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

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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.
The manuscript again improved substantially. Especially Figure 8 was the needed piece for validating the proposed prediction function. I have one major point: T_{error} is an important value for the error prediction function, but is barely described. The number is given by the manufacturer but it is not specified if a calibration and which spatial and temporal resolution was applied. Accordingly, T_{error} is not a universal number and can only be used if the setup of the manufacturer is known. I recommend to either ask the manufacturer or derive T_{error} by the bias within the calibration bath which is a temperature controlled environment. I think that the error prediction function is only correct if T_{error} is defined correctly. Further, I have three minor points: 1) the dependency between σ_p and n_{space} is not shown → I would recommend to add a second x-axis on Figure 4, Figure 5 and Figure 6 showing $n = n_{time} * n_{space}$; 2) Checking all equations for consistency and correctness; 3) Careful language: differentiate between what the paper can offer and what are potential future steps.
I am not a native speaker so I am not sure about the commas and other grammatical errors of the manuscript. I do not want to discourage the author by the amount of comments and number of reviews I demand. The manuscript is really improving. I recommend to accept the submitted manuscript after major revisions. I think that not another major revision is needed afterwards. More detailed comments are given below.

Detailed comments

- p1 l11-13: AHFO can measure the mean horizontal wind speed but not the horizontal wind speed. As the horizontal components are usual an order of magnitude stronger, it is so far not possible to measure the vertical wind speed component. At least I did not see a publication doing this so far. I also did not see this being done in your publication. So promising that AHFO combined with DTS can derive a turbulent heat flux estimation is promising more than can be done so far especially for reader which are new to this measurement technique.
- p2 l10: the "as well" can be taken out.
- p2 l11: spatial**ly**
- p2 l11-13: The underlying assumptions of what?
- p2 l21: I would remove "Recently"
- p2 l21-29: I like this paragraph very much.
- p3 l6: spatial**ly**
- p3 l8-9: as already mentioned above: how can you derrive the sensible heat flux from DTS + AHFO measurements
- p3 l15: the abbrevation DTS was already introduced earlier
- p3 l15-19: just a thought (does not need to be included): Isn't bending also a source of signal loss for the fiber? Further, when averaging over space is needed coiling up the fiber could potentially be useful. Hilgersom et al. 2016 "Practical considerations for enhanced-resolution coil-wrapped distributed temperature sensing". But maybe this is opening a new topic and is a bit too far off topic of this manuscript.
- p3 l23: T_{error}: machine specifications: How did the manufacturer define this? If this is not clear you can not use this number for your calculations! I think it is better to define T_{error} by the mean bias within one of the calibration baths using the proposed averaging over time or space which might be more fair than a constant number independent of spatial and temporal averaging of your measurements. If $T_s - T_f$ is lower than the bias within the calibration baths then this can also be considered a low signal or high σ_p. If T_{error} is defined this way, maybe the instantaneous constants are not necessary and your function could be applied to any setup.
- p3 l26-27: review this sentence. It contains "can" twice and "can cause that other ways of..." also sounds not logical to me.
- p3 l26: "radiation" $\rightarrow$ "radiative heat loss,"
- p3 l30: the measurement error can only be compensated when using another device. Please mention this. With a varying wind field or within a canopy a lot of reference devices might be necessary.
- p4 Figure 1: This is the figure of Sayde et al. (2015)! "...balance, based on..." $\rightarrow$ "... balance from..."

- p5 l9-10: The Nusselt number is defined incorrect: ratio of convective to conductive heat transfer.
- p5 l20: the unit of the temperature range is incorrect. I think it is degree Celcius, not Kelvin.
- p6 l1: I would start a new paragraph here
- p6 l9: "...we assume that there is a uniform..." → uniform in space or in time? In time could be verified by the ultrasonic anemometer data
- p7 l7: Is there a documentation of that windtunnel somewhere? Or a webpage to get further information?
- p7 l9-10: "... two segments of one cable (which encloses the FO cores) were placed 8cm apart..." → this formulated a bit confusing. "During the experiment the heated and the unheated reference cable segment were placed 8cm apart. The FO cable has two FO cores, hence, each cable segment could be sampled twice."
- 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?
- p8 l18-31: I like this presentation a lot, however, I think the bullet points could still be shortened by not using full sentences making the amount of experiments even more clear.
- p8 l27: Outside deployment: definitely turbulent conditions → please mention one sentence why this is not an issue comparing the wind tunnel experiments with outside deployments.
- p8 l33: "Splices between ends of fiber optic cables..." → "Splices connecting two fiber-optic cores..."
- 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.
- p9 l4: Sorry if I missed it, but did you explain what "duplexed FO core" means?
- p9 l4-11: I would suggest in this paragraph to describe your setup by differentiating about the used number of FO cores increasing the measurement signal instead of writing "x2" or "x1". Further, in this paragraph the effect of bending the fiber is mentioned. So again Hilgersom et al. 2016 could be worth being mentioned instead of just stating that bends can cause signal loss. Besides, if a bend caused the signal loss why is only one FO core affected? Or are both affected and did combining them made the measurments worse?

