The referee comments are copied in *blue*, our reply is in black

---

Dear Georg Jocher,

Thank you for your comments on our manuscript. Based on your comments we hope to clarify what is unclear and improve the quality of the paper.

> *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.*

Our intention was to get an order of magnitude estimation of the error caused by radiative cooling of the fiber optic cable. While the environments were indeed completely different, we transfer the results by looking at similar meteorological conditions (i.e., low wind speeds and a net longwave radiation < 20 W m^{-1}). Under those conditions the error in the gradient can be expected to be in the order of 0.01 K m^{-1}, an error we deemed to be acceptable. We did not correct or adjust the measured gradients, but left them as is. We will add a more detailed explanation when revising the manuscript.

> *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.*

We will add an explanation of the measurement principle of DTS to the setup section.

> *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.*

We will expand the introduction to properly put this work in the bigger context, discussing, e.g., *u** filtering, *sigma_w* correlation, telegraphic approximation of *w*, and cross-correlation maximum between above and below canopy *w*.

> *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.*

We did place a sonic anemometer at the bottom of the tower, but it only worked for a very short period of time before the equipment failed. I see now that this is currently not clearly explained in the manuscript. You are correct that if the data were available it could have added a lot of value to this 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)*

This would be a great addition and we will mention this possibility for future research, but sadly, without the data from the sonic anemometer (lacking as previously mentioned) we can not do this analysis.

> *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.*

We will specify that we used the aerodynamic Richardson number to distinguish 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).*

We will change this sentence, and specifically mention the 'nighttime problem'.

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.

In the revised manuscript will will extend this part of the introduction and place this work in a better context.

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

Sadly the understory sonic anemometer failed, and the collected data is insufficient and does not overlap with the DTS measurements.

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

Indeed it does. We will change the sentence to make this more clear and less ambiguous.

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

We will add an explanation of the measurement principle of DTS to the setup section.

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.

We did not correct for any radiation errors, but only used the results from the Cabauw measurements to make an estimation of the possible error or bias in the results. As the error under the expected conditions was only in the order of 0.01 K m^{-1} we regarded this error as acceptable.

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.

We will change this sentence to “These flags represent fluxes suitable for general analysis, …”, removing “are the best quality fluxes”

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

As mentioned before, we did place a sonic anemometer at the bottom of the tower, but it only worked for a very short period of time before the equipment failed. We will state this clearly in the revised manuscript.

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

Counter-gradient fluxes are indeed possible when, for example, the air temperature above the forest floor is higher than the temperature above the forest, but lower than the temperature within the overstory. We will add a sentence here to discuss counter-gradient fluxes.

> *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.*

Will will change this sentence and mention the issues with u* thresholds; “...corresponds to results from …, although the u* threshold is strongly site specific, and it is not always possible to derive a u* threshold.”

> *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.*

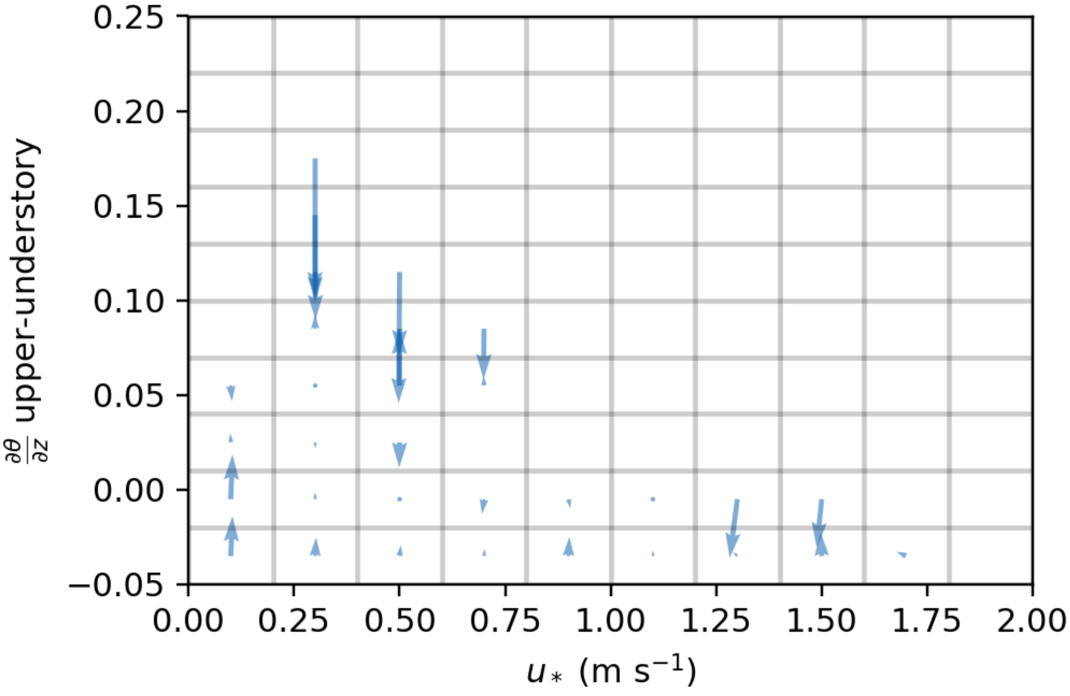
Very little light penetrates the canopy to reach the forest floor. Some can filter through to warm up the forest floor slightly, but this is only a small fraction of the total incoming sunlight.

> *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.*

For section 3.3.3 we restricted the analysis to nighttime cases as the underlying assumptions of the aerodynamic Richardson number we used are not valid for daytime conditions. The friction velocity will be strongly affected by turbulence generated by convection from the top of the canopy, and is therefore not a good measure of the wind shear mixing the under-story from the top down. The suppression of mixing by the stable stratification of the understory is also not included into the aerodynamic Richardson number.

> *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?*

Due the inconclusive results and lack of data we chose to not include this analysis in the manuscript. As a demonstration the plot below shows the mean vector in every bin; i.e. where the next data point (in time) would be. The bins are denoted by the gray boxes.



With the data available some slight patterns can be seen, but the uncertainty large and there is still a lack of sufficient data to get a clear pattern. If more data were available more filtering could be performed, e.g., for clear sky conditions, and a more conclusive picture could form.

> *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.*

As mentioned previously, the sonic anemometer only worked for a short period of time before failing. Without this data I think we can not easily get a better insight in the drainage flow within the canopy.