

Manuscript: Title: Suitability of fiber-optic distributed temperature sensing to reveal mixing processes and higher-order moments at the forest-air interface

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We thank both referees for their comments. Please find below point-by-point responses to the presented critique. Referee comments are in **bold**, responses with **red** and changes to the manuscript with **blue**.

## **Anonymous Referee #2**

**The manuscript entitled “Suitability of fiber-optic distributed temperature sensing to reveal mixing processes and higher-order moments at the forest-air interface” by Peltola et al. discusses the applicability of distributed temperature sensing (DTS) at a forest site for evaluating the mixing and higher-order moments across the forest canopy and above the vegetation. The distributed temperature sensing technique is a very interesting, relatively new, approach to get more insights into the mentioned topics, consequently, this paper is definitely answering relevant scientific questions within the scope of AMT. The experiment description is sufficient, methods and assumptions are valid and clearly outlined and the paper comes to substantial conclusions. The title reflects the content and the abstract gives a concise and complete summary of the work presented. The paper is clearly structured and well written, hence, it is possible to follow the drawn conclusions. Before this work will be published, however, I would like to ask the authors to address the following list of specific comments on their work**

**RESPONSE:** We thank the referee for this positive comment.

**Abstract, L6:** replace “quantified” with “assessed”.

**CHANGES TO THE MANUSCRIPT:** Done

**P4L26:** what is the range of the Lidar? From which height until which height it is giving measurements?

**RESPONSE:** The Doppler lidar provides data from 75 m to 9585 m in range with 30 m spatial resolution and the clearest signal typically originates from the boundary layer where the aerosol loading is the highest. The instrument is situated on the roof of a 5 m high building so that the first measurement height is at 80 m above ground.

**P7section2.2.1:** the noise is quite high, I would say. Frequencies of noise and turbulence can overlap; how do you separate the signals then? Elaborate this maybe a bit more here in the manuscript to give proof to the reader that the procedure you are applying yields the presented results.

**RESPONSE:** The noise level can indeed be quite high when compared to the signal. The technique to separate signal variance from noise variance is based on the assumption that the noise is white and uncorrelated with the signal. In such case power spectra of noisy measurements contain two

independent components (see Fig. 3 in the manuscript) and noise contributes to the autocovariance only at lag zero, which enable the separation of the effect of these two components (noise and signal) on different statistics (variance, skewness). Ability of the technique to estimate the noise contribution decreases as signal-to-noise ratio decreases (see Fig. 6 in the manuscript). Based on referee suggestion we will add a note about prior studies utilizing the technique, but opt not to dwell on the issue since the technique is already discussed and validated in several other papers (Langford et al., 2015; Mauder et al., 2013; Nakai et al., 2020; Nemitz et al., 2018; Peltola et al., 2014; Rannik et al., 2016). Hence addition of short text and reference to some of the prior studies should suffice.

**CHANGES TO THE MANUSCRIPT:** Added the following sentence on p7113: “The method has been shown to reliably estimate the influence of noise on second- and third-order statistics in turbulence measurements in prior studies (e.g. Lenschow et al., 2000; Rannik et al., 2016; Nakai et al., 2020).”

**P12L11 and P14L3:** at the first place you are stating that organized slow motions are dominating within the canopy, at the second place you are saying that small eddies are dominating since the coherency is broken up: this is contradictory, please revise.

**RESPONSE:** Thanks for noting this inconsistency. Please note that the attenuation factors in Sect. 3.2 refer to the attenuation of temperature variance measured with DTS, whereas text on P14L3 refer to the below-canopy vertical turbulent transfer of temperature. In general different size of eddies contribute differently to scalar variance and vertical turbulent flux (compare scalar power spectra and cospectra e.g. in (Kaimal and Finnigan, 1994)) and this discrepancy between scalar power spectra and cospectra is likely accentuated in below-canopy flows (Vickers and Thomas, 2013, 2014). In order to clarify the text, we will add a note on P12L11 that this part refers to variability of scalars only (and hence variance) and emphasize on P14L3 the influence of horizontal sensor separation rather than the characteristics of below-canopy turbulent transfer.

**CHANGES TO THE MANUSCRIPT:** changed “variability” on p12L11 to “scalar variability (and hence variance)” and changed the order of 1) and 2) on p14 and modified the text related to former 1) as “forest floor is close which suppresses the size of eddies dominating heat transfer which is also influenced by the canopy elements breaking the coherency of large eddies (spectral short circuit; Finnigan 2000; Launiainen et al. 2007) and DTS cannot capture the small scale turbulence”. Furthermore, we continue the sentence ending at p14L10 with “and large horizontal sensor separation”.

