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

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> *This study uses temperature profiles with high vertical resolution measured within and above the canopy to investigate the issue of decoupling between the air within the canopy and the air above. This investigation is highly relevant, given that a decoupled situation will have significant impact on the exchanges of matter and energy between the canopy and the atmosphere, and it needs to be taken into account when interpreting field data. This type of temperature profile measurement is less common, and this study contributes to the assessment of its usefulness. The manuscript is clear, well-motivated and well-written, being suitable for publication at this journal. However, I have three observations that I believe would help improve the impact of the manuscript.*
>
> *First, I believe the study should be more precise in the definition and discussion of decoupling. Right now "decoupling" is defined in terms of the aerodynamic Richardson number, but throughout the text the term "decoupling" is used much more broadly, sometimes related to local static stability. This can be confusing and misleading in the conclusions.*
>
> *The second issue is related to the analysis of u* as an indicator of decoupling. In this specific analysis, I don't agree with the interpretation of the data and the conclusions (see details below). If a more precise definition of decoupling is used, maybe this analysis won't be needed.*
>
> *And finally, I believe that having a temperature profile instead of the typical point measurement of temperature should betaken more advantage of. Right now, the local stability and the convection height analyses are great examples of that, very interesting. But the temperature difference and aerodynamic Richardson number analyses use point temperature measurements, as in previous studies. I believe that here there is a big opportunity to improve these definitions, taking advantage of the profile measurements, as point measurements might not be the best reference of the entire layer temperature or even the decoupling. Everything that is happening between the two point measurements might be impacting the coupling state, and you should take advantage of having this information, convincing the reader that performing profile measurements are relevant (or not), compared to point measurements. Overall, I believe that the current static stability and convection height analyses, combined with a new temperature difference and decoupling definition that uses the temperature profiles, would provide a better analysis of decoupling and a more convincing discussion on the potential of temperature profile measurements.*

Dear referee,

Thank you for taking the time to look at and comment on our manuscript. We will try to be more clear and consistent with our use of decoupling, and not use it in relation to just the static stability. As point two and three return in the specific comments, we will reply to them there.

Specific comments:

1. l. 50: “Instead of considering discrete point observations along the height of the canopy, we search for a more continuous probing of temperature to get a more detailed view on decoupling along the entire height of the canopy.” Can you elaborate more this paragraph? Can you discuss, for example, if previous studies on coupling have always used discrete observations, and if no study of coupling with continuous measurements have ever been done before? Maybe investigate the use of continuous measurements on decoupling of other atmospheric regions? I believe this should be the focus of the manuscript, and being more explicit would enhance the impression on the importance of the study.

Continuous measurements in the atmosphere have been performed with different equipment on a much larger scale (e.g. radar or sodar wind profiling). Within canopies we are not aware of any previous studies with continuous measurements, although some field campaigns have had a very high density of instrumentation, e.g., the Canopy Horizontal Turbulence Array (CHATS).
In the revised manuscript we will expand the paragraph and discuss previous studies in more detail.

2. Sec. 2.2: measurements other than temperature profile and u were not used in this study (eddy covariance, ground and biomass heat flux, etc). Could they be used to infer coupling/decoupling? Maybe mention that they were present in the experiment, but not used here*

