Review report: Wind speed measurements using distributed fiber optics: a wind tunnel study

Author of the paper: van Ramshorst et al.
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General Comments

The study of van Ramshorst et al. investigated the actively heated fiber-optic (AHFO) technique and estimated its accuracy and precision under controlled airflow conditions by comparing to a three-dimensional ultrasonic anemometer. A valuable error prediction equation for the wind speed measurements at different heating rates was developed, as the heating rate can be a limiting factor for long cables. This equation is also accounting for averaging over space or time which further increases precision. They conclude that AHFO measurements are reliable in outdoor deployments when correcting the measurements for directional sensitivity with a ultrasonic anemometer, choosing the right heating rate and spatial or temporal averaging.
Distributed temperature sensing (DTS) measures temperatures along a fiber-optic cable spatially continuously and can be used in various fields. Especially for atmospheric research this technique offers new insight into the temperature field and thus was implemented in many studies. By using the AHFO technique, wind speed measurements can be added to the system. As the community using the DTS and AHFO technique is growing, the study of van Ramshorst et al. is important for users to be aware of the accuracy, precision and limitation of this technique. The paper is very valuable for our community and I would like to see the manuscript being published.
After a view rounds of review is still feel that a view issues are not addressed: 1) statements which needs further context for the reader & 2) Checking all equations for consistency and correctness.
I recommend to have another person check the manuscript and accept the submitted manuscript after major revisions.

Detailed comments

- p1 l9: a high correlation coefficient is presented. However, this correlation is based on correcting the wind speed measurements by the angle of attack. Without knowing the angle of attack the wind speed measurements by FODS perform by far not as good. I think this is a crucial point, especially in the varying wind field near the surface/within canopies/within the whole boundary layer. Depending on the setup, it is very hard to have enough reference devices to know the attack angle and then correct for it. Accordingly, I think the statement in p1 l9 should at least be reformulated and the reader pointed to that a correction for the attack angle was applied.

- p18 l12-14 two publications are mentioned giving an alternative to having multiple ultrasonic anemometer station along the fiber-optic setup. But to my knowledge Zeeman et al. 2015 only provides feature tracking which does not necessarily give the wind direction within the corresponding air masses (which is also stated in the publication under Section 3.1.2). While the outcome of the publication of Lapo et al 2020 is that FODS might be used to determine wind direction at some point, but field studies have to prove that and what features can actually be resolved by it. In this stage I would not present it as done by the authors.

- p3l9-11 the authors say that sensible heat flux can be estimated, however, there is no existing study proving that. Naming this and also the already mentioned publications is not incorrect, but I think they should be put in a better context.

- The mathematical correctness of Eq. 15-18 and how they are developed needs to be reviewed. I do not know the use of an intermediate constant, but maybe this is a mathematical derivation I am not aware of. As the authors show, the numbers do estimate σ_p in a fairly good way, but the mathematical presentation of the derivation of the intermediate constants seems fuzzy to me. I would like another person to have a look on this.

- Equation 14 is introduced later than Equation 12 and 13, even though Equation 14 is used to determine the parameters derived in Equation 12 and 13. It would be more reader friendly to introduce Equation 14 together with Equation 11.

- Eq.21: As σ_p is derived by using the corrected wind speeds u_{DTS}, I think Eq. 21 is incorrect: u_{DTS} is used to derive σ_p, however, Eq. 19-20 use u_N and then insert this into Eq.21. As stated in Equation 11 and 14 $u_N! = u_{DTS}$ and thus the derivation of Eq. 21 from Eq.20 is not correct. Even if the difference between u_N and u_{DTS} is only a factor, this needs to be mentioned and discussed in the text. Also, as σ_p is derived for u_{DTS} it is not justified in my opinion to say that the prediction function is then still true for perpendicular flow as the derivation is mostly based on corrected data.

- small editing comment: I think the definition of n_{time} and n_{space} was dropped in the most recent manuscript, but should be added. I am sorry if I over read the definition of those parameter.

