumptions, but these are supposed to be direct measurements that reflect what occurs in the profile. I would expect far more information from the fluctuations than this conformity to the "standard" MOST assumptions. What is the need for a profiler if one assumes a logarithmic profile and then fits it to 2 points?

We used 15 minute average data for the Bowen ratio to have stable profiles, as the DTS measurements have an inherent noise in them. We did not calculate the averages to measure out "noise" in the air temperature itself. The Bowen ratio was only calculated above the canopy (38.5 to 44 m height, as it is not valid in the canopy, and there are questions about its validity under the canopy). The rest of the profile was used to study the storage of heat and water vapour in the air column.

To measure events in the temperature profiles (such as convective bubbles) we would need to use a much thinner fibre optic cable, and measure at 1 Hz (the machine limit). At this scale, the DTS measurement noise would have a standard deviation of 0.34 K. However, as we measured using 6 mm fibre optic cables, all the small fluctuations are averaged out.

The basis of the logarithmic profile is indeed MOST. As we looked at averages over 15 minutes, the measured profiles approached this logarithmic shape. Due to the DTS measurement noise and the very small gradients, fitting the profile to many points over the heights gives a more accurate estimate of the Bowen ratio.

As for quality flag 1; the reason we assumed the logarithmic profile is to account for changing conditions. If during the 15 minute averaged period the conditions changed (gradient changed due to the switch from unstable to stable boundary layer, rainfall started, the wind direction shifted significantly), the Bowen ratio is not valid over that averaging period. We will add this explanation to the improved manuscript.

p 12 l 15: just stating that the fetch were no equal is incomplete, what does this mean? what was done with this information? The setup leaves too many ambiguities in both the BR and the EC (considering energy closure and other mismatch issues)

During our measurements the EC system had a 80% footprint of 200 to 300 m. If we use the findings of Stannard (1997), the Bowen ratio 80% equilibration ratio after a large step change would be reached at a fetch to height ratio of 20 to 40, this corresponds to 350 to 700 m. However, there is no (large) step change in available energy or Bowen ratio near to our measurement site (as it is mixed forest in all directions, for at least 1100 m), which could make the 80% equilibrium ratio distance much smaller.

As for the energy balance closure; on average during our entire measurement period the difference in available energy between EC and BR is around 3.4 W m^{-2} (p. 14 – Fig 6). This could indicate that while the net radiation is representative for the EC fetch, the storage terms are not fully accounted for. As the biomass heat flux was not measured, it could account for over half of the current error.

A few general technical comments: *- I also wonder about the fundamentals of the measurement itself: (1) the boundary conditions for the wet bulb mass exchange (summarized in the psychrometric constant in eq. 10) are different at 0 and 40 m (the boundary layer around the wet and dry DTS cables due to different wind speed and other factors). This is somewhat related to the comment in page 12 line 10 but not only for the turbulent transfer of the two quantities heat and vapor in the air, but also for the inferences made at the two locations say 0 and 40 m regarding the wet bulb temperature (it is a bit subtle, I admit. . .). It is possible that the psychrometric "constant" which we take for granted as being constant, is different at the two elevations, because the evaporative cooling behaves differently (I am not even entering into the question if the resistance to vapor transport from cloth is important or not). Hence, separating the Bowen ratio estimate to two independent profiles for vapor and temperature may not necessarily be a good idea. . . (I don't know for sure, I simple raise a possible issue that you have listed as an "advantage")*

The Bowen ratio was only applied above the canopy. The profiles themselves can indeed suffer from an error due to different ventilation above, in, and under the canopy, but the effect of this on the results would be subtle (seeing as the differences between the wet cable and the reference sensor under the canopy where small (Fig. 4f)).

The psychrometric “constant” itself is mainly dependent on the air pressure (assuming the resistance to vapour transport from the cloth is insignificant), and influenced by the wind speed if the ventilation of the wet sock is insufficient. For the evaporation of the water from the cloth, the transport regime will change depending on the wind speed (either dominated by forced convection or a combination of forced convection and natural convection). Harrison and Wood (2012) studied the effects of wind speed on the psychrometric “constant”, and found that it scales exponentially with low wind speed.

- I think that you need to resolve the issue of water input energy to the system – for example, by applying a pulse of water during which you don’t measure and then, after liquid and energy relaxation, you may measure with confidence the entire profile without the water supply “holes” you now have.

The water input was located only at the top of the cable; as such there are no ‘holes’ in the data, just the top 2 m of wet temperature data (44 – 46 m) is affected. We will mention this more clearly in the setup section. The main gap in the profile is the canopy, where the cable heats up significantly due to solar radiation, but where no radiation shielding was located during the measurement period. Applying water in pulses would be possible, but we chose for continuously supplying the water as that is easier to set up and maintain in the field, and makes data processing much simpler.

- An important and potentially interesting feature of the proposed method is to capitalize on the observed profiles and deduce how fluxes and near surface interactions actually work

Thank you for the suggestion. We are indeed working on capitalizing on the complete observed profiles, and using them to study other processes in the forest ecosystem. An animation of some of the profiles is available on https://www.youtube.com/watch?v=7Iw4L0fFXjc . Due to the slow response of the 6mm cables we used in this publication short timescale processes were not visible. We also plan to use additional thin fibre optic cables to study processes which happen on smaller timescales.

References Stannard, D.I.: A Theoretically Based Determination of Bowen-ratio Fetch Requirements, Boundary-Layer Meteorology (1997) 83: 375. https://doi.org/10.1023/A:1000286829849

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