**Fig6 and other places:** you are using the temperature from the sonic anemometers. I guess you converted sonic temperature in “real” temperature before use, state this somewhere. General question to the use of temperatures derived via sonic anemometer: they are not very accurate when it comes to absolute numbers, how did you deal with this? For prospective studies it might be more feasible to use profiles of e.g. thermocouples for comparison with the DTS. I also wonder about the Gill sonic anemometer: did you have any issues with this sonic regarding noise? I heard from several cases that the Gill sonic type you used has a problem with noise, also have personal experience with that issue. How did you deal with noise in the sonic data, if you observed it?

**RESPONSE:** In fact sonic temperature was not converted to air temperature since the H<sub>2</sub>O concentration fluctuations needed for this conversion were not available at most heights (sonic temperature is almost equal to virtual temperature, which requires vapor and barometric pressures for computation of dry bulb ‘normal’ temperature). We failed to mention this deficiency in our measurements but will add it to Sect. 2.2 and also to Sect. 3.3 where DTS statistics are compared

against 3D sonics. This causes a minor bias in temperature variance (mainly depending on H<sub>2</sub>O variance) and  $w'T'$  covariance (depending on  $w'H_2O'$  covariance) calculated from the sonic data (Foken et al., 2012). The referee is right the absolute numbers from sonic anemometers are not accurate, but fluctuations around the means are precise. However, please note that we did not use absolute temperatures from the sonics in DTS validation, but the mean T profiles from DTS were compared against slow-response thermometers (see Sect. 2.1) which should be accurate. We did not estimate noise in sonic data with the method presented in Sect. 2.2.1. However, based on power spectra (Fig. 3 in the manuscript) and visual evaluation of sonic raw data (see examples in Figs. 8 & 10 in the manuscript) the noise was only a minor component of the sonic anemometer measurements.

CHANGES TO THE MANUSCRIPT: Added the following sentence to p5131: "Note that as H<sub>2</sub>O fluctuations were not measured at most heights, it was not possible to convert the sonic temperature to actual temperature (Schotanus, 1983; Foken et al., 2012). This caused a slight H<sub>2</sub>O variance and H<sub>2</sub>O flux dependent bias in temperature variance and  $\overline{w'T'}$  covariance, respectively." The following sentence was added to p12115: "Note that statistics derived from 3D sonic anemometers were calculated from sonic temperature and hence slightly biased by H<sub>2</sub>O fluctuations (see Sect. 2.2)."

**Fig7: reformulate the figure caption in such way that it gets very clear that you applied the Cava parameterization only for unstable conditions: i.e., "For unstable conditions, was also calculated based on Monin-Obukhov....."**

RESPONSE: Thanks, will be modified.

CHANGES TO THE MANUSCRIPT: replaced " $\sigma_T / |T^*|$  was also calculated based on Monin-Obukhov similarity theory (dashed lines) for unstable stratification using  $\sigma_T / |T^*| = 2.3 (1+9.5|\zeta|)^{-1/3}$  (Cava et al., 2008)" with "For unstable conditions  $\sigma_T / |T^*|$  was also calculated based on Monin-Obukhov similarity theory (dashed lines) using  $\sigma_T / |T^*| = 2.3 (1+9.5|\zeta|)^{-1/3}$  (Cava et al., 2008)"

**Fig8: the arrows are very hard to identify.**

RESPONSE: Thanks, we will try to improve the figure. However, we struggled with the arrows already before submission and the figures in the manuscript were the best that we were able to come up with. Hence, big improvements in the figure are not likely, but we will try our best. The arrows help to illustrate the connection between the observed large temperature structures and wind fluctuations at different heights and hence we would like to keep them in the figure despite being somewhat unclear.

**P17: regarding vertical coupling: I would love to see here a comparison between the conclusions regarding coupling/decoupling which you derive with DTS and other approaches, e.g. the correlation of standard deviation of vertical wind (cf. Thomas et al., 2013). This does not have to be a complete analysis, this is beyond the scope of your paper, but a figure comparing coupling/decoupling conclusions derived via DTS and the mentioned correlation of  $\sigma(w)$  would be very valuable, also for prospective work in this area. You have the required instrumentation there, a couple of 3-D sonics in profile.**

RESPONSE: Thank you for this comment, this is an interesting idea which we are exploring in a follow-up study. This manuscript deals with validating DTS measurements against conventional measurements at the forest-air interface and the suggested analyses on vertical coupling/decoupling are beyond the scope of this study.

