Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2020-260-RC2, 2020

Atmospheric Measurement Techniques
Discussions

Interactive comment on "Suitability of fiber-optic distributed temperature sensing to reveal mixing processes and higher-order moments at the forest-air interface" *by* Olli Peltola et al.

Anonymous Referee #2

Received and published: 18 October 2020

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:

Abstract, L6: replace "quantified" with "assessed".

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

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.

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.

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?

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. . .. . .".

Fig8: the arrows are very hard to identify.

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.

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?

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2020-260, 2020.