- p9 l17-19: I would mention again that n_{space}=5 for the 90deg angle.
- p9 l14-17: I would not introduce the error prediction function in this paragraph. This will make it easier for the reader to follow your manuscript. How can C_{DTS} be independent of DTS machine and settings? I highly doubt that as also explained earlier (p3 l23).
- p9 l23-24: "However, in reality the wind will not always have a 90deg angle..." → I agree that for a horizontal setup this is true but in a vertical harp the horizontal wind will have an attack angle of 90deg! It depends on the physical setup of the fiber.
- 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}. Is this an inconsistency?
- p111 l1-2: Is that the only discussion you provide for the different attack angles? I think that the results of the use of the direction sensitivity formula needs to be described and discussed.
- p11 l3-6:
  "...is not yet fully calibrated..." → "...is not yet applied to the 30-s averages..."
  a range is given for the coefficients of determinations, slope, and intercept: what are the ranges for? Attack angles? Averaging? What is the averaging? What is n?
  Units are missing for the intercept
  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. Do not be overpromising.
  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.
  "Finally, as expected, ...": this sentence seems out of place. If it is connected to Figure B2 I suggest to transfer the sentence to line 3. I would also change "wind speed angle" to "attack angle" as defined earlier in the manuscript.
- p11 l9: σ_a is a dependent on the averaging time,..." → "As can be seen in Figure 4, σ_a depends on the spatial and temporal averaging of the FO data. The averaging time n_{time} is defined as..."
- 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.
- p11 l14: I think σ_a is so low because the data is averaged over $n = 10$ spatial points. Accordingly, when averaging over 30s this is already averaging over $n = 300$. Please at least mention again that $n = 10$. Otherwise it is again overpromising. Is the directional sensitivity adjusted for σ_a? This would explain the negative σ_a as Fig.3b shows that for high wind speeds an overestimation of u_{DTS} can be seen when applying the directional sensitivity.
- p12 Figure4: I recommend adding the second axis showing n

- p12 l4-8: The paragraph needs substantial revision. Especially the first two sentences I do not understand. What a natural variability? what was considered about it? Does it have influence on σ_p? Or is the inconsistency in the wind speed of the wind tunnel small enough so it does not matter for σ_p? The paragraph also further describes Eq. 13 so I suggest to move it to the same section

- p12 l12-15: This sentence seems out of place and the variables are already defined somewhere else. I would delete it.

- p13 Figure5: I would also add n as a second axis.

- 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:
  As can be seen in Figure 5, σ_p is a function of ΔT and n, so $\sigma_p(\Delta T, n)$. So the question is if σ_p can also be derived by the signal-to-noise ratio $\left(\frac{\Delta T}{T_{error}}\right)$ and the spatial and temporal averaging n:
  $\sigma_p(\Delta T, n) = \frac{\Delta T}{T_{error}} \frac{1}{\sqrt{n}}$
  As ΔT depends on the heating rate P_s as well as on the mean wind speed the fiber "sees" u_N, ΔT can be reformulated:
  $\Delta T = \frac{AP_s}{Bu_N^m}$
  $\Rightarrow \sigma_p(\Delta T, n) = \frac{BT_{error}u_N^m}{AP_s\sqrt{n}}$
  I suggest to use T_{error} computed by the mean bias in the calibration bath and insert that into the above mentioned equation (without the use of C_{DTS}) and compare that to your suggested solution. I think T_{error} is in the range of 0.7K to 0.9K which is a bit lower than $C_{DTS} * T_{error} = 1.25K$ of your manuscript, but as also seen in your manuscript those numbers are overestimating σ_p (Figure 8). I think this is the most physical way to describe and derive σ_p.

- p15 Eq20 & 21: $u_n^m \rightarrow u_N^m$

- p15 l16: it is not shown or further mentionend 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.

- p15 Section "Verification of the precision prediction": This section needs to be explained and elaborated further. Why is twice the standard deviation used? Not just once? Why not using 90%- and 10%-percentiles? Is the 98%-percentile the same as twice the standard deviation? Figure 8 has many lines in it. Maybe using shading instead of several lines could be considered. To me it looks like σ_p is overestimated by the prediction function. The points may lay in the 98% bound, but on the lower end. So I would doubt the general applicability.

- p16 Figure9: Where is this Figure described and discussed?

- 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. Please add. So I do not fully agree that wind speed measurements of horizontally put FO cables can always be fully corrected. I would rather recommend to string the FO cables vertically, hence, no correction is needed as the attack angle is always perpendicular.