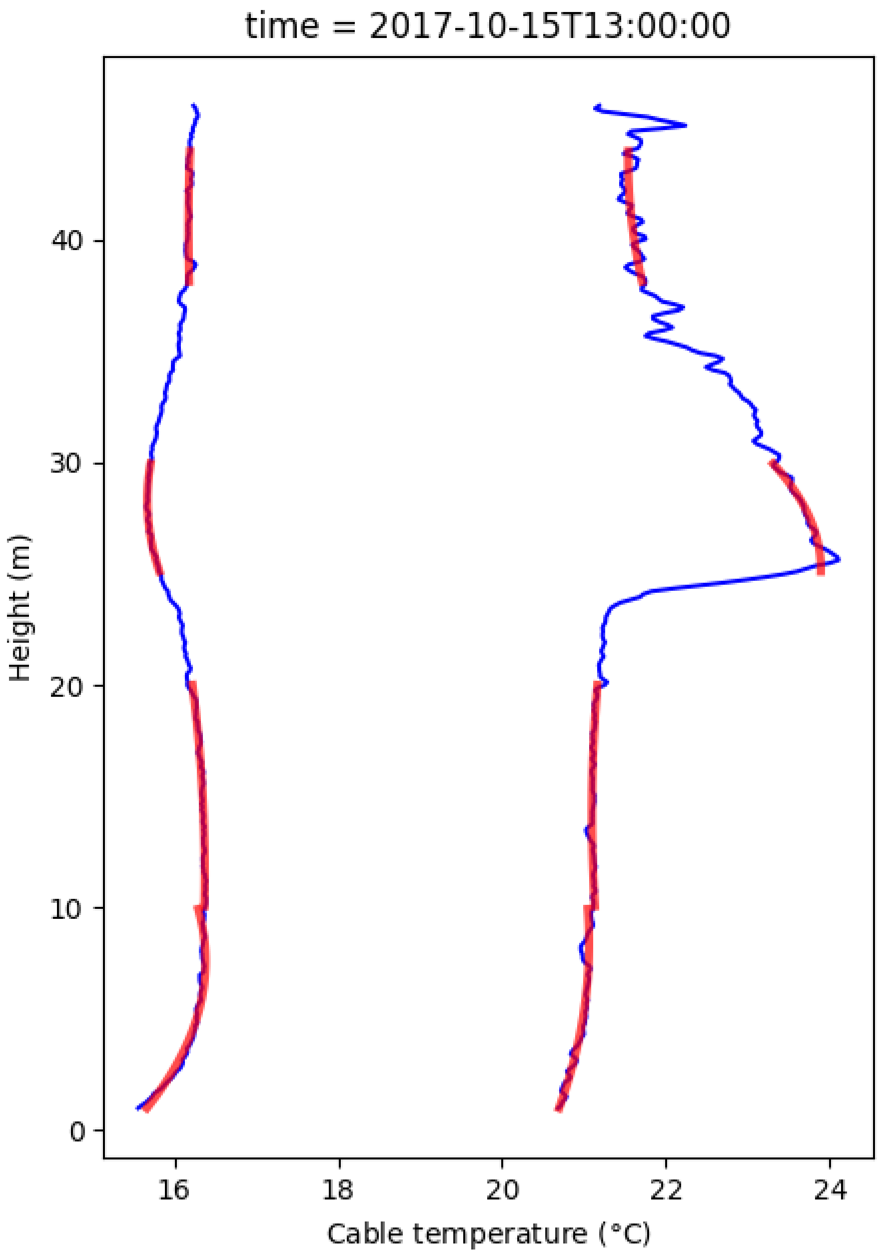
The other measurements are indeed not used in this study, except for the general characterization of the energy balance in the understory. I do not think the ground and biomass heat fluxes can (easily) be used to infer decoupling. If all energy balance components could be measured without uncertainty, coupling/decoupling could be inferred from that, but the uncertainty in these measurements is too high.
We will make it more clear in the revised manuscript that the measurements were present but not used extensively.

3. l. 164: “The height to which the parcel will rise is the height at which the local (potential) temperature $\theta(z)$ exceeds the temperature of the parcel.” Which parcel? In the results it is mentioned the floor parcel, it should be clearer here.

We will clarify this by specifically calling it the ‘floor parcel’;
“The height to which, e.g., a parcel of air from the forest floor will rise is the height at which the local (potential) temperature $\theta(z)$ exceeds the temperature of the forest floor parcel.”

4. Sec. 2.4: regarding the second order polynomial fit, why perform the fit of an analytical solution, but not use it to calculate the gradient analytically? If you are using finite-difference (eq. (3)), why not use it with the original data? How good are these fits? Can you show us some *examples of the fit, to illustrate the level of quality? How about some statistics of the quality of the fit?*

We chose to calculate a polynomial fit through the data points as DTS suffers from (normally distributed) measurement noise. By calculating the fit a lot of this noise will be filtered out, and the resulting temperature gradient can be calculated more accurately.
In the figure below the polynomial fits are added to the data of Figure 4a.