**Conclusions: you touch very briefly the topic advection there; can you write to this a bit more? A vertical profile would not be sufficient for approaching advection issues, but a 3-D array could. How would such a setup in your opinion have to look like to approach advection sufficiently?**

**RESPONSE:** Yes, for properly addressing the question related to advection one likely needs to resolve the whole 3-D flow field within the forest. This is a formidable task for experimentalists and hence the problem related to advection has eluded a complete answer. Due to the spatially continuous measurements along the cables, horizontally and vertically distributed DTS cables would be able to resolve the mean temperature field and turbulent (up to 1 Hz) temperature fluctuations within the canopy. If these would be accompanied with actively heated fibre optics measuring wind (Sayde et al., 2015), the spatial variation of horizontal and vertical heat fluxes could be presumably approximated. This setup would of course miss the spatial variability of gases such as CO<sub>2</sub>, but should still be one step closer to closing the ecosystem mass balance since it would shed light on the spatial variability of turbulent mixing within the canopy. Work is on its way regarding this aspect. We will add a note about this to the conclusions.

**CHANGES TO THE MANUSCRIPT:** Added the following sentence on p22|19: “This issue could be explored with DTS measurements utilising horizontally and vertically distributed fibre optic cables”

### **Anonymous Referee #3**

**Overall, the manuscript presents the usefulness of a new and increasingly important tool with respect to spatial measurements of turbulence and atmosphere-ecosphere interactions. The manuscript is generally well put together but are a few places where some clarification is needed (see minor/technical comments below) to improve the message being presented. It is an interesting concept that adds to the toolbox of measurement techniques beyond the traditional flux tower setup to investigate the underlying turbulence that drives scalar fluxes in different landscapes and glad to see if developed further. Across the manuscript, the spatial extent/direction of the measurement needs to be clear. My only broader comment is in relation to the connection between the flow and temperature variability; the connection between these two values needs to be better supported; there are places where it appears the two variables are used interchangeably and it can lead to a little confusion for the reader (Sections 3.4). This is primarily an organizational issue, not a science issue.**

**RESPONSE:** We thank the referee for acknowledging the value of our study.

**Pg2|n6: “practical point-of-view” meaning a sufficient amount of spatially distributed tower measurements?**

**RESPONSE:** Yes, exactly. Constructing an array of measurement devices that capture all the relevant spatial details of the flow is a big and expensive undertaking. We argue that DTS measurements could help in this respect.

**Pg2|n6: “Hence...” Not sure how many spatial stats are being derived directly from time series observations at a single point; this needs to be clarified; its point is a little convoluted. I will agree that eddy structure, particularly in the vertical, is assumed from time series using Taylor’s hypothesis but I don’t necessarily agree that spatial statistics are being derived through Taylor’s hypothesis.**

**RESPONSE:** Thanks for this comment, this needs clarification, since our wording was not accurate. What we mean with this part is that spatial details (eddy structures etc) of the flow are often evaluated from time series based on Taylor's frozen turbulence hypothesis, like the referee suggests. At the same time, it is often assumed that temporal and spatial statistics converge to ensemble statistics, i.e. the ergodic hypothesis is valid (Higgins et al., 2013) without having the experimental/observational means to verify this assumption. We will modify this part of the manuscript based on this, the previous and the next comment.

**CHANGES TO THE MANUSCRIPT:** The first paragraph of the manuscript (p2l2...p2l10) was rewritten as "The majority of the interaction between the atmosphere and Earth's surface takes place in a shallow air layer termed the atmospheric boundary layer (ABL). Insights on the atmospheric mixing processes in this layer are required in order to gain a better understanding on ecosystem-atmosphere feedbacks, air quality and weather forecasting related issues. Studies near the surface typically rely on time series analysis, since spatial details of the mixing close to the ground are difficult to measure with conventional instrumentation. However, similarity theories underlying the analysis of observations and models are posed in length scales calling for spatially explicit sampling. In addition, turbulence statistics or spatial details of different flow structures are typically derived from a time series of observations by assuming ergodic hypothesis or Taylor's frozen turbulence hypothesis (Taylor, 1938), respectively, yet it is recognised that these hypotheses are not universally valid (Mahrt et al., 2009; Thomas, 2011; Higgins et al., 2012; Higgins et al., 2013; Cheng et al., 2017)."

**Pg2Ln8: "Another motivation..."** feels tacked on; could be better framed to fit within the overall context of the need for more spatially explicit measurements.

**RESPONSE:** Thanks, we will try to improve this part, see above.

**Pg3Ln23: "Furthermore, we evaluate..."** remove "also" from this sentence.

**CHANGES TO THE MANUSCRIPT:** done

**Pg3Ln25: "Deviations from Gaussian distribution are..."**; assuming distribution of temperature? Clarify.

**RESPONSE:** This refers to turbulence variables in general (e.g. wind components, temperature, gases), since their distribution typically deviates from Gaussian in the presence of large scale coherent patterns. See e.g. profiles for third-order statistics in canopy flows in LES study by (Patton et al., 2015).

