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Interactive comment on "Decoupling of a Douglas fir canopy: a look into the subcanopy with continuous vertical temperature profiles" *by* Bart Schilperoort et al.

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General evaluation: The manuscript deals with the exploration of canopy decoupling using the relatively new technique of temperature distributed sensing (DTS). As decoupling is a phenomenon relevant for each canopy and no standard method exists yet how to deal with it, the manuscript addresses a highly relevant scientific question using a novel approach. It fits well within the scope of the Journal Biogeosciences. The manuscript is well structured and written, easy understandable and conclusions are derived in a traceable manner. The presented results are sufficient to support the interpretations and conclusions. The title clearly reflects the content of the paper and

the abstract provides a concise and complete summary.

I have, however, three major suggestions regarding the current paper version:

It remains somewhat unclear to the reader, how the temperature error derivation procedure, obtained in a completely different environment than the measurement site, was transferred to the final setup. Please explain in more detail how you applied the error derivation procedure on the final data. Furthermore, it would be valuable to add few more words to the measurement principle of DTS itself to get information how the temperatures are obtained with this technique.

It would be great to set your whole work in a bit bigger context. There was already quite some work done on the topic decoupling, several different approaches developed. I miss the discussion of all the already existing work in the introduction. Once this is done, you can place your work in this context and explain how your work provides additional gain of knowledge in the context of the existing work.

I suggest to make use of all the data you have. If I understood this correctly, you have a sonic anemometer measuring within the canopy during the presented measurement period. Why not using these data too? With these data you can apply the approach by Thomas et al. (2013) who are assessing decoupling based on the relation of σw above and below canopy, and compare these findings with your DTS data. This would give much additional value to your work. Furthermore, at one point you are mentioning advection and that you cannot assess it: I think you can. With the DTS data you can derive the buoyancy forcing which gives an indication regarding the potential of drainage flow near the surface. With the sonic anemometer you get wind direction and speed. Both quantities combined give you a clue, how important/relevant advection at your site could be (see Staebler and Fitzjarrald, 2004; 2005. Also Fig. 2 in Jocher et al., 2017).

Specific comments:

In the abstract (Line 10 etc.) it would be good to tell how you define decoupling. Which threshold of what you used for distinguishing between coupling and decoupling.

Lines 39 – 41: I don‘t think that you can say this generally, that decoupling occurs predominantly during daytime, while coupling during nighttime. It‘s rather the other way around. T profiles may indicate that, but the T profile is only one part of indicator for coupling or decoupling. The "nighttime" problem, i.e. underestimation of above canopy CO_2 fluxes due to low turbulence and decoupling, is not called like that without reason (see e.g. Aubinet et al., 2012).

Lines 43 – 50: extend this part with the most important work and approaches on decoupling. Discuss also the implications of decoupling on above canopy derived fluxes bit more.

Lines 50 – 59: Great. This to compare with decoupling assessed by the correlation of σw above and below canopy would be very valuable.

Lines 97 – 98: this refers to the understory measurements I assume?

Line 100 etc.: explain briefly the measurement principle of DTS.

Lines 122 – 127: how was this done in reality? You were grouping your data according specific conditions and applied then the error estimate on them which you derived from the reference setup? Explain this in detail.

Line 131: Best quality fluxes are fluxes with flag 0 only. Fluxes suitable for standard measurement programs are fluxes with flag 0 or 1. Specify.

Line 135: you introduce here the sonic. Why not using the data of this sensor?

Lines 205 etc.: how about counter-gradient fluxes? Fluxes against the gradient are possible, discuss this.

Lines 225 – 226: this is not possible to say in this way, that your u* threshold corresponds well with previous decoupling research, no proper justification. The u* threshold

is strongly site specific, and at certain sites it is even not possible to derive it.

Lines 245 – 246: somewhere else in the manuscript you are saying that radiation reaches the forest floor and heats it due to sparse vegetation, somehow this is contradictory.

Line 249: why restricting here the analysis on nighttime cases? In the introduction you are stating that decoupling occurs predominantly during daytime. I think it would be useful to make this analysis in 3.3.3 for both nighttime and daytime.

Line 277: you mention here that it would be interesting to explore the impact of understory stratification on the friction velocity threshold value by assessing effects of conditional sampling. Why not doing it here in this study?

Line 283: you are stating here that information of understory wind speed is lacking. But you have a sonic anemometer measuring in the canopy, so you would have this information ready. An analysis here combing the buoyancy forcing derived from DTS with wind speed and direction from the sonic anemometer can give you insights in potential drainage flow within the canopy.

References to consider:

Aubinet, M., Feigenwinter, C., Heinesch, B., Laffineur, Q., Papale, D., Reichstein, M., Rinne, J., van Gorsel, E., 2012. Nighttime flux correction. In: Aubinet, M., Vesala, T., Papale, D. (Eds.), Eddy Covariance: A Practical Guide to Measurement and Data Analysis. Springer, Dordrecht/Heidelberg/London/New York, pp. 133–157.

Staebler, R.M., Fitzjarrald, D.R., 2004. Observing subcanopy CO_2 advection. Agric. For. Meteorol. 122, 139–156.

Staebler, R.M., Fitzjarrald, D.R., 2005. Measuring canopy structure and the kinematics of subcanopy flows in two forests. J. Appl. Meteorol. 44, 1161–1179.

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