As an indication of the goodness of the fits, we calculated the RMSE of the fit, the mean values are shown in the table below. Note that the RMSE of the fit is also influenced by the measurement uncertainty of DTS.

Profile	Mean RMSE of polynomial fit (K)
Above-canopy	0.095
Overstory	0.087
Upper-understory	0.120
Lower-understory	0.112
Forest floor	0.022

In the revised manuscript we will shortly discuss the goodness of the fits in the Data Processing section.

> *5. Sec. 2.4: you should emphasize that humidity effects are not taken into account(probably due to the lack of data) but that they could be relevant in this environment (if they are).*

Humidity effects are indeed relevant, and could assist in transport from the forest floor or understory to the atmosphere above. In this study we looked at the potential temperature only taking into account the dry adiabatic lapse rate. We will emphasize this in the revised manuscript.

> *6. l. 175: “For the calculation of the aerodynamic Richardson number, we used the 10 m DTS temperature as the canopy internal temperature and the 44 m temperature as the top-of-canopy temperature.” Why those specific heights? Shouldn’t you take advantage of the fact that you have an entire profile?*

The 10 m temperature is in the center of the understory, and therefore represents the general temperature of the understory well. It could be possible to use a sample of different heights, or a profile integrated temperature but this will not change the results significantly.
The 44 m temperature was chosen as this is close to the sonic anemometer which provides the data for u*.

> *7. Figure 4: can you add the polynomial fit in this figure as an example?*

Figure 4 with the polynomial fits added is shown in the answer to question 4. We prefer not to add the fits to the manuscript as it would distract from the goal of Figure 4.

> *8. Sec. 3.3: “Dynamic and static decoupling” in the methods section you defined dynamic and static “stability”, and mentioned “decoupling” only in the dynamic sense. Can you elaborate on the idea of “static decoupling”? Is it the same as “static stability”?*

This is indeed not clear. We will change the section title to “Influence of dynamic and static stability on decoupling”

9. Sec. 3.3.1: I believe the comparison between u and local temperature gradient is difficult due to the complex dynamics and the "cause" versus "consequence" misleading interpretation. I believe that as a first order approximation, we could think of u* and surface heat flux as causes, and temperature gradient as a consequence. But the temperature gradient will also impact u* and local heat flux. For that reason, comparing u* and temperature gradient directly can be misleading. For example, we can have high shear destroying temperature gradient (as discussed in this section), but we can also have low shear and low surface heat flux resulting in (and being a result of) low temperature gradient. It is a complex interplay and I don't think that looking for a threshold value is appropriate.*

The data in Fig. 7 a, b, c has more of a "L-shaped" curve than a proper "negative-correlation". It shows that at high shear it is impossible to sustain a large temperature gradient, but low-shear is actually concomitant with small and large temperature gradients."At low shear conditions the top of the canopy is able to cool considerably, causing strong local gradients to occur." strong local gradients can occur, but will not necessarily occur.

"Interesting, the understory gradients (Fig. 7b, c) show a characteristic behavior with a kind 'threshold' value for u: below u*=0.4 large gradients tend to occur, while small gradients are observed for large u*" I don't agree with this interpretation. Below u*=0.4 in Fig. 7 b, c most of the data has small temperature gradients.*

"The forest floor is unstably stratified when u is low, and stably stratified when u* is high", again, I believe there is too much dispersion in the data for this affirmation. "The strong relationships between the understory gradients and friction velocity show that the temperature gradients can serve as a proxy for decoupling; when the friction velocity is low the understory is strongly stably stratified." as I said, I don't think there is a "strong relationship", and when the friction velocity is low the understory can be strongly stably stratified, but it won't be most of the time (I believe, based on the density of points in the figures).*

I suggest you improve this analysis and be more conservative in the discussion. If you want to keep this analysis (which I'm not sure it is needed), maybe you can use the thresholds of stability and the chosen thresholds of u and count the number of occurrences in each category, providing a proportionality analysis such as the one in Fig. 5. Also add lines for those thresholds in Fig. 7 to help the visual interpretation of the data.*