**Pg5Ln19-23: "When comparing the results..."** Not sure these two sentences are needed; there isn't that strong of a comparison back to the Thomas et al., 2012 paper within the manuscript. Though this work is based off the Thomas et al., work, unless a direct comparison within this paper is going to be made, the differences in the instrument variant is does not need to be explicitly stated.

**CHANGES TO THE MANUSCRIPT:** This part was removed from the manuscript in response to this comment.

**Pg6Ln16-19: "After determining the differential attenuation..."** I suggest splitting this into two sentences at the semicolon instead of keeping it as one longer, complex sentence since it is detailing two distinct processing steps.

**CHANGES TO THE MANUSCRIPT:** Text was modified as the referee suggests.

**Pg6Ln23: “After quality filtering...” Out of how many potential 30-minute periods, either as a percent or total measurement periods?**

RESPONSE: 89 % of the total 30-min periods were available for analysis after quality filtering. We noticed that there was a typo in the amount of data available for analysis. It was 1513, not 1353 as previously mentioned in the manuscript.

CHANGES TO THE MANUSCRIPT: Modified the sentence as “After quality filtering, 1513 (89 % of the whole period) 30-min periods of DTS data were available for further analysis.”

**Figure 3: Put the height of the sonic anemometer within the figure caption; easy to miss being the title of the graph.**

RESPONSE: Thanks, will be added.

CHANGES TO THE MANUSCRIPT: Changed the first sentence of Fig. 3 caption as “Ensemble-averaged, frequency weighted and normalised temperature power spectrum estimated from the DTS data and colocated 3D sonic anemometer (reference) at 25 m height above ground ( $1.5h_c$ , where  $h_c$  is canopy height).”

**Pg12Ln16: “The temperature variance was dominated...” This feels out of place; there isn’t any direct support for this comment near this sentence and think it should be later in the paragraph/section after the discussion of Figure 5/Table 2 or in the previous section with the discussion of the power spectra comparisons. Also strikes me as being a statement that applies to above the canopy and not as much within the canopy due to the general size of eddies closer to the ground and surrounded by obstacles (trees).**

RESPONSE: OK, we will remove this sentence and modify the next sentence so that it discusses only the power spectra and not the large coherent eddies.

CHANGES TO THE MANUSCRIPT: Removed the sentence and modified the next sentence as “Bulk of the time series variance was related to fluctuations close to the peak of the power spectra and DTS system was able to resolve the variability at these frequencies (Fig. 3)”

**Pg14Ln5: “size eddies dominating...” should be “size of eddies dominating...”**

CHANGES TO THE MANUSCRIPT: Thanks, modified as suggested.

**Pg15Ln7: Remove the “but” from “The mean potential temperature gradients from DTS bug exhibited...”.**

CHANGES TO THE MANUSCRIPT: Thanks, removed.

**Pg16Ln4: “For comparison, the unbiased median temperature gradients...”, Gradient between which heights; across canopy?**

RESPONSE: Between the heights mentioned in the previous sentence (27 m and 5.8 m).

CHANGES TO THE MANUSCRIPT: Modified the sentence as “For comparison, the unbiased median temperature gradients during these periods calculated from the reference measurements at heights mentioned above were 0.34 K and -0.29 K at night and daytime, respectively.”.

**Pg16Ln15: Remove “already” in “hence a smaller...” sentence.**

CHANGES TO THE MANUSCRIPT: Removed

**Pg16Ln33: The word “almost is misspelled.**

CHANGES TO THE MANUSCRIPT: Thanks, fixed.

**Pg17Ln6-8: “The measured skewness profiles...” There is little to no context for this sentence as it requires the reader to look up these papers or be very familiar with them to understand the similarities in the skewness profiles. Clarify or add context to the sentence or remove.**

CHANGES TO THE MANUSCRIPT: OK, removed this sentence.

**Pg18Ln8-9: It might be better to mention of the caveat using Taylor’s frozen turbulence hypothesis on this analysis at the beginning of the section instead of in the middle of the paragraph, or break the paragraph in half to be clear which pieces of the analyses are relying on Taylor’s hypothesis.**

RESPONSE: Thanks, good suggestion.

CHANGES TO THE MANUSCRIPT: Replaced “eddy size” with “turbulence time scale” on p18l2. Split the paragraph into two starting from p18l4 and added in the beginning of the new paragraph the following sentence: “By relying on Taylor’s frozen turbulence hypothesis, two dimensional spatial details of the coherent motions can be delineated from the vertical DTS measurements.”. Removed the sentence about Taylor’s hypothesis on p18l6...p18l8.

**Pg18Ln32: “Flow patterns...” This sentence is not saying anything new that the next sentence is not already saying and is not a very useful sentence. Maybe combine the two.**

CHANGES TO THE MANUSCRIPT: OK, removed this sentence.

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