Detailed comments on manuscript after revision 4

The following comments were not addressed

- p3 l8-9: as already mentioned above: how can you derrive the sensible heat flux from DTS + AHFO measurements
  ⇒ even though this might be true, until there is no study I think it is a vague statement and should be reformulated or put in better context.

- p8 l6: duplexed FO core: was this splice checked for a step loss? ; p8 l11-12: so only offset correction of the FO cable was performed? Was the differential attenuation of the FO cores checked and accounted for?; p9 l1-2: "However, in processing of the raw DTS data...." → "But in our setup the signal loss of the splice connecting the fiber-optic cores of our cable at the end of the array was not the same in both directions." - Did you introduce earlier that two cores were spliced together to create a duplexed setup?; p9 l2-3:

"Due to this assymetrical structure..." → I think it was never introduced that potentially two channels can be used for this setup. Please be either more detailed about your setup (describe and add fiber-optic cores of the cable being connected to the DTS machine in text and Fig.2) or never mention this option. Otherwise it confuses the reader.
⇒ I think this still needs clarification and how the calibration was done. Single-ended, single-ended duplexed or double ended calibration? Hausner et al 2011 presents those three options. Maybe one paragraph specifically addressing calibration is beneficial instead of single sentences hinting to the calibration setup.

- p10 Eq13: isn't it $\sum_{i=1}^{n}$ and the fraction $\frac{1}{n}$ or $\frac{1}{n-1}$? Further, σ_p is defined here by u_{DTS}, but later in Eq.20 u_N is inserted instead of u_{DTS}.
⇒ The authors responded that it is correct to as the only difference between u_{DTS} and u_N is a factor, however, I think this does justify inserting u_N in Eq.20 instead of u_{DTS}. This clearly needs to be mentioned in the text and discussed (as also mentioned above).

- In the abstract coefficients of determination are given: please also specify in the abstract on which setting those are derived or pick the best one and describe it fully. Otherwise those are just high numbers.
⇒ this is still not adjusted
The coefficient of determination is high, but the intercept as well as the slope shows that there is a systematic underestimation (slope less than one). Why are the intercepts negative? Are they ranging from -0.7 to -0.6(ms^{-1}, I guess) or from -0.7 to 0.6? This needs to be discussed.
⇒ as the coefficients are mentioned in the abstract I think the manuscript needs some discussion of the results in addition to the plots in the appendix.

- p11 l10-11: you mention that σ_a also depends on n_{space} but this is not shown in your manuscript. Only plot showing different temporal averaging is shown. It needs at least to be mentioned that this was tested but it is not shown.
⇒ I do not think it is wrong that spatial averaging will influence $_a$, however, it is not shown. In my opinion it should be tested and then at least mentioned in the text. In Figure 4 the change of σ_a is shown for increasing n_{time} increasing the total n while n_{space} is kept constant. The difference between attack angles can not be used to show that spatial averaging does have an impact on σ_a. This should at least be mentioned in the text that similar behaviour is expected when increasing n_{space} while n_{time} is kept constant.

- p15 l16: it is not shown or further mentioned that σ_p also depends on n_{space}. Please provide corresponding graphs or describe in a view sentences if this was tested but is not shown.
⇒ same comment as above. I think it is only shown that σ_p changes with temporal averaging while spatial averaging is kept constant.

- p13 Eq.15 & 16: Those equations seem weird to me as a dependency does not develop with the introduction of other variables in an equation:
⇒ also see my comments in the first section.

- p17 l23-26: I think it might be valuable to use a sonic anemometer to determine the attack angles. But depending on the wind field which can be very variable within canopies, within undulating terrain, even within a few meters. Directional sensitivity compensation can only be applied if the angle of attack is known demanding ancillary measurement

devices.
⇒ see comment above. It is not easy to correct for attack angles and to have enough reference stations which should be mentioned accordingly for future users.