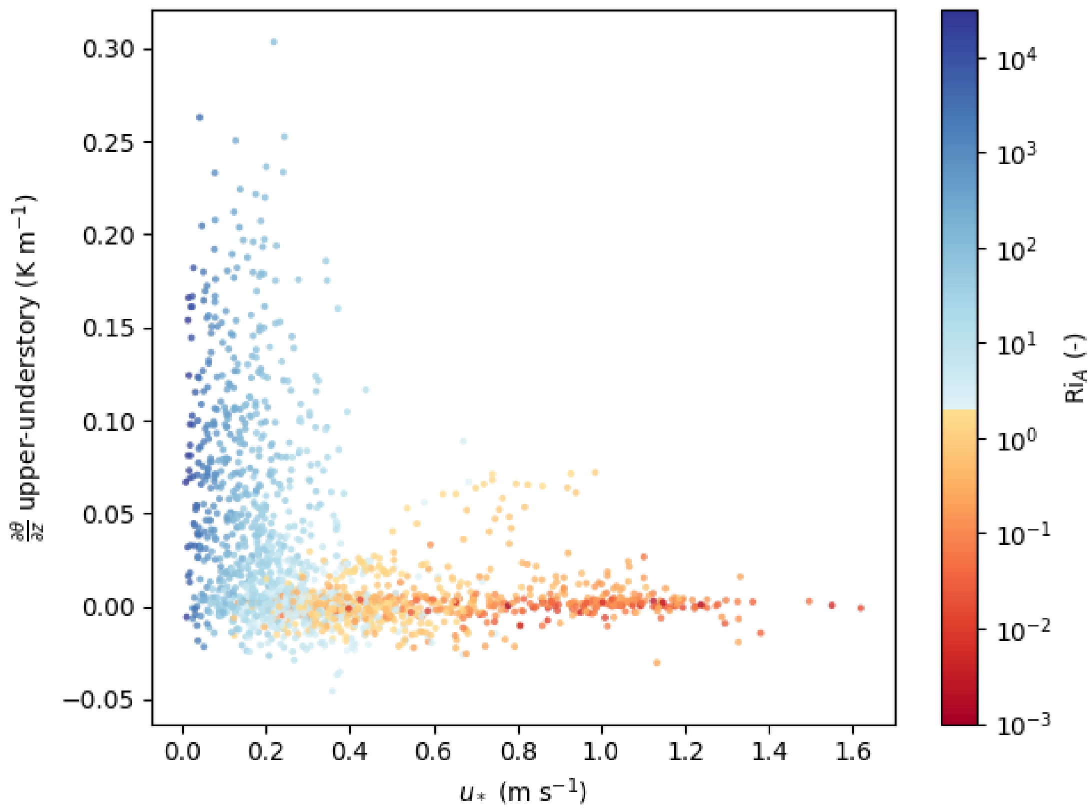
Indeed strong gradients will not necessarily occur. Besides u*<0.4, the conditions have to be right to allow for cooling of the canopy (i.e., clear skies). We will make it more clear in the revised manuscript that u* alone is not enough to discern decoupling, and refrain from calling it a hard threshold.

In our answer to your comment #10 we have added a plot which shows the two regimes (coupled and decoupling), and them overlapping in the lower left corner of the plot. This shows that even if u*<0.4 the canopy can indeed still be coupled.

We will revise this section and will more more conservative and clear in our interpretation.

> *10. l. 230: "However, the understory can still be dynamically decoupled even without strong thermal stratification, as shown by the data points in the lower left corners of Fig.7b, c. It is likely that at very low friction velocities the wind will not be able to mix the canopy even though there is no strong temperature gradient (e.g., low wind, overcast conditions)." How do you know about the level of dynamical coupling from this analysis? You defined dynamical coupling from RiA, but it is not used here. How do you know that the data points in the lower left corners are dynamically decoupled? Can you be more precise in the definition of "dynamically (or statically) decoupling", and include that in the figure? Maybe it will correspond to a region of the plot, maybe it will be a third variable, that can be added as colored dots in the plot.*

Thank you for the suggestion to add RiA to the plots as colored dots. In the image below you can see the result for the nighttime temperature gradient in the upper-understory (i.e., the data of Figure 7b).



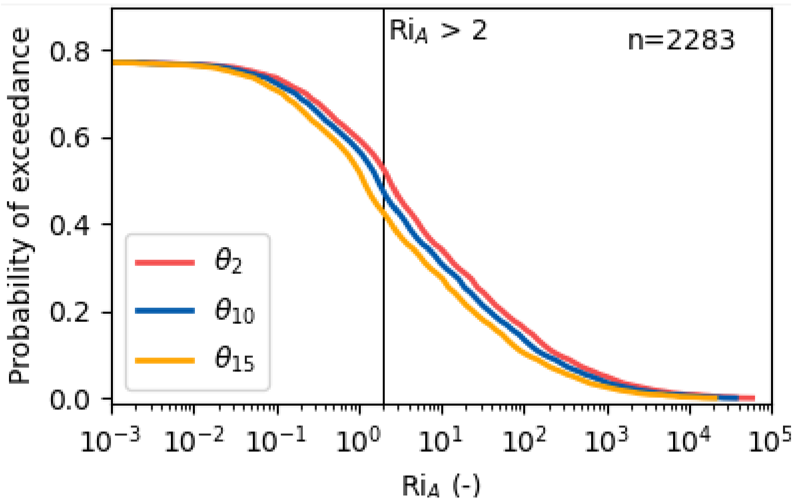
In the figure the color jumps at RiA=2; which was the decoupling threshold found by Bosveld et al. (1999). The two regimes (either coupled or decoupled) are very clearly visible. The regimes overlap around dθ/dz=0 and u*<0.4.
We will add the RiA color coded dots to Figure 7a,b,c,d, and discuss this in the revised manuscript.

11. l. 234: "While at night turbulent mixing is driven by wind shear (hence friction velocity), during daytime convection is also important for generating turbulence." Do you mean above the canopy?

Yes, we mean convection at the top of the canopy and above. We will make this more clear.

12. Sec. 3.3.2 and 3.3.3: These analyses use temperature values defined at specific heights (44, 10 and 2m) to compare temperature differences within and above canopy, and to define an aerodynamic Richardson number and decoupling. This was done as in a previous study at the same site (Bosveld et al. 1999), and although I think the direct comparison is useful and should be kept, I believe these analyses are not taking advantage of the temperature profile available. Could you replace these definitions by a more well-defined temperature difference (maybe some bulk or integrated temperature within each region) and to use a Richardson number that takes advantage of the temperature profile, or a decoupling definition that takes into account the information of the entire canopy? I believe that the definitions used by Bosveld et al. (1999) were chosen due to the data availability (point temperature), and here you have the opportunity to use a much more complete information with the temperature profile. Maybe there is a more suitable decoupling definition that takes into account the stability of the entire region(maybe in the literature about other parts of the atmosphere where temperature profiles are typically measured), something in the lines of the convection height analysis done here.

We can change the temperatures involved to the bulk/integrated temperatures for each region (e.g. forest floor and lower understory), however this will not impact the results in any significant way. To illustrate this the image below shows Figure 9, with added data of the 2 m and 15 m temperatures instead of only the 10 m temperature. While there are differences, these are not very large.



A decoupling definition that takes into account the entire temperature profile does sound very interesting, but we are not entirely sure on how to approach this.

13. l. 253: “According to Bosveld et al. (1999), decoupling occurs when the aerodynamic Richardson number exceeds approximately 2.” Since this decoupling criterion is used here, it is important to explain how it was obtained in the original study, and why it is also applicable here. It would be interesting to add that discussion to a definition of decoupling in the Methods section.

We will add an explanation to the Methods section on how Bosveld et al. determined the critical aerodynamic Richardson number.

Technical corrections:
Sec. 3.3.2: “Temperature difference subcanopy” improve title

We will change the title to “Magnitude of the temperature difference between the subcanopy and atmosphere”

l. 306: “aN open subcanopy”

Thank you. This has been corrected.